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SCIENCE IN THE CONTEXT OF APPLICATION

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SCIENCE IN THE CONTEXT OF APPLICATION

Edited by

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Science in the Context of Application: Methodological Change, Conceptual Transformation, Cultural Reorientation

Martin Carrier and Alfred Nordmann

Research Going Practical: A Break with the Epistemic Past?

Ever since Francis Bacon societies have looked to science to provide answers to its practical problems, to stimulate the economy, or to inspire useful applications. Arguably, research scientists didn't live up to these expectations until some time in the nineteenth century. They were epitomized in the motto for the 1933 century-of-Progress exhibition in Chicago: "Science finds, industry applies, man conforms." Only in the decades to come, however, arose an awareness of rather more complicated relations in the context of scientific and technological applications. We increasingly view the world around us as a product of science and technology. Accordingly, we have begun to appreciate that science does not take its problems only from nature and then produces technological applications, but that the very problems of scientific research themselves are generated by science and technology. Simultaneously, problems like global warming, the toxicology of nanoparticles, or the use of renewable energies are constituted by many factors that interact with great complexity. Science in the context of application is challenged to gain new understanding and control of such complexity – it cannot seek shelter in the ivory tower or simply pursue its internal quest for understanding and gradual improvement of grand theories.

Science's increasing dependency on technical apparatus, its technological ambitions to manage the complexities of highly developed societies, and the heavy application pressure under which it operates have prompted a flurry of analyses which converge on the claim that the scientific enterprise as such has undergone a profound methodological and institutional transformation during the past decades. Science is viewed today as an essentially practical endeavor; it appears inextricably interwoven with technology and heavily intertwined with the economy, politics, the media and other realms of society.

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This markedly practical orientation of research is claimed to have a significant impact on the institutional and methodological characteristics of science. In terms of the goals that are pursued, university research in the sciences tends to increasingly resemble research in industrial laboratories. Both public and private institutions define research problems chiefly in terms of practical projects. Scientific knowledge is produced in the context of application; and application-oriented research is in no way tantamount to the transfer of more basic knowledge to practical challenges. This attitude contributes to changes in the institutional system of science. Universities found companies in order to market products based on their research. Companies buy themselves into universities or conclude large-scale contracts concerning joint projects. With respect to methodology, the emphasis on intervention is sometimes claimed to push theoretical representation into the background. Shaping the world, rather than understanding it, appears to be the chief objective of contemporary science.

In Europe, the most widely used designation of this change is "mode 2 research" (Gibbons et al., 1994; Nowotny et al., 2001) which refers to a sort of phase transition in the epistemic and social order of science. The contention is that science has deviated significantly from the traditional or "mode 1" academic form of knowledge production and has entered a new mode-2 regime. Mode 2 is characterized by features like "primacy of the context of application" (which means that the distinction between theoretical understanding and its practical application is replaced by the production of knowledge in practical contexts), "transdisciplinarity" (according to which the research agenda is not set by organic disciplinary development but rather dominated by practical challenges), or changes in quality control procedures (which involve a dominance of social, economic, or political demands that tend to overshadow traditional epistemic commitments like theoretical explanation or causal penetration). The thesis is that science has moved out of the mode-1 laboratory with its idealizations and artificial conditions and entered the social arena where demands are tough and predictions risky.

In the US, the most popular designations are "post-academic" and "entrepreneurial" science along with the notion of the "triple helix." Other pertinent labels are "technoscience" or "post-normal science." Here too, these labels are supposed to capture the widely shared view that science presently undergoes a profound transformation which affects the assessment procedures in science, the connection between science and technology, and the relationship between science and society.

Changing Conditions of Scientific Research

The contributions to the volume attempt to identify, explore and assess these changes. It is undeniable that science is intensely involved in technological progress, economic growth, emerging risks and risk perception, and even public culture. Yet it is in no way clear how such entanglements affect the scientific enterprise. For instance, the credibility of science itself could be among its casualties. To the extent that science is intertwined with social issues or becomes part of political powerplay, the scientific claims to objectivity and trustworthiness tend to be attenuated. The loss of objectivity in the double sense of adequacy to the facts and interpersonal neutrality is striking and obvious in the field of expert testimony where scientists are frequently accused of pursuing vested interests and where the expert is confronted with the contrary judgment and the divergent advice of the counter-expert.

The marked emphasis on usability and utility might also affect scientific research practice significantly. A feature assumed to be characteristic of basic or epistemically oriented research is that it seeks to widen the understanding of the phenomena of a field (Stokes, 1997, 7). By contrast, application-oriented science is characterized, in general, by its pragmatic attitude and by its commitment to the proper functioning of some device as its chief criterion of success. The hallmark of applied research is the search for the control of natural phenomena; intervention, not understanding, is at the focus (Polanyi, 1962, 182–183). In addition, the pronounced pragmatism of application-dominated research translates into a restricted scope of theorizing and explaining so that applied-science researchers can be expected to resort to tentative epistemic strategies which feature, for instance, local solutions without theoretical integration and tend to cut off research from any deeper epistemic aspirations.

However, a contrary intuition is rooted in the view that superficial knowledge will eventually fail to support technological progress. Conversely speaking, the theoretical integration and the causal explanation of an empirical regularity improve the prospect of bringing other factors to bear on the particular process and to twist the latter so that it delivers what is demanded more efficiently or more reliably. The same goes for the risks associated with science. In applied fields of inquiry, error may spawn grave non-epistemic consequences and may cause harm beyond the walls of laboratories and libraries. Non-epistemic risks such as potential damage of health, lives, or property need to be incorporated in the assessment of a hypothesis. Technological intervention which is at once reliable and safe is achieved best by relying on knowledge that has passed tough standards of quality control (Carrier, 2004, 4–6).

These considerations outline a field of contrasting intuitions as to the methodological features of applied research which will be explored more thoroughly in the book. They take account also of the various ways for science and technology to interact in the context of application. Only the first of the following three fits the traditional picture of basic and applied science, and especially the third may reflect a rather more troubling and contemporary situation:

- Scientific research creates new technical capabilities which are then developed in engineering contexts (with more or less prominent feedback-processes – Heinrich Hertz and electromagnetic signal transmission).
- Technological innovation gets ahead of scientific understanding and prompts research activity to attain comprehensive understanding of its basic principles (be it to better manage the technology or to gain fundamental insight from technologically produced phenomena – thermodynamics and the steam engine, plasma physics and the fusion reactor).

- Piece-meal research activities manage complexity of socio-technical systems with no expectation of comprehensive understanding (nanotoxicology, foresight knowledge, commissioned explorative research to support specific public decisions, etc.).

A fourth type of interaction between science and technology comes from the knowledge-intensive and frequently opaque technical instruments that enable cutting-edge basic and applied research. Computer simulations, in particular, have given rise to the contentious issue how the use of numerical simulations is related to theoretical derivation and experimental exploration (Humphreys, 2004; Lenhard, 2006, 2007; Winsberg, 2003).

The role of instruments and technological intervention is stressed by "technoscientific" accounts which suggest a qualitative change in the constitution of research objects. The breakdown of the classical division between representing (the interest and goal of science) and intervening (the purpose of engineering) is supposed to be reflected in the hybridization of nature and culture. The traditional project of probing into nature is assumed to have come to a close, and its supposed end is associated with the end of nature – traditionally conceived. It is true that classical experimental science employed technical laboratory equipment to prepare its phenomena but it was still possible to think of these phenomena as "nature" presenting itself under special conditions. On the technoscientific account, it is no longer possible even to construe objects like the hole in the ozone-layer or the cancer-mouse as natural. They have been created by humans but they constitute objects of scientific research all the same (Nordmann, 2006).

As for institutional conditions, application-oriented research is to a large extent conducted in industrial companies, and it is important to understand the institutional features of commercialized research or "science in the private interest." For instance, one relevant constraint is the commitment to secrecy which restricts part of industrial research. Research outcome is produced but kept behind closed doors, a feature which is apt to hide possibly dangerous side-effects and also tends to reduce the level of scrutiny to which the results are put (Concar, 2002, 15; Gibson et al., 2002). By contrast, a number of companies recognize that laboratories operating behind a veil of sequestration are cut off from the benefits of cooperation (Wray, 2002, 155–158). Consequently, attempts to suppress the circulation of results usually backfire and often damage those responsible for trying. In fact, a large number of published articles originate from private research sites (Carrier, 2008). Institutional characteristics of research in the context of application will be addressed respectively from a sociological and a methodological perspective.

Science, Values, and Society

A related question of great relevance concerns the impact of science on society. What kind of challenge does science, as it is practiced and understood today, pose to society? Here, the selection of research topics is of critical importance. In commercialized research, problems are imposed from outside of science according to their practical relevance. One effect is that research tends to comply with customer desires while only goods that can be sold profitably are worth any research effort. A disturbing trend in medical research, for instance, is that neither ailments of the poor countries nor exercise programs rank highly on the research agenda.

Another highly relevant question concerns the interrelations between science and social values or between freedom of research and accountability. Social values that are taken to be relevant to science mostly fall into two broad categories which have to do with participation in and with impact of knowledge. Participation concerns the inclusion of individuals or social groups in the production of knowledge. Social evaluation of the impact of knowledge becomes manifest in a demand-oriented and in a precautionary variant. First, this sort of knowledge that increases the welfare of a social group or promotes the common good will be highly appreciated. Second, the knowledge to be gained is required not to harm the well-being of a social group or have other similarly negative influences (such as damaging the environment).

Restrictions on research violate the freedom of research which is widely accepted as a vital intellectual value and a precondition of creativity. Thus, a tension emerges between the demand for the social accountability of science and the commitment to freedom of research. Kitcher's conception of an idealized institutional procedure for setting the research agenda democratically puts tight constraints on the ability to conduct research freely (Kitcher, 2004, 51). Alternative approaches suggest anchoring the responsibility of science in the ethics-based duty of individual scientists to accept responsibility for the foreseeable consequences of their work. Scientists are called upon to take the wider impact of their research into account and to refrain from the pursuit of research projects which might interfere with the common good (Koertge, 2000, 48–49, 2003, 224).

Clearly, application-orientation strongly increases the social relevance of science. The success of science in dealing with the details of complex processes and the phenomena of the life-world makes science a distinctive feature of shaping nature and society. The social relevance of science generates the demand for social responsibility, and the question is how this responsibility is and ought to be instituted. All approaches toward this end agree in that they stress the importance of a dialog between science and society – though they envision differently the precise form of their interconnectedness.

The culture of science is generated by a system of epistemic interests and social norms which reinforce each other and maintain methodological standards for the production and assessment of objective knowledge, on the one hand, and ethics-based demands and values that govern the behavior of scientists, on the other. Examples of the latter are Robert Merton's cultural values of science which reinforce Karl Popper's methodology and include impersonal confirmation procedures and organized skepticism (Merton, 1942). The culture of science thus encompasses the goals science is committed or expected to pursue and the means for achieving these goals accepted as legitimate or reliable in science or society. The prominence of application-oriented research may engender changes in the practice of science or changes in the "ethos" of science which amount to cultural changes in the scientific community and may produce alterations in the self-understanding of science and the image of science prevalent in society.

Exploring Science in the Context of Application

It is an important challenge to explore how science and culture are affected by these influences and developments. As a first step, this collection of papers looks into contemporary research practices. It compares application-dominated and epistemic or truth-oriented research with regard to the role they play in fundamental theories, confirmation procedures, models, simulations, and experiments, respectively. Questions pursued here include: Do application-dominated research projects exhibit novel, characteristic methodological features? Can the new research practices be described with the traditional methodological vocabulary and do the research results measure up to received standards of trustworthiness? What are the changing interpretations of the relationship between science and technology? Is it true that the distinction between representation and intervention becomes blurred with respect to the object of research and the objective of research?

The second step concerns the interrelations between science and society and, in particular, the impact of science on society. Here some of the posed questions include: Is it justified to fear that a science subject to strong social, political and economic pressure loses its critical role of "speaking truth to power"? Can it be expected that science increasingly becomes participatory and enters a state of a social science of nature? Are we facing a tendency toward the privatization of knowledge with an emphasis on intellectual property rights or does the Mertonian norm of "communism" – or "communalism" – persist, according to which scientific knowledge is and remains in public possession (Merton, 1942)?

In the third step, the questions outlined at the beginning are taken up: Do the observed changes amount to a cultural transformation? Is it warranted to interpret these changes as a major, epoch-making realignment of science and technology in society? The understanding of science within society and the self-understanding of science are at issue here. What are the perceived relationships between science and society, science and nature, and science and technology?

Thus, the three central questions are: Does science proceed differently, and if so, how? Does science affect society differently, and if so, how? Is science conceived differently, and if so, how?

This challenge is genuinely interdisciplinary and is addressed from a variety of disciplinary sources, in particular, philosophy, sociology, history, and cultural studies. Philosophy of science is invoked for analyzing the epistemic procedures used in different social configurations and for illuminating the general cultural impact on science. Sociology of science is indispensible for charting the intricate territory of diverse institutional settings as they emerge in present-day society and for capturing the various social forces that act on science. The history of science is of crucial importance for dealing with the question whether the philosophical and sociological characteristics that were identified before are really novel or turn out to be familiar features, perhaps in a different guise. Cultural studies illuminates the role of visions and imagery in the development of science, including the changing relations of science and science fiction. By joining forces, these disciplinary approaches can determine how deeply the changing interests and the public image of science permeate laboratory and publication practices. In bringing these sources together, a novel and exciting picture of science in the context of application emerges.

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Part I
Changing Conditions of Scientific Research: Science and Technology

Knowledge, Politics, and Commerce: Science Under the Pressure of Practice

Martin Carrier

Research in Extra-Scientific Interest

Robert Merton included "communalism" (or "communism," as he originally called it) in the list of "cultural values" or "institutional imperatives" he insisted to be constitutive of science. "Communalism" was intended to express the requirement that knowledge should be considered a common good rather than private property and that no rights to its exclusive use should be granted. Merton sought to affirm the primacy of the epistemic commitment of science and considered the politicization and commercialization as an obstacle to reaching this goal. "Communalism" was intended to prohibit, in particular, the commercialization of science.[1]

In the past half-century, in contrast to Merton's admonition, communalist obligations of science were increasingly abandoned. Economic companies take science into their service in the search for new technological products. As a result, industrial research or industry-sponsored university research has been of growing importance to science in the past decades. The Bayh-Dole act of 1980 (which granted US universities intellectual property rights on their innovations based on public financial support) and the growing dependence of European universities on external funding and industrial sponsoring have transformed the research in public laboratories as well. The race for patents pervades large chunks of research in the natural sciences. Other practical challenges exert their pressure on science as well. Politics demands advice and short-term solutions to all sorts of concrete problems, ranging from the adjustment of social security systems to global warming.

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[1] It is under debate whether Merton's "ethos of science" was meant in a descriptive or a normative sense. However, Merton was well aware that two of his four values were violated by the science of his period. "Universalism" (the irrelevance of personal traits of scientists for the evaluation of their findings) was not respected by Nazi science (which was among the motives for working out or reaffirming the ethos) and communalism was infringed by the capitalist economy (Merton, 1942, 270–271, 275). It follows that a purely descriptive reading is ruled out.

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Two sorts of worries are frequently associated with the politicization and, even more pronouncedly, with the commercialization of science. These worries concern, first, the nature of the research agenda and the ways of establishing it, and, second, the test and confirmation procedures in science. In both respects, moral as well as epistemological misgivings are articulated. Modes of problem selection are criticized on moral grounds by claiming that questions of short-term benefit are emphasized in politicized and commercialized research, while issues that are essential for large parts of humankind are neglected. With regard to methodological considerations, a relevant argument is that if research questions are imposed on science from outside, the problem-solving capacities of science are likely to be overtaxed and that science is pushed into a realm of uncertainty it would never have entered if left to its own devices. Another worry is that commercialized research is confined to short-term goals and fails to embark on visionary projects. Such research is likely to be pedestrian and unimaginative. This means that, methodologically, science is claimed to fall short of the standards of reliability, depth and innovativeness that are used to characterize research (see section "Problem Selection in Fundamental and Application-Driven Research"). If science is left at the mercy of politicians and corporate leaders, its commitment to truth is feared to be traded for its capacity of intervention.

I distinguish between "epistemic research," on the one hand, and "application-oriented research" or "application-driven research," on the other, as the relevant kinds of research whose features are intended to be clarified. The former category is traditionally conceived as academic fundamental research and is characterized by the search for understanding; the latter is supposed to refer to research endeavors that are driven by the search for utility. It merits emphasis that application-oriented projects include genuine research and are distinct from mere development. Research projects aim to gain new knowledge while development is intended to make a laboratory process or a prototype product suitable to large-scale production (Godin, 2006, 645–646). The crux of application-driven research is conceiving new processes or products, development is concerned with implementing research outcome. Application-oriented research can be distinguished from epistemic research by appeal to the primary purposes they are intended to serve. I will go into this distinction more extensively later; the suggestion is that application-oriented research and epistemic research can each be recognized by drawing on their institutional goals and success criteria.

In what follows I address first the modes of problem selection in science and afterward turn to the methodological issues such as reliability and depth. The first thesis I wish to defend is that in the large majority of cases the kinds of knowledge produced in fundamental and application-oriented research agree in their epistemic characteristics. Science in the context of application is in general not inferior to epistemic research regarding the quality of knowledge. Second, the research agenda of application-driven research is often biased and does pose serious threats to the proposition that science maximizes utility. However, fundamental research fails to be an efficacious cure. Politicization and commercialization are less harmful than it may look at first sight, and the valid core of the objections leveled against these features needs to be addressed by "science in the public interest." All in all, science under the pressure of practice is mostly unobjectionable in epistemic respect whereas important deficiencies and serious side-effects emerge with regard to the research agenda pursued.

Problem Selection in Fundamental and Application-Driven Research

An important characteristic of epistemic research is its knowledge-guided mode of problem selection. The research agenda is set on the basis of previously solved problems and against the background of a theory. For instance, after some issue has been treated successfully by using idealizations or simplifications, the restrictions are gradually relaxed. The development proceeds from the more simple to the more complex, following the guidance of heuristic ideas associated with theories or research programs.

This framework has been elaborated by Thomas Kuhn and Imre Lakatos. Kuhn claims that in normal science the selection of problems is determined by their expected solubility. In normal science, a scientist is permitted "to concentrate his attention upon problems that he has good reason to believe he will be able to solve" (Kuhn, 1962, 164). What scientists set out to do is determined by what they think they can achieve. This entails that scientists are free to address subtle and esoteric phenomena and they are shielded from the demands of society and everyday life. This is different for engineers or medical researchers who need to grapple with questions which are considered urgent by the non-scientific environment or lay people (ibid.). In the same vein, Lakatos assumes that in powerful or "progressive" research programs, the "positive heuristics" determines the choice of problems. The positive heuristics consists of a set of suggestions as to how to improve the theories that make up the research program. It adumbrates a path from the available, avowedly imperfect versions of the program to more elaborate versions in which unrealistic restrictions are dropped. For instance, Newton had conceived his celestial mechanics for a point-mass (representing the earth) in a central force field. Subsequently, he relaxed these constraints by replacing the field with the sun, thus introducing reciprocal actions between the sun and the earth. The next steps were to allow for extended bodies (rather than point-masses) and to assume several planets, thus taking interplanetary forces into account. This process of articulating the basic scheme or "hard core" of a research program is structured in advance and anticipated by the positive heuristic. No look at the data was needed in order to realize that and in which respect the early steps in pursuing the heuristic were deficient and faulty (Lakatos, 1970, 50–51, 60–61).

The accounts of Kuhn and Lakatos agree that problems in mature science are picked by theory-internal considerations and independent of practical concerns or aspirations. Their emphasis on theory needs to be supplemented with considerations regarding the use of devices in experimentation and the employment of measuring instruments. Problem-choice in epistemic research proceeds for the sake of exploring new means of intervention or new registration technology. The scanning tunneling microscope was at first employed in elucidating the molecular structure of semiconductors and subsequently applied to biomolecules. The atomic force microscope, its sophisticated descendant, was utilized in manipulating single atoms. Device-centered problem-choice is guided by considerations of what can be accomplished with a given tool.

In epistemic research, problems are picked by a process of expanding the domain of application of a theory or of an experimental or detection technology. In both variants, problem-selection is knowledge-driven, not demand-driven: treatment deserves what can be resolved, not what needs to be addressed. It is often assumed within this framework that any deviation from the knowledge-driven mode of problem selection will degrade epistemic quality and impede scientific progress. In this vein, Kuhn argues that addressing problems in the way of engineers and medical scientists will slow down the growth of knowledge (Kuhn, 1962, 164).

Accordingly, the suggestion is that a research agenda set up by considerations of practical relevance or social importance tends to diminish the epistemic quality of science. Let me dissect this suggestion into two parts. First, epistemic research is characterized by this knowledge-driven mode of problem selection. The gist of it is that you go wherever your theory or experimental approach leads you to. This is similar to how Vannevar Bush conceived of basic research in his most influential, 1945 "Report to the President"; according to Bush, basic research is "performed without thought of practical ends" (Bush, 1945, Chapter 3). This account places the intentions or motives at the pivotal role of characterizing what epistemic research is. However, the motives of the researchers themselves are often less than relevant; what is more important is the intentions of the pertinent institutions (Rosenberg, 1990, 169–170). The reason is that the institutions determine the criteria of success or failure of a project and thereby affect the choice of research questions.

Second, the exclusion of practical considerations from the process of problem-selection suggests the "understanding of nature" as the goal of epistemic research (Bush, 1945, Chapter 3). Taking account of nature's workings is the objective that typically underlies the fundamental research. Accordingly, utility and understanding are the cognitive aims of application-driven research and fundamental research, respectively (Stokes, 1997, 7–8). To repeat, this is to be understood as institutional aims in whose lights rewards are granted to the pertinent researchers. Institutional aims operate as criteria of success. Nobel prizes are typically awarded for breakthroughs in understanding nature, corporate leaders assess the quality of research efforts in terms of profits reaped. Such institutional goals may become effective as stop-signs for projects. Application-driven projects may be terminated if they do not yield the required results within the envisaged period, while the search for understanding may go on indefinitely.

As explained before, the factual claim traditionally associated with this distinction between epistemic and application-oriented research is that letting the research agenda be determined by considerations of practical relevance induces a reduction in epistemic quality of the knowledge produced. "Epistemic quality" is a notion that can be captured in a variety of ways, and in the present context nothing turns on making this notion more precise. Promising ways of adding more details may proceed via Kuhn's list of epistemic values (Kuhn, 1977), Bayesian hypothesis probability (Carrier, 2008), causal analysis (Salmon, 1984, 135–147, 259–263) or theoretical unification (Kitcher, 1981).

Of course, the claim that the mode of problem selection affects the epistemic quality of the outcome is in need of empirical scrutiny. But here are some considerations and incidental observations in its favor. A practical problem may be solved by bringing to bear a seemingly remote scientific principle or by combining knowledge pieces in a novel way. This means that the theoretical resources apt for clearing up a practical difficulty cannot be established beforehand. Rather, practical success may be made possible by findings that are *prima facie* unrelated to the problem at hand. If the solution to a problem requires forging new links or new insights, starting research from a practical perspective will be less than promising. Yet in many cases it is uncertain in advance whether the necessary knowledge is already available. Therefore, it is advisable to take the opposite direction and to proceed from the system of knowledge to the practical challenges that can be addressed on its basis. Accordingly, broad epistemic research rather than narrowly focused investigations is the royal road to bringing science successfully to bear on practical problems. The mode of problem selection that is assumed to dominate epistemic research is recommended for application-oriented research as well. As a matter of fact, this advice agrees precisely with the policy Vannevar Bush recommended for making science practically fruitful (Bush, 1945, Chapter 2).

President Nixon's "war on cancer" buttresses this recommendation and provides an example of how mission-oriented research can fail. This coordinated research program on fighting cancer in the 1970s was fashioned after the model of the Apollo Program and was planned to defeat cancer by pursuing targeted, application-driven research projects on a large scale. Yet in spite of generous funding, the medical endeavor fell short of palpable success; it largely resulted in a relabeling of projects of fundamental research. This failure is attributed with hindsight to insufficient basic knowledge about the disease (Hohlfeld, 1979, 211–212). Yet incomplete knowledge of the fundamentals does not always thwart coordinated research endeavors. After all, the Apollo program had successfully brought a man to the moon by following precisely this kind of recipe – in spite of serious shortcomings in the relevant knowledge. In addition, when the Human Genome Project was started, the structure of the genome was not understood in depth and the relevant sequencing technologies were poorly developed. Technological revolutions were necessary for a successful completion of this ambitious endeavor, and these revolutions were anticipated and factored in when the project was conceived. This time the bold expectations were met. The puzzling result emerging from these observations is that sometimes science can be pushed into a certain direction by narrowly targeted research but that sometimes such attempts fail completely.

A worry raised with respect to the confirmation procedures in application-oriented research is based on the complexities typically inherent to practical challenges. Science in general needs to wrestle with experience, to be sure, but not with particularly intricate phenomena. On the contrary, empirical tests can often be better performed by focusing on idealized conditions, since the processes considered to be fundamental by the theory at hand show up without distortions. By contrast, the research agenda of application-oriented research is set from outside and thus typically includes problems that exhibit a more intricate interconnection of factors. Applied science cannot circumvent complexity but needs to confront it (Carrier, 2004, 4). This means that practical challenges are not infrequently beyond the reach of scientific understanding; they tend to overtax science and compel it to adopt tentative research strategies. The effect might be a reduction in epistemic quality.

In a similar vein, John Ziman argues that "instrumental science," that is, research intended to produce knowledge with a clear and narrow potential use, suffers in its credibility and, therefore, in its epistemic quality (Ziman, 2002, 397). Instrumental science is "captive to material interests" and "serves specific power groups." For this reason, its judgments are "partisan rather than objective," and the solutions suggested in its framework are narrowly targeted to the problems at hand. As a result, the knowledge produced by instrumental science is "local" and unimaginative; and pragmatic success is the only criterion of acceptability (ibid., 399). Likewise, Silvan Schweber fears that the politicization and commercialization of research favor lax confirmation procedures and undermine the cultural model of science as a truth-seeking enterprise (Schweber, 1993, 40). The claim is that key features of scientific research as a knowledge-seeking enterprise are sapped by the intrusion of politics and commerce. The economic or utilitarian orientation expels the epistemic culture.

My aim is to have a closer look at this claim of the epistemic decline of science in the context of application. The thesis I will defend is that this claim is largely mistaken and holds only under particular conditions which are seldom realized. Two related questions are whether the epistemic mode of problem selection is really most stimulating to the growth of knowledge and whether it is most beneficial to the public utility of science.

Three Ways of Selecting Research Topics

Let me address the last question first and come back to the issue of epistemic quality in section "The Epistemic Dignity of Application-Oriented Research". It has frequently been claimed that research questions are now chiefly raised within the context of application and that we are witnessing a secular transition from epistemic science (or "mode 1") to application-oriented science (or "mode 2") (Gibbons et al., 1994, 1–17). In contrast to the disciplinary mode of problem selection, research topics are mostly imposed on science by practical considerations which have their origins in politics or the economy. The question I address in this section is whether it is legitimate to subject science to non-scientific and extra-epistemic goals and to take research into the service of utility. I claim that science has no ethics-based right to be left to its own devices. Rather, freedom of research needs to be balanced by accountability.

The legitimacy of societal intervention is based on the non-epistemic outcome scientific knowledge can have. Scientific progress may produce adverse and unwelcome consequences outside of laboratories and libraries. The impact of science in the context of application permeates everyday life. The right of society to intervene in the research agenda derives from the risk or the potential damage associated with science. Relevant are, first, ethical constraints on scientific experimentation (as they are brought to bear presently on stem cell research), but also, second, the damage possibly done by the products of scientific research. The latter kind of risk perception is exemplified by the postwar debate about the responsibility of science as regards the production of nuclear weapons, the public opposition against atomic power plants in the 1980s, and the present widespread rejection of genetic engineering. In all three cases, legitimate claims are made that no research be conducted that is hazardous to humankind or violates human rights.

The right of society to shape the research agenda and to influence thereby pathways of scientific theorizing derives from the legitimate aim to protect humankind from detrimental consequences of science. This rather defensive train of thought can be complemented with an approach that places science in the offensive and takes scientific research primarily as a means for relieving the hardship of the human condition. In precisely this fashion science was conceived in the Scientific Revolution: the barren erudition of scholasticism was supposed to be replaced by knowledge that is beneficial for life. It is true, only around the mid-nineteenth century science reached a state sufficiently advanced for making it a significant source of technological innovation and industrial manufacture. But the promise of utility was part of the scientific enterprise right from the beginning, and it is this entanglement of knowledge and practice which underlies application-driven research.

This consideration brings us back to the mentioned practical mode of topic selection in scientific research. Prospects of usefulness provide an important motive for conducting research, which means in market economies that the research agenda is set on the basis of expected commercial success. Decisions about research items are made on the basis of estimates of the size of relevant future markets. Companies consider application-oriented research as an investment that is expected to yield financial returns.

It deserves emphasis that a research agenda set by considerations of utility or commercial interest is not without social advantage as compared to the epistemic mode of problem selection. The latter, namely, may constrain science to an ivory tower and restrict its exploratory endeavors to self-generated issues around which little turns outside of university seminars. The pressure of practice is sometimes helpful so as to break up self-imposed confinements and to hook up with the non-scientific world. An example of a beneficial political influence of this sort is the more recent debate in the "International Panel on Climate Change," an institution of the United Nations which won the Nobel Prize for peace in 2007. In the framework of the IPCC, scientists and politicians collaborate in composing reports on climate change and, in particular, in drawing up executive summaries. Climate researchers tended to develop their isolated and narrow models and did not dare to venture their integration. Some addressed atmospheric circulation, others wrestled with the details of oceanic currents, yet others dealt with the biosphere. All parties cultivated their disciplinary specialties. It was the politicians in the IPCC who urged scientists to interlink their partial accounts with one another and to create a more comprehensive representation (Küppers and Lenhard, 2007, 131–136). It is clear that from a political perspective action is what matters, and responsible, targeted action needs comprehensive accounts as its basis. In this case the pressure of practice had a beneficial influence on the research agenda.

Many instances of application-oriented research exhibit the same structure – if not at the same global scale. Giant magnetoresistance, whose discovery was honored with the Nobel Prize for physics of 2007, was first identified in public research institutes, to be sure, but was transformed into marketable products by industrial research. Giant magnetoresistance is suitable for building extremely sensitive magnetic field sensors; it underlies all of the more recent computer hard disks and has expanded their capacity considerably.

Examples of this sort demonstrate that a research agenda shaped by commercial aspirations is often in the interest of many people. This is no accident. Customers are human beings and for this reason market orientation generates a kind of democratic perspective for application-driven research. Only what is expected to be met with the approval of the market, only what will be appreciated and utilized by many, will be subject to privately financed research endeavors. Sometimes there is harmony between knowledge and commerce.

To sum up, it is the protection from damage and the prospect of the betterment of the human condition which gives society the right to intervene in scientific problem selection. Moreover, in some cases external pressure on the research agenda successfully stimulated the production of socially welcome outcome. So, it is not only desirable but also feasible to take research into the service of promoting the common good.

However, the present mode of political and commercial problem selection may engender serious side-effects. This is particularly striking in the economic realm. The crux of the problem is that all customers are human beings, to be sure, but that not all human beings are customers. Different groups of society and different segments of humankind exert an influence of quite different weight on the research agenda. Problem selection in industrial research does not take the consequences for all those into account who are affected by the research. The commercialization of research generates a list of items that is narrowly targeted on assumed market demands. Addressing economically promising areas is not always in the best interest of those concerned.

The most prominent examples stem from medical research whose biased agenda is notorious and obvious. Research efforts focus on illnesses that almost exclusively afflict the rich countries whereas Third-World ailments mostly fail to attract scientific attention. Large amounts of research expenditure is devoted to high blood pressure and diabetes, scourges of the wealthy, but little is done to improve the treatment of bilharzia or malaria which mostly plague poor countries. In quantitative terms, the suffering produced by the latter diseases exceeds by far the distress brought by the former. The research on methods of treatment is biased in a similar fashion. Privately funded research is focused on the development of patentable medical drugs and largely ignores lifestyle effects. Commercialized research may find out truthfully that some illness is alleviated by some expensive medication. Yet the question whether a similarly beneficial effect might have been accomplished alternatively by exercise and sports is not even asked (Brown, 2001, 210). If market interests are at work, the spectrum of problems addressed is narrowed and shaped in a one-sided fashion. In the wake of such a biased selection of research questions, only particular relations are uncovered while others remain eclipsed and unattended. Although nothing wrong is said, the preferred investigation of questions of a certain kind tends to give rise to a gappy and oblique account.

The upshot is that application-oriented research does not always promote public utility. The same holds for epistemic research that is frequently irrelevant from a utilitarian perspective or needs to be constrained by considerations of accountability. Strangely enough, the debate about politicization and commercialization often takes epistemic science as a remedy for the one-sidedness of application-driven research. In fact, by virtue of the democratic tendencies involved in market-based mechanisms, the latter is even more socially accountable than the former. In order to redress the balance, a third type of research is called for, namely, *science in the public interest* (Krimsky, 2003, 178). Research of this kind selects research questions according to the interests of all those concerned by the possible research results. Global warming is an example of a research endeavor of high practical importance, which neither grew out of epistemic research nor was it addressed by industrial research. Another case in point is the stimulation of a regime change in polio vaccination by a public sector vaccines institute. Up to the 1970s, polio vaccination had been dominated by attenuated-virus vaccines until findings indicated that the use of inactivated-virus vaccines (with which polio vaccination had begun in the early 1950s) was advantageous after all. But the market was in a so-called "lock-in" state in which a less than optimal solution persisted since the inertia of the existing systems and routines, as well as past industrial investments in production facilities, discouraged actors from introducing changes. Only thanks to the research activities of a Dutch public health institute on inactivated-virus vaccines could this lock-in state be broken up and an improved system be established (Blume, 2005, 164–171).

Such examples show that it may be the research done or promoted by public institutions, which produces insights that serve the public interest. Philip Kitcher has urged to intensify this third branch of research and suggested a public procedure designed to determine the research agenda. This procedure is intended to create a counterweight to the tacit influence of the rich and powerful on the questions to be pursued (Kitcher, 2001, 117–131).

Science in the public interest does not emerge by itself. In general, it requires active intervention and political interference to make sure that certain questions are asked and pursued in the first place. Science exclusively aimed at understanding nature runs the risk of losing contact with the exigencies of the social world around it. Exclusively market-based research is in danger of being tied to partisan interests and losing its commitment to the common good. Both kinds of imbalance need to be corrected by a science for the public interest (see section “Benefit and Hazard of Application-Oriented Research”).

The Epistemic Dignity of Application-Oriented Research

Societal intervention in the research agenda would be of dubious value if it tended to degrade the epistemic quality of knowledge. The examples given in the last section suggest that political and commercial impact on the list of research issues is not bound to have detrimental effects of this kind. Still, a more systematic consideration of the relevant influences is called for.

I use “application-oriented research” as the counter-concept to “epistemic research” (see section “Research in Extra-Scientific Interest”). Yet this conceptual distinction is not supposed to entail that a given research project belongs exclusively into one of these categories. On the contrary, the same research endeavor may be driven by the search for utility and at the same time aim to deepen our understanding of natural processes. For example, Louis Pasteur famously sought to elucidate fundamental biological processes and by the same token to prevent beer, wine and milk from spoiling or protect animals and humans from rabies (Stokes, 1997, 12, 63, 70–74). Yet in spite of their possible numerical identity, epistemic and application-oriented research projects can be separated conceptually by appeal to the goals pursued or, correspondingly, by the success criteria invoked. The conceptual distinction does not rule out that a given research project serves both ends simultaneously.

I mentioned that the heavy application pressure on scientific research raises apprehensions to the effect that the focus on utility and short-term economic benefit will issue in a diminution of research quality (see section “Problem Selection in Fundamental and Application-Driven Research”). These misgivings are motivated to a great extent by the supposedly purely pragmatic attitude prevalent in application-driven research. The proper functioning of some device is its chief criterion of success; intervention, not understanding, is at the focus (Polanyi, 1962, 182–183).

This suspicion of a thoroughly pragmatist attitude in application-driven research ramifies into three epistemic worries. The first one refers to the superficiality or the diminished epistemic penetration of applied knowledge. Theoretically integrated laws are replaced by observational regularities. Second, the emphasis on intervention produces loose standards of judgment in testing and confirming assumptions. The third worry addresses a supposed lack of creativity. In sum, the claim of epistemic decline says that targeted, application-focused research tends to neglect understanding, apply lax standards of quality control and to follow trotted-out paths (see section “Problem Selection in Fundamental and Application-Driven Research”).

The thesis of diminished epistemic penetration suggests that application-oriented research is satisfied with causal relations and ignores the elucidation of underlying mechanisms. A case in point is the identification of starter genes which trigger gene expression and are thus suitable for controlling genetic processes. For instance, the so-called "eyeless" gene controls for the eye morphogenesis of *drosophila* and other species. If the operation of the gene is blocked or lost, no eyes are formed – which is why the gene is somewhat misleadingly called "eyeless." The expression of the eyeless gene in suitable tissue is sufficient for eye formation. That is, eyes can be generated by appropriate stimulation in the legs or wings of flies. The same holds for other species like mice; the activation of eyeless reliably produces eyes. But eyeless only sets off a complex series of intertwined genetic processes which only in their entirety control eye formation. The gene operates as a trigger and allows the control of eye morphogenesis without theoretical understanding of the underlying processes.

In the 1990s, biotechnologists indeed argued that intervention can proceed independent of theoretical understanding. Genes are tools for bringing about intended effects, and this is what biotechnology is all about: identifying levers to pull and switches to press. There is no need to trace the complex chain from eyeless to the working eye. Pressing the initial switch is everything, biotechnology needs to care about (Bains, 1997). Put more generally, the argument was that control and intervention do not need causal analysis and theoretical unification but may rely on observational regularities and empirical adjustments.

Yet the later development went in a quite different direction and amounts to the inclusion of proteomics and the partial supplant of genomics by proteomics. The control of gene expression has gained prime importance for biotechnological research endeavors. Genes need to be switched on and off, and this is achieved by the action of proteins. Such proteins are in turn produced by other genes in the cell or stimulated by other influences from within the cell or from outside. The activity of a given gene is promoted or inhibited by a plethora of genetic and non-genetic factors and thus depends heavily on its context. In stark contrast to eyeless, the "distalless" gene acts in a more specific way and affects embryonic development differently. In caterpillar embryos, the expression of distalless induces the formation of legs, whereas in the developed butterflies the same gene generates colored eye-spot patterns on the wings (Nijhout, 2003, 91). Obviously, in some instances the context is of critical significance for intervening reliably which speaks in favor of preserving the depth of epistemic penetration. Using observational regularities devoid of theoretical underpinning does not always yield dependable results after all.

An example from chemistry points in the same direction: Relations without theoretical underpinning are difficult to transfer to other situations and phenomena since it is not obvious which conditions need to remain invariant. An example for how theoretical accommodation facilitates generalization refers to a reaction pattern called olefin metathesis. This pattern involves the exchange of carbon atoms between different compounds and provides a means for dissolving carbon double bonds which is difficult to achieve otherwise. However, initially the reaction was hard to control; it was not clear under which circumstances it proceeded and at which rate. The causal mechanism was clarified in the early 1970s and the role of metallic catalysts disclosed. Only after the underlying processes were understood, the reaction pattern could be applied reliably to other cases not studied beforehand. The elucidation of the mechanism, albeit incomplete and subject to further specification for the particular cases, made it possible to generalize the reaction pattern, anticipate the outcome of changed conditions, and develop more efficacious metallic catalysts. Olefin metathesis is now regarded as a revolution in metallorganic chemistry; the Nobel Prize for chemistry of 2005 was awarded for its clarification (Groß, 2005).

The second worry concerns the emergence of a sloppy practice of judging hypotheses in application-driven research. It is true, there are cases to this effect; for instance, a tendency in application-oriented research to disregard welcome anomalies. If a device works better than anticipated before on theoretical grounds, most researchers in the context of application offer nothing but handwaving as to the underlying causes. Ad-hoc-hypotheses without theoretical or empirical backing tend to be offered, enriched with the demand for further clarification which, however, nobody cares to conduct. Rather often, the case is closed gratefully. This failure to address the mechanisms underlying a practically promising effect confirms the suspicion of methodological deficiency (Carrier, 2004, 7; see Nordmann, 2004).

However, closer scrutiny reveals that such shortcomings remain occasional and cannot be generalized. More often than not, the demanding standards of judgment that distinguish epistemic research are retained. The reason is not difficult to identify: superficially tested relations will eventually fail to support technological progress. Conversely speaking, the theoretical integration or causal explanation of an empirical regularity improves the prospect of making reliable technical use of it. Disclosing causal mechanisms often opens up options for controlling a phenomenon, and giving a unified treatment may forge links to other relevant processes and thereby make accessible additional options for intervention (Carrier, 2004, 4–7). In sum, technological intervention needs to be based and is based, typically, on knowledge which has undergone a tough process of quality control.

Another consideration makes it plausible that demanding standards of judgment are retained in application-oriented research. Namely, the devices based on the results of such research need to operate properly not only under controlled laboratory conditions but also in chaotic everyday situations and fluctuating conditions. Suitability for practical use means in the first place that the impact of such distorting factors can be kept under control so that they do not impair the proper functioning of the device. Reaching this aim usually requires the theoretical penetration of the underlying effect.

The point is that superficially tested claims do not furnish a viable basis for reliably operating devices. In fact, apart from special cases to be discussed shortly, I have failed to identify any interference on the part of the sponsors. This is plausible enough since tampering with the outcome would be against the interests of the sponsors. What they pay for is reliable, robust results which stand the test of practice, not the approval of wishful thinking that would collapse under real-life conditions. In industrial research, unreliable performance or serious side-effects may jeopardize a company. Functional failures in products are often a threat to the manufacturer and this risk is augmented by gappy knowledge of the processes underlying the performance of a device. As a result, the standards of reliability are frequently placed at a level comparable to academic epistemic research.

Other cases show that the perceived risk involved in accepting a hypothesis increases the level of confirmation required for its adoption. In view of the potential damage involved, the EU authorities apply a "precautionary principle" for regulating biotechnological risks. In particular, the possibly deep-reaching impact of the introduction of genetically modified organisms on the biosphere has prompted the relevant EU institutions to tightening safety demands and raising standards for preemptive measures (Commission of the European Communities, 2000). In a similar vein, Nancy Cartwright has argued that epistemic standards should be raised in transition to the applied realm. Such "evidence for use" needs to be distinct in taking the large fluctuations in the conditions of everyday use into account. Such fluctuations arise either due to natural variations (which occur inevitably once the idealized, laboratory-like test conditions are removed) or due to thoughtless misapplication (notorious in everyday life) (Cartwright, 2006).

The upshot is that application-oriented research does not, in general, suffer from reduced standards in quality control. Rather, knowledge produced in the context of application is required to be subjected to additional requirements. It needs to undergird devices that perform their function in a stable way when being subjected to distorting influences. Robustness in this sense is just such an additional virtue which products of research in the context of application need to exhibit. There is no general tendency toward superficiality in application-oriented research.

Yet what are the special conditions under which epistemic degradation is likely to ensue? One relevant factor is the distribution of risks of failure between producers and consumers. If the producer risk and consumer risk are borne by different people or institutions, epistemic deficiencies are liable to arise. Take so-called phase-III clinical trials, the realm from which the large majority of horror stories about the corruptive influence of money on research are taken. The issue is who needs to pay for false positives and false negatives, respectively. If an efficaceous drug without side-effects is mistakenly dismissed in a clinical trial, we are faced with a false positive. If a harmful drug is passed erroneously, we are dealing with a false negative. In drug testing, false negatives are generally hard to detect after the corresponding study has been completed. Side-effects are mostly unspecific and difficult to attribute to a particular medication outside of a controlled study. This means, a company can hope to get away with a false negative, while the relevant patients would suffer. Consequently, a false negative is a consumer risk rather than a producer risk.

A false positive means that a drug developed with large amounts of money is kept out of the market without justification. False positives are mostly producer risks: the producers incur losses while the missed marginal benefit for the patients is usually small. In most cases a standard therapy exists so that the medical disadvantages generated by keeping the new drug from the market are typically rather small. It is clear that producers are keen on keeping the producer risk low and thus prefer false negatives to false positives. But the general public will evaluate the two risks of failure the other way around. At the same time, there is a lot of money at stake in clinical trials. The upshot is that the two kinds of risks are borne by different bodies (Wilholt, 2006, 70–71; Wilholt, 2009, 94).

This is different in cases like computer hardware research. The consumer risk is that the device won't work. But this agrees with the producer risk: companies selling malfunctioning devices will suffer economically. In sum, in clinical trials, but not in computer hardware research, producers strongly favor false negatives over false positives. Only if these two kinds or risks diverge, the sponsors of a study are inclined to accept particular kinds of errors, and only under such conditions, application pressure can be expected to bring about epistemic decline.[2]

The third epistemic worry mentioned is the supposed lack of imagination and creativity in application-oriented research. But this worry is without firm foundation. Instead, the choice of problems according to their solubility is rather likely to result in unimaginative research. Kuhn's normal scientist does not burst with creativity and innovativeness. This may be a defect of the Kuhnian picture in that real-life scientists are less confined to routine approaches than the normal scientists Kuhn portrays. In fact, Kuhn was frequently castigated for his allegedly unrealistic views in this respect. The salient point is, however, that the epistemic mode of problem selection does not vouch for valiant endeavors to transcend cognitive boundaries.

By contrast, what is striking is the reverse relation of a seminal influence of application-oriented investigations on epistemic research. It sometimes happens that the basic knowledge necessary for bringing about some technological innovation is produced in the context of application. Applied challenges may raise fundamental questions which need to be addressed if the practical task is to be mastered. Applied research never merely taps the system of knowledge and combines known elements of knowledge in a novel way. Rather, applied research almost always requires constructing specific models which are apt to control the processes underlying a device. As a result, applied research is bound to involve some amount of creativity. But the amount of novel insights fed into the models varies among applied challenges. The invention of the dishwasher mostly relied on the ingenious connection of known parts and needed no new theoretical knowledge. Yet in *application-innovations*, new insights are gained in the course of an applied research endeavor. Such "application-dominated research" is driven by technological intentions, to be sure, but the necessary scientific basis is not yet available or not sufficiently elaborated. Application-dominated research attempts to lay the scientific ground for a technological innovation. It is the epistemically fertile part of application-oriented research. In order to accomplish its practical objective, application-dominated research needs to produce novel pieces of scientific knowledge and improve the understanding of nature.

[2] In fact, phase-III clinical trials do not even constitute research in the proper sense since no discoveries are aimed at. Rather, the expectation is that earlier research outcome is confirmed and that a cumbersome legal procedure is completed smoothly. Clinical trials are obstacles to be overcome by pharmaceutical companies in order to get market access. The exceptional factor is that no genuine epistemic interests exist among those who pay for the study. The sponsors don't want to know, rather they believe they know and want to pass an inconvenient and economically risky examination quickly and without much ado (Carrier, 2009).

This kind of research is sometimes called "strategic research." Such research endeavors are driven by the search for utility, but the temporal perspective is rather long-term, the topical spectrum is broad, and the technological goals are unspecific. Quantum computation is a case in point, as is growing new human organs. A more mundane example is "giant magnetoresistance," as mentioned in section "Three Ways of Selecting Research Topics", which constitutes the physical basis of today's hard disks. In the 1980s, the search for the effect was motivated by prospects of application. The relevant laboratories looked for physical means to efficiently alter electrical resistance by applying magnetic fields. One such effect was known for more than a century, namely, "anisotropic magnetoresistance," whose physical basis is the spin-orbit coupling of the conductor electrons. The pertinent research teams actively searched for stronger spin-related effects of this sort and eventually came across a new effect of spin-dependent electron scattering that relied on spin-spin coupling. This giant magnetoresistance represents a novel physical phenomenon which was discovered en route to the applied aim of packing data more densely and developing hard disks of increased capacity. Moreover, the effect was correctly explained within this application-dominated research context (Wilholt, 2006).

Application-innovative research is successful application-dominated research; it is epistemically fertile but unintentionally so. The motive lies with technological progress but among the results are epistemic gains. The attempt to improve the control of nature leads to better insights into nature's contrivances. Application innovation involves a mechanism for stimulating creativity. In such cases, technological difficulties raise theoretical problems that would have hardly been addressed otherwise. The lesson is that practical challenges may promote the development of novel and original epistemic approaches.

Another instance of this kind is semiconductor doping by introducing impurities, typically metal atoms, into the crystal lattice of a semiconductor. Doping affects the electric properties of the semiconductor and can be used to tailor materials to specific functions. Doping is not only practically important but also theoretically significant. It was worked out in the 1940s in the Bell Labs in the context of developing the diode and the transistor. In devising these novel devices, new insights were gained into the band structure of and the electron flow in solid state crystal lattices. Research intended to seek a replacement for the inconvenient vacuum tube contributed a lot to solid state physics in its present form. Likewise, the revolutionary idea of a "retrovirus," i.e., a virus able of transcribing RNA into DNA in the course of its replication, was conceived around 1970 in the practical context of elucidating infection chains. Yet it had a tremendous impact on biological understanding and led, for instance, to an important qualification of the so-called "central dogma of molecular biology." The intention was practical but the impact was a deep-reaching transformation of biological concepts.

The basis of application innovation is the dependence of targeted intervention, reliable and not afflicted with side-effects, on the understanding of the underlying processes. This substantive relationship between scientific understanding and technological use may be called "cascade model" and should be distinguished from the "linear model" which delineates a temporal sequence ranging from fundamental discoveries to the manufacturing of new products (Carrier, 2009, Section 5).

The cascade model rather refers to entailment or logical dependence; it involves the claim that technological novelties typically rely on scientific understanding, albeit not necessarily on recent scientific progress. Practical tasks are best solved by bringing to bear insights into the underlying processes – as Francis Bacon famously claimed (Bacon, 1620, Bk. I., §§ 3, 110, 117, 129). Application innovation is the converse claim that if this understanding is lacking, it is generated in the context of application.

It is true, we are witnessing a change in the epistemic objectives in that the search for new overarching laws of nature tends to be replaced by the attempt to extend the grip of known laws to the subtle details of experience. The in-principle understanding, as provided by general theory, hardly ever suffices to actually build a device. Lots of details need to be added in order to achieve robust and reliable control of the processes underlying some technical device. In this sense, application-driven research always involves gaining a creative understanding in its own right. In general, applied research endeavors are no less epistemically demanding than academic research projects.

These considerations suggest that the commercialization of research does not result, generally, in a loss of depth, credibility and creativity and that, consequently, the thesis of epistemic decline is mistaken. This contention is backed up by the observation that fraud and falsification of data is in no way restricted to application-driven research. Scandals in the applied domain, like the Vioxx swindle (Biddle, 2007), are complemented by fraudulence in fundamental research, the cases of Jan Hendrik Schön in 2002 and Hwang Woo Suk in 2006 being the most spectacular in the recent past. It is not only the race for profit but also the lure of gaining reputation, receiving positions, and being awarded research grants that may corrupt researchers and seduce them into manipulating the evidence and cooking up data. Fraud in fundamental research mostly involves the claimed discovery of phenomena that are expected anyway by the scientific community. The forger's idea often is that somebody else will soon make the relevant discovery in reality and thus confirm the forger's ostensible breakthrough.

The striving for reputation along with its latter-day collateral benefits such as awards, public attention, and media presence tend to corrupt science no less than application orientation. This detrimental impact of "medialization" was placed in the limelight during the final phase of the Human Genome Project. Its closing stages were characterized by a competition between the publicly financed program and the private company Celera. The primary effect of this finish, watched closely by the mass media, was a serious deterioration of the data. The public project had initially published only multiply checked sequences whereas the private company accepted flawed raw-data as sequenced genome sections. In the wake of this entry of a private company, speed began to matter more than dependability.

Yet this lowering of the standards was not brought about by application pressure but by the pursuit of media attention. The company publicly bragged about "winning the genome race" and employed this ostensible victory in a marketing strategy for its other products. The genome data themselves were faulty and could not be used nor sold for this reason. In fact, the process of deciphering went on for years after its declared completion in 2001.

The lesson is that medialization may lead to corruption even without application pressure. Conversely, the competition between Luc Montaigner and Robert Gallo regarding the identification of HIV in 1983 proceeded under heavy application pressure and with huge economic benefits in view. But this time the race did not produce a similar decrease in reliability. It can safely be assumed that the preservation of high standards was due to the lack of media observation. It seems not so much the application-orientation as such but rather the quest for publicity and fame which undermines quality standards of research.

Benefit and Hazard of Application-Oriented Research

I wish to condense the preceding considerations into some conclusions. To begin with, it merits notice that the application-orientation of research has led to a huge influx of research money into science and has prompted an unprecedented dynamics of research. It is often claimed that the majority of scientists of all times live and work now, in our age. Application-oriented science has been a major factor in an accelerated growth of knowledge which I for my part appreciate and welcome. "All men by nature desire to know" Aristotle rightly says in the opening sentence of his *Metaphysics*. Application-oriented research serves to fulfill this desire.

Second, application-oriented science does not suffer, in general, from a loss of depth, credibility or creativity. Uncertainty, superficiality, tentative epistemic strategies, and corruption haunt application-oriented research but epistemic research is also not free of such detriment. It is not true, generally speaking, that pursuing practical ends undercuts or overturns epistemic aspirations. In the decades after World War II, the prevailing attitude was that fundamental research should be shielded from interests of application, since otherwise the growth of knowledge would be impaired. The Bush report is among the most influential sources of this view (see section "Problem Selection in Fundamental and Application-Driven Research"). But confirmatory evidence is rare. There are instances of epistemically fruitful application-driven research and examples of epistemically deficient fundamental research. Practical objectives may drive science into uncharted territory, and prompt research to embark on demanding epistemic projects. What matters is not so much the interests pursued, but openness and leisure to look for and explore unexpected novelties. The traditional demand to keep the quest for understanding separate from the pursuit of practical ends cannot be justified by appeal to the quality of the knowledge produced.

This consideration entails an epistemic vindication of application-oriented research. It is true, the latter often taps the reservoir of the system of knowledge rather than generating novel insights. In most cases, application-oriented research is parasitic upon epistemic research and merely combines familiar knowledge elements in an unfamiliar way. Application innovation is a rare achievement. However, as claimed before, the lack of imagination and creativity also tends to beset epistemic science and the disciplinary mode of problem selection, whereas practical challenges sometimes stimulate epistemic innovativeness (see section "The

Epistemic Dignity of Application-Oriented Research"). What is essential, in addition, is that application-oriented research does not, in general, suffer from a loss in credibility and trustworthiness.

Instead, the trouble lies with the one-sidedness of the research agenda. This is particularly glaring in pharmaceutical research. Among the 1,360 new medical drugs which were admitted to the world market between 1975 and 2000, only 10 had been developed specifically for Third-World illnesses (Schirmer, 2004, 111). What causes concern are cases like eflornithine, a medical drug originally developed for treating the African sleeping disease which kills its untreated victims. But the production of the drug was stopped because of the insufficient market volume. The patients couldn't pay and did not qualify as customers for this reason. Only after it was discovered that the drug could also be employed in removing unwanted facial hair and thus proved helpful for cosmetic purposes, was production resumed (Schirmer, 2004, 111–112). Of course, medical research is not to be blamed for this scandal; after all, it had developed the drug in the first place. But this example places in the limelight the dubious system of incentives that governs medical research. This case nourishes worries to the effect that in other, similar instances the drug was not jettisoned after its market release but before.

For instance, the lack of effort on the part of the pharmaceutical industry to combat tuberculosis is striking. For half a century, no research endeavors regarding this disease, widespread in particular in the Third World, were made by the industry. All relevant initiatives were undertaken by private foundations and state institutions (Barry and Cheung, 2009, 59). Another case in point is the decline of vaccination research. Vaccines are inherently less profitable than drugs against recurrent or chronic diseases. The vaccine is administered once and a costly treatment is avoided. In cases of this sort, the contrast between commercial goals and public interests becomes strident and obvious. However, it should be kept in mind that the commercialization of medical research is nowhere written in stone, but rather the result of the withdrawal of state authorities from public health needs (Blume, 2005, 160–161; see Krimsky, 2003, 179). In medical research, we witness a deplorable depoliticization in whose course private companies are handed over large areas of public affairs in order to save costs.

The upshot is that the most questionable aspect of commercialization is the biased research agenda thereby set up. It is impossible to do research on everything, and the selection of problems worth being studied depends on interests and values, which are often partisan and particular rather than universal and comprehensive. However, it is only the lack of a public counterbalance which makes this obliquity so pernicious. I had distinguished between epistemic research, application-driven research and research in the public interest (see section "Three Ways of Selecting Research Topics"). It can hardly be demanded of privately financed application-driven research that it be always conducted for the sake of the common good. The pursuit of public interests is in the first place a matter of the public. The wrongful priority list of medical research is first of all the result of the decline of public medical research. We are called upon as citizens to urge the relevant political bodies to redress the balance and to see to it that science for the benefit of the people gains its due significance. Certain forms of the politicization of science are appropriate and even overdue.

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Between the Pure and Applied: The Search for the Elusive Middle Ground

Margaret Morrison

Introduction: Defining the Problem

It is commonly thought that in many cases a sharp distinction can be drawn between pure and applied science. We typically think of the former as involving work done in the pursuit of explicitly theoretical knowledge without an eye to its possible applications. Applied science is usually concerned with such applications and the production of concrete results geared toward specific technological problems. Engineering, instrumentation, medical technologies and drug development are all examples of what we understand as applied research. Despite the ability to draw such distinctions it is also commonly believed that pure and applied science are sometimes linked. For example, superconducting magnets required to direct particles travelling in accelerators at the speed of light form the basis of MRI technology used to diagnose illnesses.

These various relationships raise an obvious philosophical question: is there more to the pure/applied distinction than simply application related goals; and, if so, how does one characterize the differences and/or similarities? One obvious way might be to focus on differences in *methodology* and the *kind of knowledge* that is produced in each context rather than the use to which that knowledge is put. For instance, theoretical science typically focuses on the development of laws and broad explanatory principles that enable us to unify phenomena and explain their behaviour. Applied research may be less concerned with explaining "why" a particular phenomenon behaves as it does and more focussed on explaining "how" its behaviour can be reproduced in certain concrete settings. While I don't intend these differences to define the boundary between pure and applied science, we might think of them as a starting point for analysing the distinction in a way that goes beyond "use-related" issues.

That said, Martin Carrier (2004a) has provided a powerful defense of the epistemic power of local models in both pure and applied research by showing

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how both fields are committed to virtues like unification and causal analysis. He highlights how these goals are achieved in each domain by using a network of local models that enhance understanding and explanatory power. Hence, both fundamental and applied science employ epistemic strategies that are tentative and heterogeneous; but rather than detracting from our ability to provide comprehensive explanations of the phenomena these features actually enhance that capability. Indeed, he argues that in many cases this type of methodology is necessary for empirical adequacy [11]. The reasons for the parallels can be traced to the difficulties in linking up theories with facts. In pure research the theory-phenomena link is achieved via model building strategies that often focus on the particular problem at hand rather than attempting to derive explanations/predictions from fundamental laws and theoretical principles. In other words, theory does not often succeed in reaching the level of concrete experience. In applied research this problem manifests itself as the difficulties in translating general insights into working devices [9]. The universal principles characteristic of theory often “fail to extend to multi-faceted and complex experience” [ibid.]. Hence, on this characterization it would seem that both pure and applied science utilize similar methodologies. But does that mean that they produce the same kind of knowledge?

While Carrier wants to maintain some distinction between the pure and applied he argues for an interactive view stressing that technology development is heavily dependent upon scientific understanding. He claims that applied science in general is bound, for methodological reasons, to transcend itself and grow into fundamental science (2004b). Applied science tends to focus on contextualized causal relations that are relevant for the problem at hand but without any deeper theoretical understanding of these causal relations the opportunities for intervention and control are limited. “A good theory is extremely practical” [ibid.]. What this seems to entail is that while applied science may start out with different goals and differently structured knowledge it will very often evolve into a theoretically based activity.

The merging of, and interaction between, pure and applied science involves what Carrier calls application dominated research (ADR), a specific activity where epistemic fertility and new insights into nature’s workings become possible. This ability to produce new insight he calls application innovation. So, instead of trying to redefine the boundaries between pure and applied science we have a new context where the pure and applied come together to produce theoretical knowledge with practical advantage. As Carrier notes, applied science has a diverse nature and a variety of different methodological orientations [3]. Application dominated research qualifies as applied science because it is driven by concrete technological interests directed at practical goals but its methods involve more than just application of basic principles; hence it typically produces new knowledge of some kind. To that extent it functions at an intermediate level between applied science simpliciter and pure theoretical research. Because applied science involves a commitment to the proper functioning of some *device* its research agenda is often set from the outside; as a result it is usually devoid of epistemic novelty.

In making these distinctions Carrier calls our attention to what I think is exactly the right question to ask: How do we distinguish between the structure of knowledge gained in applied research as opposed to simply bringing theoretical knowledge to bear on practical questions? Carrier suggests that properly posed "application questions" can produce epistemic insight by transcending purely practical concerns and addressing the *fundamental problems that arise in the context of application*. In that sense ADR differs in character from both pure and applied science and offers a genuinely new way of thinking about the relation between them.

While I wholeheartedly endorse many features of this characterization my concern is whether it is possible to articulate a sufficiently sharp distinction between what Carrier calls "application dominated research" and the practices and methodology involved in cases of applying theory. Put differently, what is the nature of Carrier's "application questions" such that they enable us to demarcate ADR from other types of theory-application? If we consider condensed matter physics (CMT) as providing examples of ADR we find that in many contexts our classification results from a comparison with disciplines like particle physics which is more theoretically motivated. So, although CMT certainly has more application based credits than high energy physics, a good deal of its research agenda is nevertheless controlled from *within* the discipline where the concerns are first and foremost epistemically oriented. Although advances in CMT have been responsible for tremendous technological advance it isn't at all clear that it fulfills the criteria of applied or application dominated research. The issue then seems to be one of spelling out the nature of the "application questions" that characterize ADR in an effort to see whether this can help in characterizing the middle ground between pure and applied science.

In articulating the boundaries of ADR two issues come to mind. First, although Carrier has shown that heterogeneous models are characteristic of both pure and applied research it would seem that for ADR to provide a robust middle ground it must have its own methodological practices that yield knowledge whose structure is distinct from either pure or applied science. Secondly, focussing on the fundamental problems that arise in the context of application suggests that the activity of application exposes theoretical problems that can then be addressed in a practical context. However, while the application context may *expose* the difficulties it isn't immediately clear that they can be *resolved* in that context. Quite possibly, one will need to refer back to the domain of pure research before the application can proceed. And, this "referring back" needn't involve a well defined interaction between the two domains. Hence, unless we can isolate a particular type of scientific research/practice as "application dominated" the category may not be as helpful as we prima facie think. In an effort to shed some light on these issues I want to look more closely at a particular part of CMT, the development of superconductivity. The example is particularly appropriate because the evolution of the science and methodology is closely linked with both purely theoretical as well as application questions. My goal is to isolate aspects of the development of superconductivity that might enable us to differentiate practices or knowledge that can be properly characterized as application dominated. However, as we shall see below, this proves to be a more difficult task than one might anticipate.

Superconductivity: Puzzling Phenomenon to Microscopic Theory

Understood in physical terms, superconductivity is really just a special case of superfluidity (electrons which are fermions create a "bosonic fluid") which was first discovered by Onnes in 1908 in liquid Helium. (The interest in liquefying gases began with Faraday in 1823 when he liquefied chlorine.) What Onnes later discovered was that when cooled to a liquid Helium temperature Mercury looses all trace of resistance to the flow of electricity. It was soon found that many metals and alloys become superconducting below a transition temperature characteristic of the material. Another peculiar feature of this phenomenon was that once a current was started in a superconducting ring it flowed indefinitely with no source of power. However, because it defied theoretical explanation despite the advances in quantum theory, little was known about zero resistance. Most of the work in the field consisted of investigation of various types of superconducting materials and increasing the transition temperature, which would be necessary if superconductivity was to have any practical applications. In the 1930s a further feature of superconductivity, the Meissner effect (the expulsion of magnetic flux from the interior of a superconductor at transition temperature), was discovered. Although some progress was made during this time in developing a phenomenological theory, no microscopic account was forthcoming until the Bardeen-Cooper-Schreiffer (BCS) paper (1957) which forms the basis of the current account of superconductivity.

Once that theory was in place research in the field seemed to diverge, with the theoreticians attempting to refine and extend the theoretical picture and the experimentalists searching for higher temperature superconductors. Because the temperature issue is related to, among other things, applications of superconductivity to practical (technological) contexts it serves as a possible case of application dominated research. In order to determine whether it fits this category we need to first identify what constitutes the theory, what its fundamental principles are and the extent to which they explain important phenomena in the designated field. In other words, we need to know what the boundaries of BCS are in order to see whether investigation into superconducting *materials* serves as an example of application dominated research or whether it is simply a case of applying theory in different contexts, as is the case with a good deal of experimental investigation. Clearly there are cases of "applied science" in the field of superconductivity, such as the now defunct programme by IBM to build a superconducting computer based on the Josephson effect and attempts to build levitating trains. These cases more or less speak for themselves in terms of methodology and goals. However, in the search for high temperature superconductors materials research is a different type of endeavour, a seeming hybrid between pure and applied science which is why it presents interesting possibilities as a case of application dominated research. Let me quickly review the fundamentals of and difficulties with BCS and then go on to see where materials research fits in the overall framework.

Prior to 1957 the phenomenological models of superconductivity focussed on thermal and electromagnetic properties of superconductors. These models all incorporated an energy gap between the normal and superconducting states to describe thermal properties; the task of the microscopic theory was to explain, in a quantum mechanical way, why the gap arises.[1] Work by Frohlich and Bardeen in the 1950s pointed out that an electron moving through a crystal lattice has a self energy by being "clothed" in virtual phonons. This distorts the lattice which then acts on the electron by virtue of the electrostatic forces between them; in fact, one can think of the interaction between the lattice and electron as the constant emission and re-absorption of phonons by the latter. The problem however is that the phonon induced interaction must be strong enough to overcome the repulsive Coulomb interaction, otherwise the former will be swamped and superconductivity would be impossible.[2]

So, the question was how to account for the electron–phonon interactions such that they would give rise to the gap in the one-electron energy spectrum. Further investigation by Cooper (1956) revealed that two electrons with the same velocity moving in opposite directions with opposite spins had an attractive part that was stronger than the normal Coulomb repulsion. This net attractive interaction involved a dynamical pairing of the two electrons, a process that came to be known as "Cooper pairing". As long as the net force is attractive, no matter how weak, the two electrons will form a bound state separated by an energy gap below the continuum states. In short, the phonon induced interaction gives rise to Cooper pairing which produces the energy gap required for superconductivity. The Cooper pairs form a condensed state whose lowest quantum state is stable below an energy gap that separates the normal and superconducting states.

The 1957 BCS paper contained an account of the pairing process which provided both a *general* explanation and representation of how superconductivity arises. Essentially it is a property not of the atom but of the free electrons in the metal; electrons that do not move independently. The overall picture is this: At 0 K the superconducting ground state is a highly correlated one where in momentum space the normal electron states in a thin shell near the Fermi surface are, to the fullest extent possible, occupied by pairs of opposite spin and momentum. This picture allowed BCS to focus on those single electron states that had paired states filled, allowing them to construct what they called a "reduced" Hamiltonian. This allowed for a more simplified mathematical approach that dealt with only the essential aspects of the superconducting state itself. In other words, they assumed that all interactions except Cooper pairing are unaltered at the normal to superconducting state transition. That is, the only energy change involved when a material goes superconducting is due to the formation and interaction of Cooper pairs.

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[1] An energy gap is simply a gap between the valence and conduction energy bands; metals, however, do not have separate bands but a single band containing many more states than electrons to occupy them.

[2] This is what happens in the case of semiconductors, i.e. solids which also have an energy band gap but yet don't show superconducting properties. The key difference between these two types of metals is of course the presence of Cooper pairs.

This idea of electron pairing was the fundamental "core" of the theory and is thought to constitute the basic causal mechanism responsible for low Tc superconductivity.[3] BCS made several other *specific* assumptions about *how* the electron pairing might take place and indeed there have been several different accounts of the pairing process put forward since the initial BCS theory, some of which correspond more or less with experimental results on pure metal superconductors. There were two main problems that affected the BCS theory: one was its lack of gauge invariance, a purely "theoretical" problem that was soon remedied by Philip Anderson; the other was a problem from solid state physics proper that concerned the phonon mechanism and the lack of quantitative energetics that would allow one to predict which materials would become superconducting.

At this point in the development of superconductivity we begin to witness a deviation away from pure theoretical work toward a more practically based approach. The reasons for this are two-fold. The first concerns questions about whether the so-called "theory" had really explained the basic mechanism of superconductivity and if so where does one go from here with respect to application questions. The theory seemed to lack any suggestions for applications or ways of approaching them. Secondly, if one assumes that fundamental features of superconductivity should furnish *predictions* about materials and temperatures then a good deal is lacking from BCS and another approach is required. In that sense what constitutes more practically oriented or application related activities will depend on how we understand not only the boundaries of fundamental theory but whether and what type of further research is likely to answer outstanding questions. That is, gaps in the theory that relate to its application may require more theoretical inquiry rather than what is provided by application dominated research. But, which path is taken will be determined largely by the ability of the theory to suggest the direction of future research. If the theoretical foundations are firmly in place and the theory is able to provide a context for formulating application questions then it is unlikely that answers to these questions will yield new "knowledge". If, on the other hand, gaps in the theory are too large then it is also unlikely that application dominated questions will reveal new theoretical discoveries. It would seem then that ADR requires a delicate balance of theory and application in order to fulfill its agenda. In many cases of solid state physics once basic ideas have been clearly posed and understood the motivation to continue reproducing theoretical refinements wanes. For instance, in the case of semi-conductors questions about theoretical or intellectual goals were always overshadowed by questions of utility, turning results of research into widely used practical devices. Superconductivity did not follow this path because successful applications never constituted an appreciable market. But why was this the case?

The reason was because BCS had enormous explanatory power and virtually no predictive power. While it provided a global description of superconductivity it could not explain the differences in behaviour from one metal to another. It explained why the energy gap occurs but could not predict the precise value for the gap in a specific material. It included an expression for the critical temperature

[3] For a discussion of BCS as it relates to the issue of theories and models see Morrison (2007).

but that value had to be given by experiment and only then could one deduce the magnitude of the electron–phonon interaction. These problems were addressed by developing models of the interaction between the electrons and the crystal lattice but they had only moderate success and involved very difficult calculations. It was these difficulties that led superconductivity out of the domain of pure research and into what was known as materials research. So, as a result of difficulties in fleshing out the theoretical picture, pure research gave way to more practically based approaches. But, as we shall see, none of these exemplify features of application dominated research.

From Materials Research to Josephson Junctions: Between Theory and Practice

Materials research tries to produce new materials and study their physical properties with an eye to certain kinds of potential applications. The idea was that it should occupy a middle ground between the demands of fundamental research and those of technical studies. In some sense this seems to fit the mould of "application dominated research". However, while it avoids the clash between pure and applied science it runs the risk of being reduced to simply characterizing materials synthesised by chemists; a kind of measurement with no guiding principles. The crucial question is whether the *results* of superconducting materials research can be characterized as epistemically significant and if so, is the methodology truly empirical (i.e. application dominated) in ways that allow us to distinguish it from pure research. The other issue, of course, is whether application dominated research needs to be defined in terms of a specific goal that is related to some technological function or purpose. While Carrier's account emphasises application innovation – the emergence of theoretically significant novelties within the framework of use-oriented research – it isn't necessarily tied to specific types of application. It would seem then as long as the questions are "use-related" that is sufficient to qualify as ADR. In that sense the generic category of materials research seems to satisfy, at least prima facie, the conditions.

While these issues are relevant for the "applied" part of materials research, one can also characterise the search for new materials in a more theoretical way – as an attempt to understand the systematics of superconductivity and thus obtain a deeper understanding of the factors that determine critical temperature and other parameters. Once again, how to characterise the interaction between the applied and theoretical aspects of materials research will depend, in part, on the role that theory plays in achieving the desired goals.

As we saw above, the lack of predictive power for critical temperatures was the primary motivation for many solid state physicists to turn away from theoretical work toward more empirically based approaches. Bernd Matthias from Bell labs, a solid state physicist with strong roots in chemistry, used the periodic table as his guiding principle, systematically exploring all the elements, alloys and compounds to see whether they were superconductors. Matthias developed some empirical rules involving electron/atom ratios and atomic volumes that proved extremely useful as a guideline for estimating critical temperatures and finding new material (Matthias and Stein, 1980). His work emphasised that for superconductivity to occur the type of chemical bonding between atoms, the crystal structure and the metallurgical state of the material are all important. The outcome of the research was that Matthias increased critical temperature which led not only to a significant improvement in the detailed BCS expression for that parameter but also to the discovery of type II superconductors where mixed states involving normal and superconducting regions coexist. So, although he and his group did not succeed in finding an exception to the basic electron-phonon mechanism (which they had hoped to do) they did make the theory more responsive to experimental facts. So, can this be appropriately classified as application dominated research? The outcomes are epistemically important but the goal is only indirectly related to applications. In other words, while the methodology is strongly empirical there is, strictly speaking, no context of application. Instead materials research seemed more like a way of supplementing a less than productive theoretical approach .

The other effect of materials research was to turn solid state physics into a kind of industrialized discipline, not in the sense of having industrial applications but rather in following the "big science" model of particle physics. Studies on synthesised anisotropic materials required precise knowledge of their crystalline structure. Crystallographers had new tools for generating X-rays – the accelerators that had become rather passé in high energy physics were now used to characterize precisely the distortions induced by charge density waves in anisotropic conductors. Neutrons from nuclear reactors became the tools of crystallographers because of their sensitivity to phonon scattering which could reveal changes in atomic structure. Interpretation of these results was clarified in terms of the quantitative theory phase transitions developed by Landau.

While much of this "materials" research looks more like basic research, or at least an interplay between theory and experiment rather than "application dominated", there is no doubt that the understanding of type II superconductors and their behaviour in magnetic fields led to a broad range of applications. One of the best known is cryogenic cables for superconducting electromagnets used in particle accelerators and MRI machines. But these are, in a sense, applications that are specific to other areas of science and instrumentation; applications that result from increased theoretical understanding rather than vice versa. These applications, in turn, produce new theoretical knowledge generated from within the relevant disciplines where the application is used. But that is a characteristic feature of all scientific activity – new instruments produce new knowledge – rather than a defining feature of ADR. Another type of application is the high current variety used for levitating trains; a programme that hasn't borne much fruit with high speed trains being a consequence of sophisticated metallurgy rather than superconductivity. The important point however is that no increased understanding of the nature of superconductivity has emerged from these applications. Instead, one could say that it is the *promise* of potential applications, rather than any specific one, that has motivated much of the materials research in superconductivity, research that, in the end, does not really satisfy the constraints of ADR.

But perhaps there is a "technological" reason for this. The high current applications of superconductivity are all inspired by well-established techniques and apparatus so it looks more like a case of simply applying theory to an intended domain. There is however another important field of application that involves the Josehpson effect which represents the most direct demonstration of the macroscopic quantum character of the superconducting state. Applications based on the Josephson effect, unlike those associated with the high current effect, are entirely new and require new tools and techniques; consequently they may provide a context where application dominated research is primary.

The precursor to the Josephson effect was the discovery by an engineer turned physicist Ivar Giaever (1960) of the tunnelling of a single electron. The possibility of tunnelling had been known since the early days of quantum mechanics where in alpha decay in nuclei, particles could escape through a hill of potential energy too high for a classical particle. Hence, the idea of it tunnelling through the potential barrier. Giaever thought that the energy gap of superconductivity might show up in the tunnelling characteristic and since the gap corresponds to a few millivolts it should be in an accessible range. The tunnel junction was a kind of sandwich in which a very thin insulating layer separates a superconducting electrode from a normal one. When a voltage difference was applied to the electrodes a current flowed from between them despite the insulator. The magnitude of the voltage necessary for this effect turned out to be a direct determination of the energy gap in the superconductor. Tunnelling experiments also yielded the strength of the electron–phonon interaction which, in BCS, was taken account of by a phenomenological term that could not be predicted from within the theory or its various models. What Josephson (1962) later found was that this tunnelling also took place with Cooper pairs even when there is no voltage difference between the superconductors (DC). And, when there is a voltage difference there will be an alternating current. The two types of current depend on the relative phases of the wave functions in the two superconductors.

The Josephson effect was a theoretical breakthrough which showed that the tunnelling depends on the relative phase between two macroscopic objects. Unlike ordinary microscopic objects the waves associated with Cooper pairs in a superconductor extend over a distance of 10^{-6} m. This size scale defines a coherence between all electrons forming the pairs which extends over the whole of the superconducting body. In other words, they behave not only in the same way with respect to their internal structure but also with respect to their motion. The waves superpose to form a synchronous co-operative wave with the same wave length. This phase coherence is what leads to the energy gap and is why superconductors exhibit quantum effects over large distances. Part of the theoretical significance of Josephson's work was showing the importance of the phase of the wave function. He demonstrated that if two superconductors were close enough together the two independent wave functions (where the phase of one is random relative to the other) would cross the barrier between them and couple to each other, effectively locking the phase of one to the other. Because of this overlap a supercurrent could flow from one superconductor to the other with the sign of the phase difference determining the current direction and the intensity of the current depending on the magnitude of the phase difference. The phase difference had effectively become a physical object! The question now was whether it was possible to build quantum interferometers based on the Josephson current as a measure of the phase difference between two circuits. The construction of just such a device was one of the first applications of the Josephson effect and has become one of the most sensitive detectors of magnetic fields ever constructed.

What is particularly interesting from the point of view of applications is that once the experimental findings supported Josephson's hypothesis, the experimentalists Rowell and Anderson consulted a patent lawyer who advised them that Josephson's paper was so complete that no one was ever going to be very successful in patenting any substantial aspect of the proposed effects. Not only had he formulated the necessary theoretical foundation for Cooper pair tunnelling and its physical ramifications, he also explained how to observe the effects he had predicted. One of the most significant aspects of the ac Josephson effect is that it allows a measurement of frequency as a function of voltage which allows for the ratio of e/h to be obtained more accurately than by any other method. A good deal of the application work has involved enhancing the technology for producing tunnel junctions but perhaps the most promising area for application dominated research was in the development of SQUIDS, superconducting quantum interference devices. So, unlike the case of materials research which was prompted by significant gaps in the theoretical framework, the Josephson effect fleshed out the theory in ways that cleared the path for applications.

Before looking at the ways in which SQUIDS might serve as an example of application dominated research we should reflect again on the conditions used to define this category. Until now we have seen that much of the application based work in superconductivity falls in the domain of either applied science, experimental activity, applying fundamental theory in the construction of specific technological devices, or using materials research to fill in gaps in the theoretical framework . The Josephson effect, by contrast, involves the application of theory to concrete phenomena that are otherwise not well understood as in the case of tunnelling of Cooper pairs. Although SQUIDS clearly qualify as an application of these ideas, in order to determine whether they fit the mould of application dominated research we need to know not only the goals of SQUIDS research but also the type of knowledge that is produced in these contexts. Depending on how we characterize each of these parameters SQUIDS may or may not qualify as application dominated research.

From Theory to Machines and Back Again

SQUIDS are devices that are the practical consequences of flux quantization (magnetic flux is a multiple of the basic quantum mechanical unit h/e) and phase coherence around a superconducting ring intersected by a Josephson junction. The advantage of a SQUID is that it converts extremely small changes in magnetic field into a voltage measurable at room temperature. It is the most sensitive device of any kind available for the detection and measurement of minute changes in magnetic fields, especially the very weak fields associated with biological technology. Consequently it forms the foundation for advanced measurement technology.

In a SQUID the incoming Cooper pair wave current flows around the two halves of the ring and recombines on the far side. The separate superconducting electron pair waves pass across the two Josephson junctions and interfere when they meet (a demonstration of the macroscopic phase coherence in superconductors). The changes in current induced by variations in the magnetic field are measured as voltage produced across the SQUID so it operates like a magnetic flux to voltage transformer of extreme sensitivity. Their versatility stems from their ability to measure any physical quantity that can be converted into a magnetic flux (e.g. magnetic fields, field gradients, current, voltage and resistance). They have applications in medical diagnostics, geological prospecting, measuring instruments and fundamental physics such as in the search for gravity waves.

So, given these diverse areas of application can we classify SQUID research as application dominated? The development of the SQUID itself was just an extension of principles used in constructing Josephson junctions but the applications of SQUIDS in other areas yielded a number of results that contributed to pure research. Four SQUIDs were employed on the Gravity Probe-B experiment designed to test the limits of general relativity. In addition, using an array of SQUIDS it is possible to map the spatial variation of the magnetic field produced by an organ which can then be correlated with an abnormality. Magnetoencephalography (MEG) is increasingly employed by neuroscientists to study the way that signals from our senses are processed by the brain. The magnetic field profile produced by these processes and their location are detected by MEG. Mathematical modelling can then be used to assess what a magnetic field pattern corresponds to in terms of fundamental electrical activity in the brain resulting from the sensory stimulation. The pioneers in MEG technology in Helsinki (Olli Lounasmaa) were also pioneers in SQUID technology in the 1960s and developed important techniques in ultra-low temperature physics, making important contributions to the understanding of superfluidity in liquid He3. Lousnamaa's quick recognition of the potential for SQUIDS outside low temperature physics and his ability to exploit this resulted in important contributions in areas as diverse as superfluidity and brain research (Matrican and Waysand, 2003).

What this suggests is a possible reformulation of application dominated research, especially in the field of superconductivity, to include activities that involve *using* devices in certain ways rather than simply acquiring knowledge as a by-product of *building* devices or having a particular *technological* goal in mind. In other words, we ought to distinguish between devices used in experimentation such as accelerators that are employed specifically to investigate the predictions of hypotheses and the ability to use technological devices in ways that permit the unintended transfer or development of knowledge from one context to another. The latter was the case with Lousnamaa's use of the SQUID in both low temperature physics and encepholography. I focus on this notion of knowledge *transfer* for what I think is an important reason. If we simply define application dominated research as involving the use of technological devices in production of new and unexpected results then there is very little to distinguish the activities typical of experimental work from those characteristic of application dominated research. Instead we need to define the context for application dominated research such that it extends beyond questions that have application goals in mind in order to capture the innovative way in which technology in various forms can be put to use in the production of new knowledge.

Contrast this with the discussion above on materials research which classified as fundamental because the *goal* was to fill perceived gaps in the BCS theory that related to prediction of critical temperatures. At the same time, the prediction of critical temperatures in specific metals is an important criterion for rendering superconductivity useful in practical contexts. Because the promise of new knowledge arising from applications requires that features of superconductivity that facilitate application be well understood, it seems that materials research finds itself in a kind of no-man's land. Indeed, if we look at the connections between materials research and the field of superconductivity more generally we find little in the way of a coherent, goal directed methodology. There was no coherent research programme behind the investigations and as a result the role of materials research in the quest for high Tc lacked a theoretical foundation.

Aside from applications based on BCS and Josephson effects it was largely the chemists who had come to dominate the field of superconductivity by experimentally synthesising samples, using empirical reasoning and by taking over the classical techniques of physical measurement. In the end the discovery of high Tc superconductivity arose not out of materials research but out of theoretical speculation done by Karl Muller, a long time IBM researcher who had just joined the elite group of "fellows" at IBM permitted to focus on pure research. In conjunction with a German chemist Johannes Bednorz he began looking for oxides that were potential candidates for strong electron–phonon interactions. What this produced was a kind of hybrid practice that combined aspects of materials together with pure research. Finally, then, can this hybrid practice be classified as application dominated research?

The guiding hypothesis was that as electron-phonon coupling became stronger there would be no free electrons and a superconductor would become an insulator. The existence of these bipolarions (as they were known) was already well established in the materials research, so they began looking for oxides that were potential candidates for these strong interactions. They perfected a method for preparing perovskites (most metallic oxides had this crystalline structure) which are ceramic compounds of several metallic atoms with a non-metallic atom. Their formula ABX_3 describes several hundred materials with all sorts of electrical behaviours which is what makes them materials of choice for electronic components. Some are insulators, some are semi-conductors while others conduct electricity as well as metals do. But perhaps their most important characteristic is that slight modification to their basic structure can radically change their electrical properties. Preparation of these compounds is not easy but they finally succeeded and at 30 K the resistance started to decrease as temperature decreased and went to 0 at 10 K. Part of the sample seemed to be superconducting around 30 K which brought the record for critical temperature upward by 7 K.

Mueller's and Bednorz's paper in *Zeitschrift* (1986) began, ironically, with a quote from American physicists which stated that "the empirical search for new materials is at the forefront of superconductivity research" which they went on to contradict by emphasizing the main theoretical ideas that led to their discovery, specifically the strong electron-phonon coupling as the cause of superconductivity in oxides. They were also able to explain, in a systematic way, why in some cases lowering the temperature in samples reheated in air at high temperature does not result in superconductivity (electron localization was significantly weaker). What their work pointed to was the importance of the interaction between pure theory and materials research that had, until then, been the province of the chemists. This type of collaboration led to an informed search for the kinds of materials that would exhibit qualities that would make them superconducting, unlike the strictly empirical methods of Matthias. Obtaining interesting materials has little chance of occurring as the result of straightforward empirical predictions since in understanding why materials behave as they do requires a certain amount of theoretical expertise. For example, a group of researchers in the materials/crystallography group in Caen developed techniques for studying perovskites but failed to produce superconductivity because the samples had been reheated in air at high temperatures which weakened the electron-phonon coupling. Because of the lack of theoretical guidance the group had no sense of why their efforts failed, yet Bednorz and Muller were able to provide a theoretical explanation for the failure and also avoid this type of mistake in their own materials research.

Materials research without a strong theoretical foundation is no longer the physics of materials but takes on the practices of chemical engineering – synthesizing materials in order to raise the critical temperature of superconductivity with an eye to application. In some sense this is what has happened with a good deal of subsequent high Tc research where arguments based on chemical structure seem to be the tools of progress. That is not to say that such practices necessarily lack any "theoretical" understanding but rather that theoretical understanding of these materials involves a purely technical activity based on well established methods, an activity akin to the calculation of band structure in semiconductors or molecular orbits in chemistry. Neither of these could be appropriately classified as application dominated research; consequently neither should high Tc research.

Although Muller's and Bednorz's work represented an amalgam of pure and applied work, in the end it was theoretically driven and not really representative of ADR. If the goal of defining ADR is to allow for a middle ground between the pure and applied then we need some clearly defined criteria that will enable us to properly differentiate it; but as we have seen in the case of superconductivity this is by no means an easy task. One obvious example that seems to fit the ADR paradigm is the Manhattan project but even in that context many ethically minded scientists described their work as pure research.

Although Bednorz and Muller won the Nobel in 1987 for their work, understanding high Tc superconductivity remains a problem. The failure of BCS to provide an explanation has the field in a state akin to a Kuhnian "crisis". Dozens of explanations for the interaction responsible for superconductivity in cuprates have been proposed, based on the results from all kinds of experiments. But, beyond this a certain number of characteristics of these metals have been established:

(1) The electron pairs are different from Cooper pairs – they have a short coherence length (several nanometers) and a symmetry reflecting the strongly anisotropic crystalline structure. Cooper pairs are spherically symmetric (independent of crystal structure) and a coherence length of roughly 20 nm.
(2) The superconducting properties depend sensitively on the quantity of oxygen in the compound relative to the exact chemical formula. The doping (in oxygen) can be as high as 2%, unlike semiconductors where the doping in impurities is typically one part in a million (so the two are not really analogous).
(3) The electrical properties of oxide superconductors are different from the standard ones because the energies of interaction between vortices can be much smaller than thermal energies. Near Tc the regular network becomes like a pile of intertwined spaghetti (Matricon and Waysand, 2003).

Applications of high Tc have generally been meager. The failure to find stable, long (several hundred meters) wires with millimeter diameters that could be wound round a coil means that there are no high Tc magnets, transformers, turbines, etc. Low current applications have fared better (superconducting electronics), benefiting from the progress in instrumentation for microelectronic circuits (mostly filters for mobile phones). In the end, however, despite these successful low current applications, the methodological practices fail to satisfy the constraints of ADR.

Conclusions

One of the problems in isolating cases of ADR in the field of superconductivity research is perhaps the lack of detailed theoretical knowledge. Despite having a well articulated theory of the general causes of superconductivity the theory suffered from a lack of predictive power; consequently applications were difficult to carry out. But, even in cases of materials research the goal seemed to be filling in the blanks left vacant by theory. The use of SQUIDS resulted in the uncovering of new knowledge but it was knowledge that resulted from using SQUIDS technology in the same way that new instrumentation frequently allows us to uncover new knowledge.

So, what can we say about ADR and the relation between the pure and applied? Does the problem lie with the theory (BCS) itself and its applications, or with the way ADR is categorized? The answer is, I believe, both. In order to identify the middle ground of ADR we need to focus not just on the goals but on the practices and methodology that define it as a separate category of research. And, as we have seen, even in "application friendly" contexts like superconductivity this can sometimes be a difficult task, particularly when the theory itself is somewhat open-ended. While many of the major breakthroughs in the field involved some type of interaction between pure and applied research, isolating a context for ADR has proved remarkably difficult. As a category ADR captures many of our intuitions about the need for some intersection between pure and applied research but problems arise in attempting to match those intuitions to some *systematic* criteria applicable in practical contexts. My intention is not to deny the existence or importance of ADR but rather to highlight some of the problems in disentangling it from pure research on the one hand and applied science on the other. As a category in its own right, defining ADR, like many other attempts to identify a middle ground, turns out to be particularly challenging problem.[4]

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Science in the Context of Industrial Application: The Case of the Philips Natuurkundig Laboratorium

Marc J. de Vries

Introduction

In November 1971, a meeting took place at the Philips Electronics company, in which a group of representatives from the Philips Natuurkundig Laboratorium (Nat.Lab.), the company's corporate laboratory, and a group of representatives from the Product Division (PD) of Radio, Gramophone and Television (RGT) discussed the possible merits of a new type of tube for displaying television images: the index tube. In the Nat.Lab., a group of researchers had spent several years of hard work on this device that could replace the conventional shadow mask tube in television sets. The Nat.Lab. had been encouraged to continue this research by the fact that previous presentations of their work had not raised any objections in the PD, and that they had heard there was a group in the PD's pre-development lab that was also working on a similar tube. This had given them the impression that the PD had a serious interest in their index tube. What happened at the meeting therefore came as a complete surprise to them. After they had demonstrated the tube, the technical director of the PD immediately applauded the work of the Nat.Lab. group. The commercial director, though, responded by claiming that the tube was not interesting from a commercial point of view, because it worked well only for smaller sized images. The Nat.Lab. group reacted to this by pointing out that most of the Japanese television sets had small images. "Then we will teach those Japanese to appreciate large images", the commercial director answered, and closed the discussion with that.[1]

When this story was told by one of the Nat.Lab.'s former directors,[2] his frustration about this event was still notable. In fact, the whole Nat.Lab. group had been

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[1] This story was told to the author by two people who were involved in the discussion, namely Dr. E.F. de Haan (interview on September 9, 1997), former director of the Nat.Lab., and Mr. B. Kaper (interview on March 3, 1998), former director of the Product Division Radio, Gramophone and Television (RGT).

[2] For exact references, see de Vries (2005), 137.

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outraged after the meeting. But on the PD's side there were frustrations too about the enormous waste of effort in the Nat.Lab. The PD's pre-development lab had terminated the work on the new tube when it had become clear that there was a new trend in the Japanese television market towards larger images. Besides that, the PD had decided that it was not wise to have two competing products with the same function in their portfolio. They had expected the Nat.Lab. to have picked up their signals about this, but evidently the Nat.Lab. had just continued the work on the index tube in spite of that. As the PD never had to contribute financially to the Nat.Lab. research, they had no formal influence on the Nat.Lab.'s research programme. Their comfort was that at least the waste of financial resources did not weigh on the PD budget. Still, it seemed like a shame to them that company money was spent on work of which it as known beforehand that they would never make use of it.

This anecdote is one of the many examples of mutual frustrations between the Philips Natuurkundig Laboratorium and the various Product Divisions in the Philips company in the 1950s and 1960s. In the history of the Nat.Lab.,[3] this was in particular the period in which the lab thought of its role in the company in terms of what we now call the "linear model". According to this model, technological innovation takes place in a sequence of phases, of which the first one is called "fundamental" or "basic" research. This model had been "preached" by Vannevar Bush in the USA, in the famous "Science: The Endless Frontier" report, which he had written as an advisement to the president to indicate the most desirable way to proceed for the post-WWII development of science and technology (Bush, 1945). The report is often seen as the starting point of a period in which the linear model was practiced in the USA, but also in other countries, although it can be questioned if it was really this report that had that effect.[4] After some decades, doubts about the success of the linear model started to emerge and today it is no longer seen as the ideal for technological developments. Still, terms like "fundamental" and "basic" can be found in, for instance, the European Framework Programmes and in the OECD Frascati Manuals.[5] The history of the Philips Nat.Lab. sheds some interesting light on the reasons why confidence in the linear model waned. The anecdote already gives a clue for that. The Nat.Lab. had expected that the index tube would follow a route in which they would do the basic research that would lead to a working prototype of the tube, after which the invention was to be transferred to the PD's lab in which it would be elaborated into a mass-producible device, and then introduced to the television market by the PD's commercial department. But evidently it did not work like that, and the index tube is indeed not an exceptional case in which the linear model did not fulfil its promises. The history of the Nat.Lab. shows that the practice of the

[3] de Vries (2005) describes this history. This contribution to the "Science in the context of application" project is largely based in this book. I will not mention further detailed references to the book, as all examples can easily be found through the book's Table of Content.

[4] Edgerton (2005).

[5] OECD (1963, 1970, 1976, 1981, 1994).

linear model and the motives for using terms like "fundamental" and "basic" were not always the same as they were in theory. In this chapter various issues related to the emergence and decline of the linear model will be discussed by using the history of the Philips Nat.Lab. as an empirical resource. We will also see that the idea of a transition from Mode-1 to Mode-2 research, as Michael Gibbons has described it,[6] is an equally strong simplification of reality. The history of the Nat.Lab. will show how scientific research in the context of industrial practiced can be described in a more proper way than the linear model and the descriptions of Mode-1 and Mode-2 research suggest.

From Seamless Web to Ivory Tower

The first decades in the history of the Philips Nat.Lab. indicate that the linear model is by no means the only option for an industrial research laboratory of operating successfully in its mother company. The whole dynamics of scientific research and technological development was very different in the pre-WWII years of the Philips Nat.Lab.'s history. The impression we get from both oral and written sources about this period is that the lab served in a seamless web of various activities in the company, and that this was an appropriate model for that period.

The main motives for initiating a research laboratory in the Philips company were the protection of the company's position in the light bulb market and the extension of the product portfolio to spread the market risks. Philips had been founded in 1891 by two Philips brothers, Anton and Gerard. Soon the two developed a working relationship in which Anton took care of the commercial issues and Gerard was primarily involved with the technology of their sole product, the light bulb. Soon a small chemical laboratory was created to perform tests on light bulbs for quality assurance. In that lab also chemicals for filaments were prepared. In 1914 the Philips brothers decided to start a second lab for different tasks, among which the study of phenomena related to the functioning of light bulbs, in the hope that such research would yield ideas for improvement of that product. Gilles Holst was appointed to set up this laboratory. In Holst's own words: "I am to be given a whole laboratory to equip, and I shall carry out all manner of investigations that will teach us the formula of the incandescent lamp".[7] Holst's ambition was to acquire better insight into the phenomena that made the incandescent lamp function. Such knowledge could lead to improvements of the product. The same knowledge could also provide a basis for developing other products that were based on the same phenomena. Having those other products in the company's portfolio would create possibilities for survival when the light bulb market would decrease or when competition would become even more severe. The study of the various components of light bulbs,

[6] Gibbons et al. (1994).

[7] The quote is from a letter written by Holst to A.D. Fokker dated November 30, 1913. The English translation is from Heerding (1988, 314).

namely glass, vacuum, filaments, inert gases, and properties of light, also applied to a variety of tubes. The difficulties in acquiring X-ray tubes from Germany in WWI created an opportunity for Philips to develop their own X-ray tubes, and the research into the mentioned phenomena was useful for that. Also the amplification tubes in radio transmitters and receivers were based on the same phenomena, and this clearly was a substantial market opportunity for the company. Once thus having become involved in radio, the Philips brothers decided to extend the scope even further into complete radio sets. Holst and his researchers were called in to realise that new ambition too. As a result, the Nat.Lab. started working on new topics such as electrical circuits and acoustics. These new knowledge domains brought about new opportunities for further extension of the product portfolio. Thus the company moved into fields such as telephony and later also television. The pattern that developed was one in which the company directorate (the Philips brothers) communicated directly with the research lab in a reasoning that constantly moved back and forth between products and phenomena. The study of phenomena related to products that were in the portfolio already created options for new products, and getting involved in those new products created the need to study other phenomena that were related to the new products, which in turn created new options for extending the product portfolio, etcetera.

This pattern clearly differs from the linear model in a methodological and an organisational respect. Methodologically, the development of a product is not based upon the scientific study of a hitherto unknown phenomenon that was never before used for the development of a product. This would have been the case in the linear model. Here there is a cycle of reasoning from product to phenomenon and from phenomenon to (another) product. Organisationally, there was a continuous contact between the researchers and the company directorate. Also there were continuous contacts with the factories. An example of this was the procedure for development and production of new radio receivers. This procedure entailed a continuous back and forth movement of the design between Nat.Lab. and the apparatus factory. This, too, is quite different from the linear model in which there is only a one-way transfer between research, development and production.

Although the way the Nat.Lab. functioned in the Philips company in the pre-WWII years differed from the linear model, some elements in the Nat.Lab.'s practice seem to have served as preparatory for the post-WWII years in which the lab changed its ambitions in the direction of the linear model. In the first place there is Holst's conviction that an academic climate was necessary for his lab to perform its task. To realise such a climate Holst organised seminars in which the best physicists of that time presented their ideas. One of the frequent speakers was Paul Ehrenfest and even Albert Einstein once spoke at such a seminar. There was, however, no clear relationship between the topics they addressed and the lab's research programme. It seems as if Holst's purpose was not in the first place to get the most up-to-date information about scientific theories on relativistic and quantum phenomena, but rather to stimulate his researchers to grow in their scientific thinking to a level on which they could communicate with those famous physicists. Furthermore, Holst stimulated his researchers to publish in scientific journals. For that purpose he initiated a journal that was published by the lab itself: the Philips Technical Review. But the researchers also published in other scientific journals. Soon, however, a tension became evident that is inherent to industrial research. For publishing in scientific journal it is important to be the first researcher to present findings and theories. But often it is in the interest of the company first to protect the outcomes of research by patenting. In the Holst period the decision was made to prioritise patenting. This did not lead to great problems, but in a linear approach this tension can be expected to be a cause of conflict. The fact that even in the 1950s and 1960s no such conflicts were reported are a first reason to doubt if a linear approach was really practiced in that period. Later we will see more reasons for doubting that.

Another element in the lab's practice that has analogies to the linear model is the substantial contribution that was made to certain scientific areas. The practical context in which the Nat.Lab. scientists worked did not make them fail in making serious scientific contributions.[8] For instance, two of the researchers in chemistry, A.E. van Arkel and J.H. de Boer, published a book on chemical bonds as an electrostatic phenomenon that was to become well-known in their field. This was an interesting combination of authors, because they were quite different in their interests. Van Arkel was primarily intrigued by the theoretical aspects of the chemical bonds, while De Boer was more interested in the industrial aspects of his discipline. In this case the cooperation of the two worked out well, as the publication of the co-authored book indicates. There were, though, similar duos of researchers that were less successful in their work together. Willem Elenbaas and Cornelus Bol cooperated in developing mercury lamps. In this case, Elenbaas was most interested in theoretical aspects, while Bol was mostly interested in practical affairs. A conflict between the two arose when both started to make claims with respect to being the real inventor of the devices that emerged from their research. This competition between theoreticians and practitioners is a second tension in the lab's practice that can arise in industrial research. Unlike the first mentioned tension (that between publishing and patenting), though, it can be questioned whether it is inherent to industrial research. In the Nat.Lab. history we find it mainly in the pre-WWII years, when the lab's number of employees was still growing. It may well be a remnant of the earlier period of individual inventors (such as Edison) in which individuals could profile them selves. In the post-WWII years, the lab had become so large that this was much more difficult.

The phenomenon of individuals trying to profile them selves is related to a third tension in the practice of industrial research, of which we find the beginnings in the Holst period of the Nat.Lab., namely that between the freedom of the individual researchers and the need to manage the research programme. Although the impression we get is that Holst did not try to steer individuals in certain directions, he did have explicit ideas about the desirable scope of his lab's research programme. For was, for example, against doing much work on television, because in the 1940s he considered this not to become a mass consumer product in the near future, and as a

[8] Wilholt (2006) describes another example: the Giant Magneto Resistance (GMR) effect.

result not much effort was spent on television until after WWII the company directorate forced him to increase this effort. On the other hand, one individual researcher, named Albert Bouwers, was able to shield his group working on X-ray tube from Holst's managerial influence for several years. To his annoyance, Holst at a certain moment found out that Bouwers even had direct contact with Anton Philips without ever having informed Holst about this. This was, though, an exception, and typically the tension between individual freedom and management of the collective was more theory rather than reality in Holst's time.

What we have seen so far is that the linear model evidently was not a necessary condition for an industrial laboratory, neither for making serious contributions to science, nor for serving a useful role in the development of new products, at least in the conditions of the pre-WWII decades in Europe. This conclusion can be extended to the USA, because studies similar to the one concerning the Philips Nat.Lab. dealing with some of the USA's major industrial labs (e.g., Bell Labs and GE Labs[9]) show that in those labs there was a situation quite similar to the one that was described here for the Nat.Lab. There too, we find the beginning of certain tensions that were still fairly unproblematic in this period, but would become more pressing when the linear model was adopted, at least in theory, by the industrial research labs.

The Pains of "Fundamental" Ambitions

Soon after the ending of WWII, we find a change in the Nat.Lab.'s research strategy. Suddenly terms like "fundamental research" and "basic research" frequently feature in high-level management debates about the lab's research programme. This naturally reminds us of the Vannevar Bush doctrine. "Science: the Endless Frontier" had only recently been published. This report itself may not have been as influential as has been claimed in the past, but it can hardly be imagined that the increased popularity of terms such as "fundamental research" and "basic research" had nothing to do with it. These terms inherently have some relation with the idea of a linear relationship between science and technology, although maybe not as simplistic as much of literature about the "linear model" has suggested.[10] Whatever the exact cause may have been, a number of large industrial research labs in the USA, e.g. Bell Labs, did start research groups for that type of research. One is tempted to conclude that the emergence of the linear model in the USA was also the cause of the emergence of the mentioned terminology in the Nat.Lab. and that the 1950s and 1960s can be seen as a period in which the linear model was practiced. This,

[9] See Reich (1985).

[10] Edgerton in my view too easily discards the existence of the linear model by claiming that linearity is not explicitly found in the policy documents of both industrial and public research. It cannot be denied that the very notion of "fundamental" or "basic" suggests that something can only be built once a foundation has been laid. This does suggest linearity, and therefore the frequent use of the terms "fundamental" and "basic" does entail a certain popularity of the linear model.

though, must be questioned, and a more precise observation of what went on gives us new insights into the science-technology relationships in the context of an industrial research laboratory.

To get a good insight into the reasons for the Nat.Lab. in starting to use the terms "fundamental" and "basic" as characteristics for important parts of its research programme, we have to know the background of the lab's changing position in the Philips company in the first post-WWII years. In these years important changes took place in the company. A new structure was formalised, in which the company consisted of a company directorate, a series of Product Divisions, and a couple of corporate organisations, among which was the Nat.Lab. The company directorate no longer consisted of just the Philips brothers, but became a formal body with (initially) nine members. The Product Divisions had grown out of the factories, and now were given certain autonomy in determining their own product portfolio. Besides that, they also had their own laboratories. This changed the position of the Nat.Lab. dramatically. Until then they had been the company's main technological knowledge resource. Now they were only one among many places where technological knowledge was developed. This posed a new question to the Nat.Lab. What was to be their added value to the company? Was there a possibility to be still unique in some sense, as they had always been before? In the context of that situation the linear model, as hinted to in terms like "fundamental" and "basic" research, came as a welcome idea. From the very start it was clear that the PD labs were focused on research that was directly linked to the (further) development of products. There was no PD lab that would ever want to claim a role in what was called "basic" research. The Nat.Lab. managers concluded that this was an excellent option for again creating a unique position for themselves in the company. They were to be the only lab for "fundamental" or "basic" research at Philips. A welcome characteristic of that type of research was also that it would enable the Nat.Lab. to avoid PDs from meddling with their affairs, because the secret of "basic" research was that it was supposed to be entirely free from concerns about the commercial value of the ideas that emerged from this research, as well as from all other sorts of practical concerns. Researchers involved in "basic" research were to be left undisturbed by the PDs' day-to-day concerns in order to be free in the selection of research topics. The promise of long-term relevance of such free research for the company was often used to emphasise the importance of having a substantial part of the Nat.Lab.'s programme dedicated to "basic" research.

At first sight it may seem that indeed the linear model was implemented. In the highest management meetings, the Company Research Conference (CRCs) new scientific developments were always the first topic on the agenda. These meetings were chaired by Hendrik Casimir, one of the three successors of Holst who was soon to become the primus inter pares and later the research representative in the company's Board of Management. Casimir himself had been active in quantum mechanics and a good friend of several of the world's best-known physicists, such as Bohr and Pauli. He was particularly keen at discussing the option of starting work on some new phenomena, like superconductivity and superfluïdity, for which there were by far no ideas about possible applications yet. The same holds for laser physics, because at that time the laser only served in research laboratories as a source of light with interesting properties. Contrary to the way Holst used such fields as only a stimulus for an academic climate in the lab, Casimir mostly argued that in some way or other those phenomena might well be applied in new products that would give the company a long-term advantage over companies that had limited themselves to using more traditional knowledge.

The claim that it was primarily the need to guarantee a legitimate position in the company rather than a belief in the merits of the linear model that had made the Nat.Lab. choose this new direction, can be supported by investigating the practice of the lab's research in this period. It appears that in a number of ways this practice deviates from what one could have expected from a "basic research" laboratory. In the first place Casimir's search for new phenomena, only a few cases displayed an actual uptake of research into those phenomena in the lab's research programme. Even the laser research, although already linked to a device (the laser itself) was abandoned after some years, just like research into the other new phenomena. Secondly, the linearity is missing in two of the most successful projects of that period, namely the Plumbicon (a television camera pickup tube) and LOCOS (LOCal Oxidation of Silicon, a technique to produce flat-surfaced integrated circuits). Different as the two stories may be, they both lack the linearity of the linear model. In the case of the Plumbicon, knowledge of solid state physics was used to find a solution to a practical and well-defined problem. RCA had invented a television pickup tube that was based on the phenomenon of photoconductivity. Until then such tubes were based on photoemission. But the tube displayed certain problems (it did not respond adequately to rapid changes in the image and there was a "dark current" when no light fell on the tube's target), which according to RCA were inherent to the phenomenon of photoconductivity. The Nat.Lab. researchers, however, did not believe this and used their knowledge of energy band structures to select a limited number of probable options for alternative target materials out of the many that were available. Having made this selection it did not cost many experiments to find out that lead oxide was the best candidate. With this new target material the pickup tube functioned properly and soon became the world standard for television pickup tubes. The knowledge that had been used had not been developed for this specific purpose, which is in accordance with the linear model. But it had certainly not been this knowledge that had triggered the invention, but a particular problem in an already existing device. The Plumbicon case therefore does not illustrate the linear model. What we do see is that scientific theories can serve as heuristic means in the search for solutions to a technological problem.[11]

A quite different story is the invention of LOCOS. But here, too, we find evidence that the idea of "fundamental" research was not practiced as it was preached. LOCal Oxidation of Silicon is a process for making Integrated Circuits (ICs) that have very flat surfaces. In LOCOS a layer of silicon nitride is used to shield a silicon substrate from being oxidised at specific places. At those places where the silicon

[11] Kroes (1995).

does oxidise, the silicon oxide layer was found to sink halfway into the silicon substrate, which produces a nice flat surface. The shielding property of silicon nitride was found by accident when a Nat.Lab. researcher, Else Kooi, had been working on depositing a silicon nitride layer onto a silicon substrate by inserting a silicon oxide layer between the silicon and the silicon nitride. He discovered that the silicon did not oxidise under the silicon nitride. He then realised that the silicon nitride could be used as a mask in IC production. This started a period of research aimed at optimising this process by eliminating a number of undesired side-effects. Kooi's research group was part of the "devices" department in the Nat.Lab. The two other main departments were "materials" and "systems". After the process was optimised and a patent was acquired, it was found out that in the "materials" department a "fundamental research" group had been working on surface chemistry and had produced outcomes that would have been very useful for the LOCOS research. Although it is not certain, it is improbable that this group had never heard about the LOCOS activities, but they had never contacted the LOCOS group. So the success of LOCOS was by no means an example of the validity of the linear model, because there had been concrete possibilities to transfer outcomes of "fundamental" research to the LOCOS research, but this had not been done.

The Plumbicon and the LOCOS patents were seen as the "crown patents" of the 1950s and 1960s, because both have brought in great incomes due to the fact that the whole market was dominated by these inventions for several decades. But as we saw, neither of them can be regarded as the outcome of a linear approach. There is one more indication that the linear model was rhetoric rather than reality. This is the fact that a number of research projects were executed without any influence of PDs, while it can be questioned if they were "fundamental" in the sense of research that is focused on phenomena without concrete expectancies for application. An example of such projects was the Stirling or hot air engine. The Stirling engine research had started just before WWII and was an effort to develop a source of electrical energy that could function independently from the electricity net. It was based on an elegant thermodynamic process in which a gas (initially air, but later other gases were used) was compressed in a cold space and expanded in a hot space. This cycle resulted in a transformation of heat into motion. This research continued until the 1970s in spite of the fact that no PD had shown any interest in it and all efforts to transfer the research outcomes to a PD lab had failed. Yet, the research was totally focused on optimising the engine and not on acquiring any new "fundamental" knowledge of the thermodynamics underlying it. In spite of the fact that in certain years the Stirling engine group was one of the largest in the Nat.Lab., no commercial activity was ever developed. Of course the linear model does not guarantee that all fundamental research leads to application and commercial success, so in itself the fact that the Stirling engine research never reached the stage of commercialisation does not disprove the linear model. But in this case the research can hardly be claimed to be fundamental, because it was not focused on getting to know better a phenomenon, neither was it separate from concrete products. Yet, it was regarded to belong to the Nat.Lab.'s task, which in that period was seen as being primarily the "fundamental research" lab within the company.

Shouldn't these examples make us doubt whether the linear model was even promoted in theory at all? Perhaps so, if it were not for the fact that Casimir had been quite explicit about his ideas on the role of the Nat.Lab. as a lab where fundamental research should be done in the first place. In his book *Haphazard Reality*,[12] he describes what he calls the science-technology spiral as a model for the relationship between science and technology. According to him, in the twentieth century a new dynamics arose in which most technologies depended much more on scientific knowledge than before, in particular in the use of knowledge about atoms and electrons. He claimed that his model was more sophisticated than the linear model as it also entailed the stimulus for new scientific research emerging from technological inventions (hence the "spiral"). Still, it does emphasise that "fundamental" scientific research is necessary for technological innovations, in particular for those fields in which the Philips company was active. One could say that the line had been wound up to a spiral, but still the linearity was there. Casimir put this into practice in the agendas of the highest level management meetings that he chaired. Always the first issue on the agenda was: are there any new developments in science, new phenomena that are studied, that could be of importance for the Nat.Lab.? Clearly, he saw new scientific developments as a key issue for these meetings, more than the question as to which were the research requests coming from the PDs.

Of course transfer of research output to PDs did happen in this period. But even when this occurred it was not unproblematic. Again the Plumbicon can be mentioned as an example of that. Once the design had been completed, it was transferred to the PD for further development into a mass-producible artefact. This appeared to be a much more complex matter than the Nat.Lab. had anticipated. All sorts of practical problems emerged when the tube started to be produced in larger quantities. Speckles in the image, imploding glass bulbs, and various other problems plagued the factory. The problems even became so serious that the project had to be taken back by the Nat.Lab. to work on redesign of the device. In the second transfer effort, one of the Nat.Lab. technicians was transferred to the factory to provide assistance in solving the remaining production problems. This was by no means the only case in which the PD made a complaint about the Nat.Lab.'s lack of awareness of possible problems in putting the design or prototype into mass production. This irritation of course added to the already existing annoyance about the Nat.Lab.'s disrespect for the PDs research requests.

Although today many Nat.Lab. researchers still tend to see this period as a highlight in the lab's history, it was by no means unproblematic. The Nat.Lab.'s claim for independence in choosing research topics caused a mutual lack of commitment between the Nat.Lab. and the PDs. As a result there were mutual frustrations, as described in the introductory anecdote at the beginning of this chapter. The Nat.Lab. felt that PDs often failed to recognise the potential of the research output for developing very innovative products, and the PDs felt that they had no say whatsoever in the Nat.Lab.'s research programme. Besides that, the Nat.Lab.s budget was allocated

[12] Casimir (1983).

directly by the company's Board of Management, so that the PDs felt even freer to take research output for granted because they had not paid for it anyway. It was this very independence of "fundamental" research in the linear model that seriously hampered its functioning within the company. It not only caused a lack of mutual commitment between the research organisation and the PDs, but also resulted in a way of thinking in the Nat.Lab. that failed to recognise the difference between making one prototype function and designing a product that can be produced in large numbers. Those innovations that were most successful, the Plumbicon and LOCOS, had not happened according to the linear model. From this we conclude that the linear model existed in theory and in rhetoric, but was never successfully put into practice.[13] We do see the use of scientific theories in technological developments, but in the sense that science provides heuristic means for product development (as was illustrated by the Plumbicon case). The linear model appeared to have primarily a function in claiming independence by the Nat.Lab. rather than being an ideal for determining the lab's research agenda.

Finalization as a Feasible Alternative?

By the end of the 1960s financial problems started to emerge in the Philips company. This was not unique for Philips. The early 1970s were a period of transition for many industrial corporations. It was not only the economic stagnation that caused this transition, but also the growth of social critique on science and technology. It was felt that society should have a say in the development in science and technology. This, of course, was not consistent the idea of free, independent "fundamental" research. For Philips it became clear that the Nat.Lab.'s independence had resulted in a couple of "big hits", but it was questionable if the necessary reduction of its budget would allow for this independence to continue if the percentage of research projects resulting in such "big hits" remained the same. What would happen if a two-decade period of research would not lead to two "big hits" (Plumbicon and LOCOS), but only one? Would that still compensate for all the unsuccessful research projects (such as the Stirling engine project)? Such consideration led to a new period in the Nat.Lab.'s history that I have characterised elsewhere as "the road towards mutual commitment".[14] The end of this road was the introduction of contract research in 1989 by the company's Board of Management. The Board had made this decision when it had become clear that all previous efforts to create commitment by stimulating contacts between the Nat.Lab. and the PDs had failed. It seemed that financial measures were necessary to force the Nat.Lab. to tune the research programme to the PDs' needs. On the road towards this point we meet an interesting phenomenon

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[13] So I concur with Edgerton's doubts about the reality of the linear model, not because it did not exist in theory, but because it was never really put into practice (at least, not in the Philips company).

[14] de Vries (2005), Part III.

that reminds of the concept of “finalisation” as developed by the Starnberg group in the 1970s: the Transfer Projects. They were the Nat.Lab.’s ultimate effort to prevent the introduction of contract research. The concept of finalisation has been a topic for discussion in the late 1970s and 1980, but in the end the interest waned because the model was seen as insufficiently different from the linear model. The failure of Nat.Lab. in using Transfer Projects to keep away contract research offers an interesting empirical counterpart for the theoretical critiques on the concept of finalisation.

Initially, the Nat.Lab. and the company’s Board of Management had hoped that the effectiveness of research in the Nat.Lab. could be enhanced by creating more formal opportunities for exchanging ideas between the Nat.Lab. and the PDs. For this purpose special meetings were set up in which representatives from both parties would meet. In the “R-PD Management Meetings” Nat.Lab. managers met with PD managers. There were different series of R-PD Management meetings for different PDs. In the Review Meetings, the Nat.Lab. directorate met with representatives from the company’s Board of Management to discuss the relationship between the Nat.Lab.’s research programme and the PDs’ interests. These meetings were stimulated strongly by Casimir’s successor, Eduard Pannenborg. Pannenborg was an engineer and perhaps already because of this background was more interested in contacts with PDs than Casimir with his background in theoretical research in quantum mechanics had been. But in spite of all the good will on both sides, these contacts did not yield the results that had been hoped for. It did not create any formal commitments other than promises on paper, and there was no real incentive for the Nat.Lab. to use the PDs’ requests as guidelines for defining the lab’s research programme. Then the Nat.Lab. management came up with an idea for a new type of project in which a phase of transition was created for certain research topics. They were called Transfer Projects, and about a year before contract research was introduced, Pannenborg’s successor, Van Houten, announced that no less than 25% of all the efforts of Nat.Lab. were to be spent on the Transfer Projects. The Transfer Projects in a way were an alternative for the Geldrop project centre that had existed since 1963 and was a Nat.Lab. research premise in a village near Eindhoven. In that centre projects were carried out in which always other parties were involved, in many cases not only one or more Philips PDs, but also external parties. In the 1970s and 1980s this project centre played a useful role in learning how to get other parties interested in and committed to Nat.Lab. research initiatives. An example of this was the optical communication system for the city of Berlin. This project was done by the Nat.Lab. in cooperation with the PDs Glass and PTI (Philips Telecommunication Industries), and with Felten & Guillaume and TeKaDe, two German telecom companies. In 1978 the system was implemented and was an example of a quite satisfactory cooperation between the Nat.Lab. and other organisations. In 1990 the centre was dissolved and its activities were continued at the Eindhoven Nat.Lab. premises and extended to the Transfer Projects.

The idea behind the Transfer Projects was explained by its inventor, Piet Kramer, one of the lab’s deputy directors. In his experience the transfer of research output was often hampered by the fact that the researchers kept working on improvements of the product before handing it over to the PD. A Transfer Project committed the researchers to "freeze" the product and consider it as finished in the sense that from then on transfer was the only focus for the researchers' activities. The product was then said to be "in state of transfer" and no longer in development. This bears resemblance to the idea of finalisation, as developed by the Starnberg group. Their model was meant to be an improvement of the linear model. In their view there are three phases in the research for innovation: a preparadigmatic phase in which there is not yet one generally accepted theory, a paradigmatic phase, in which one theory becomes dominant, and a postparadigmatic phase, in which the theory is considered to be fully developed and further research is now aimed at extending the theory in the direction of practical applications ("finalization").[15] One could say that research being transformed into a Transfer Project was considered to be in its postparadigmatic phase.[16] In a Transfer Project the research was "frozen" in the sense that no more new knowledge was developed.[17] As in the concept of finalisation, the Transfer Projects were seen as a separate type of research and not merely as the application of previous work. After transfer a PD would then take up the actual finalisation, which again was seen as a separate type of research, done in a (PD) laboratory and not in a development group. Initially, the Transfer Projects seemed to work out well. The idea was put forward by Kramer in 1987, approved by the Board of Management later in that year and in the next year already 72 Transfer Project proposals had been outlined. Later that year, however, it became evident that only few of the proposals really made it to practice. In June 1988, only four of these projects had the status of "running, and documents signed by managing directors". All of these projects were for one and the same PD (Consumer Electronics). But no less than 133 research people and 147 PD people were involved in these projects. This indicates that it was a major effort. Besides that, another 22 projects had the status of "Running, documents agreed and signed by (deputy) directors and ready for signing by managing directors". These projects were for seven different PDs. Most of them were small projects, though. One of the largest ones was the Mega Project, a cooperation with the Elcoma (Electronic components and materials) PD aimed at developing 1-Mbit Static RAM ICs. The details of this project show why the Transfer Project did not fulfil their ultimate aim (preventing the introduction of contract research). In spite of the fact that in these projects both Nat.Lab. researchers and PD researchers were involved, the old problem of commitment was not resolved. This project was led by Roel Kramer (no relation to Piet Kramer, the "father" of the Transfer Projects). According to him, only part of the PD management was interested in the project. No concrete applications for the SRAMs had been defined; neither were there any concrete production targets set in the agreement. At a certain

[15] Böhme et al. (1983).

[16] The term "postparadigmatic" as such was not used, of course.

[17] The term "frozen" was used by Dr. K Bulthuis, former director of the Nat.Lab., in an interview with the author of this chapter on November 18, 1997.

point the uncertainties were so great that the project had to be terminated abruptly (some people were even fired on the same day they were hired!).

The drama of the Mega project showed that there was more to a Transfer Project than just the idea of a certain “ripeness” of research output for “finalisation” by transfer. It still leaned too much on the idea that the research in the Nat.Lab. was a phase that necessarily preceded the work in the PD lab.[18] Even though the idea of Transfer Project was more subtle than the linear model, it still had this phase in which there was no PD commitment in the research activities, and this lack of commitment lingered on in the Transfer Projects. For that reason, contract research was introduced. From 1989 on, the Nat.Lab. had to acquire two thirds of its budget by offering research contracts to PDs. This meant that they were forced to do only research for which PDs really had an interest to such an extent that they were willing to pay for it. Only one third of the lab’s budget could be spent on non-PD commissioned research. Part of this was spent on activities supporting the contract research activities, and part of it was dedicated to what was previously called “fundamental” research. But that term did not feature that much any more in the management debates after 1989. It was seen as belonging to an abandoned paradigm. A new era had begun.

Mode-2 and Interdisciplinarity as New Ideals

In 1994 Michael Gibbons published a book (co-authored by Limoges, Nowotny, Scott, Schwarzman and Trow) in which he introduced a terminology that was to become widely spread in the world of both academic and industrial research: Mode-1 and Mode-2 research. Gibbons described Mode-1 research as the traditional way of developing knowledge in the context of science as a context of its own right. Mode-2 research then was described as the development of knowledge in the context of a practical problem.[19] In fact, that was precisely what contract research in the Philips Nat.Lab. was supposed to stimulate.[20] By forcing the lab to acquire two thirds of their budget from the Product Divisions by offering them contracts for specific research projects, the idea of the Philips management was to make the Nat.Lab. tune its research towards the actual needs of the PDs. No PD would pay for research of which they did not see the relevance, and thus the Nat.Lab. would have to listen carefully to the needs as expressed by the PDs themselves. For accommodating the process of negotiating, both in the research lab and in each PD persons were appointed to be in charge of the contacts between the Nat.Lab. and that PD.

[18] Böhme et al. also insisted that finalisation was science-driven, not society-driven, as Forman (2007) showed. In the case of the Nat.Lab. the Transfer Projects were seen by the PDs as Nat.Lab.-driven and not PD-driven, and for that reason faced problems of transfer similar to those in the 1950s and 1960s.

[19] Gibbons.

[20] The term Mode-2 was not used, of course, as it did not yet exist.

This new mechanism for funding the Nat.Lab. had a great impact on the role of the lab in the development of new products. As we saw before, in the Holst period the Nat.Lab. was the only place where the necessary knowledge to develop a new product was present, and therefore the Nat.Lab. was a leading actor in all new product development. In Casimir's period, the Nat.Lab. was the initiator of some very successful products. But in that period there were also many successful products developed in the PDs without any Nat.Lab. involvement. The compact cassette is perhaps the most important example of such a product. This remained pretty much the situation until the introduction of contract research, although numerous efforts had been made to decrease the lack of commitment that had caused so many frustrations in the Casimir period. In the period following the introduction of contract research the role of the Nat.Lab. became that of being provider of specific knowledge on demand by the PDs. Perhaps the best example to illustrate this is the development of the Compact Disk, and the way that differed from the development of what can be called its technical predecessor the Video Long Play disk.

The development of the Video Long Play started in the Casimir period of the Nat.Lab. The idea was to store video information optically on a disc. It was a Nat.Lab. initiative, in which later PD developers became involved. In the project a variety of disciplines was involved. Laser research was part of it, as well as research into optical and electrical signal processing and mechanical engineering for realising a high-precision mechanism for rotating and reading the disc. Piet Kramer, who later came up with the idea of the Transfer Projects, was the project leader for the VLP. It was a race against competitors, in particular RCA, who worked on a similar system. It was quite a challenge to reduce the price of the laser, as this device had only been used for laboratory applications until then and price had never been an issue. But in the end the technical realisation was accomplished and a working product was demonstrated in 1972. Some competitors were able to demonstrate their devices at around the same time (RCA, Teldec, MCA and Thomson). Yet the market appeared not to be interested in this type of device. At least to some extent, that was due to a lack of software (discs with content that appealed to the market). By 1986 the name VLP had totally disappeared from the Nat.Lab. research programme. In the meantime, however, a new idea had emerged in the PD Audio, namely to use the optical recording technique for developing a disc with audio rather than video information, and in a digital rather than an analog format. From 1978 on the PD started working on this digital medium. For realising this new idea, the PD called in the Nat.Lab. expertise in optical recording, that had been gained in the VLP work. Also the Nat.Lab. was commissioned to bring in their knowledge about digital signal coding, that had been gained in telecommunications research. But the PD remained in charge of the whole effort. Thus we see a shift from the VLP to the CD in that the latter project was primarily a PD initiative with specific input from the Nat.Lab, whereas the former had been primarily a Nat.Lab. initiative with later assistance by the PD. As the CD became a big success, whereas the VLP had been a big failure, it seemed that the success of product development was not guaranteed in cases where the Nat.Lab. had taken the initiative, while the CD as a PD initiative proved that this way of working had a better chance of success. Contract research can be seen as this latter road. In contract research the initiative for product development was in the PD, and the Nat.Lab. was to give specific contributions by offering research and expertise in certain fields.

In Gibbons' description of the nature of Mode-2 research two characteristics are seen as inherently related, namely the practical context that triggers the research, and the transdisciplinary nature of that research.[21] Transdisciplinarity is a term Gibbons uses to indicate knowledge that transcends the boundaries of traditional disciplines.[22] The example of the CD illustrates the importance of combining a variety of different disciplines. Jürgen Lang in his dissertation on the CD development claimed that without the presence of that variety of disciplines in the Nat.Lab. Philips would probably not have been able to develop the CD.[23] In this example the Nat.Lab.'s transdisciplinary nature fit well with the idea of Mode-2 research (as formalised and practiced in contract research). From the Nat.Lab. history, though, it can be questioned whether the connection between the practical need as a driving force behind Mode-2 research and its transdisciplinary nature is as tight as Gibbons suggests in his definition of Mode-2 research. In his description of Mode-1 research monodisciplinarity features as one of the characteristics. Most research in the Casimir period can be described as Mode-1 rather than Mode-2 research as it was usually the Nat.Lab. picking up a scientific interest and working that out into a product or prototype. Was that research monodisciplinary? Not in the case of the VLP, which was clearly not commissioned by any PD, but purely a Nat.Lab. initiative. Also if we look at the way Casimir stated his ideas of industrial research for the Nat.Lab. it seems that Mode-1 is not always that monodisciplinary. Casimir did not bring these ideas forward as his own, but called them the "Holst rules" as if it had been Gilles Holst's guidelines for managing the Nat.Lab. In my book "80 Years of Research at the Philips Natuurkundig Laboratorium 1914–1994" I have argued that these rules had not been Holst's ideas, but Casimir's own ideas projected onto Holst. One of these rules (rule #6) was: Do not split up the laboratory according to different disciplines, but create multidisciplinary teams. And indeed, in the Casimir period the organisation of the lab was not in terms of disciplines, but in terms of levels of the products. There were three main divisions, namely Materials, Devices, and Systems. In all three divisions researchers of different disciplinary backgrounds worked together. The VLP team was an example of that. But multidisciplinarity was also seen as valuable in the Holst period, and in that respect what Casimir formulated as Holst rule #6 was one of the few that actually fitted with Holst's ideas. Holst used very practical and often informal instruments to make researchers of various disciplines work together. For instance, he stimulated that all researchers would have lunch in the same canteen (the lab was still of a size that allowed for that) and thus

[21] Gibbons et al. (1994).

[22] In fact Gibbons gives a more complicated description with four distinct characteristics, but it boils down to this.

[23] Lang (1996).

got to talk with one another about their work. His expectation was that new fruitful ideas would emerge if someone from another discipline would give thoughts about the work of a certain group working on a new product. Perhaps one could argue that in the Holst period research was of a Mode-2 rather than a Mode-1 type. But this would certainly need a broadening of the concept of Mode-2 research, as there was no clear party commissioning the Nat.Lab. for doing certain research projects (unless one would consider the informal communications between Holst and the Philips brothers as a sort of commissioning process, but that is not a very natural way of characterising these communications). The Nat.Lab. history suggests that industrial research can be transdisciplinary in every period, including when Mode-2 research is not practiced.

Disentangling Dichotomies

The history of the Philips Natuurkundig Laboratorium indicates that the dichotomies of fundamental or basic versus applied, and that of Mode-1 versus Mode-2 are inadequate to account for the changes in the Nat.Lab.'s research strategy. With respect to the first dichotomy, the Casimir period shows that research that was primarily aimed at acquiring knowledge of phenomena existed, yet clearly done in the context of a concrete application. Laser research is an example of this. When it was introduced in the Nat.Lab.'s research programme in the 1960s, studying the phenomena of the light and the way it was produced were the primary aims, yet the laser was a device for which one hoped to find an application. At that time there were no concrete ideas about that yet. It would not be until the VLP research started that the laser began to serve as a device in a consumer application. Casimir often stimulated research into new phenomena for which no concrete application was yet known, but always with the argument that certain applications would become feasible once knowledge about those phenomena was gained. Thus research into superconductivity and superfluidity was introduced in the Nat.Lab.'s research programme in that perspective. The dichotomy fundamental-applied suggests that either research aims at acquiring knowledge and it carried out without an application in view, or it is done in the context of an application but then aims at developing or improving that application. Clearly there is a need to distinguish at least a third type of research. This is what the "finalisation" approach offered, but because of its claim that this finalisation is science-driven and not-society driven (Forman, 2007) it still was too close to the linear model to survive. The need for the identification of a third type of research was also recognised by Stokes when he drew up his "Pasteurs' quadrant" (Stokes, 1997). In that quadrant he separated the two characteristics of the content of the research (knowledge about new phenomena or extension of knowledge about already researched phenomena) and the context of the research (a concrete application or a more general interest). Apart from a type of research that was focused on phenomena and not related to a concrete application purpose ("fundamental" or "pure basic" research) and research that focused on products and was aimed at application ("pure applied research"), Pasteur's quadrant has a third type of research that is focused on fundamental understanding, yet with a practical purpose ("use-inspired basic research" in Stoke's terms).

In a similar way the Mode-1 and Mode-2 dichotomy in fact includes two entangled dichotomies: the theoretical versus practical origin of the research, and monodisciplinarity versus multidisciplinarity. Here, too, the Nat.Lab. history gives rise to doubts about the necessity of the combination of the two. In the Nat.Lab. history there was multidisciplinary research that was not triggered by practical needs. An example of this is the hot air or Stirling engine research that was conducted in the period 1937–1979. This engine had not been asked for by any PD, nor was there any interest for it in the PDs. The Nat.Lab. researchers were interested in the machine because of its elegant thermodynamics. The research team was multidisciplinary: there were physicists and mechanical engineers involved. This multidisciplinarity was the result only of the device as such, not of the context in which it had to be applied. The VLP research also started as a Mode-1 research, as no PD had asked for it. Here the variety of disciplines was even broader: physicists, electrical and mechanical engineers. Moreover, much of the research that was triggered by practical needs was monodisciplinary. The LOCOS research, for instance, was mainly a matter of chemists, and in the Plumbicon research mostly physicists took part. So here a quadrant would also be needed to provide a full account for the various types of research in the lab.[24] In that quadrant the axes would be: research done for concrete practical application situation: yes/no; multidisplinary: yes/no. Contrary to Pasteur's quadrant, in which one cell is empty, this quadrant could have content for each of its four cells. An example of research with no specific application context and monodisciplinarity is the research into superfluidity (done by physicists only), for monodisciplinary research done in the context of a concrete application the LOCOS research would count as an example, the research that was done in the late 1970s to develop an optical communication system for the city of Berlin (done by the Nat.Lab. in cooperation with a Philips PD and a number of external telecom companies) is multidisciplinary research in a practical context, and finally an example of multidisciplinary research without a practical application is the Stirling engine research.

The history of the Philips Nat.Lab. also shows that the terms discussed above not only have a role in distinguishing specific types of research in order to put together a research programme with a certain desired orientation, but also have a rhetorical side, that perhaps was even more important. Defining the Nat.Lab.'s main role as a fundamental lab was most certainly meant as signal to the PDs not to interfere too much with the Nat.Lab.'s priority setting. In the Casimir period one research group was concerned with "fundamental mechanical research". One can question if this term had any meaning at all in terms of characterising a certain type of research, as

[24] In Stokes' Pasteur's quadrant one cell remained empty (the one for research not aimed at new knowledge, neither done in the context of a concrete application). In the quadrant to replace Mode-1 versus Mode-2 all cells can be filled. My critique on the oversimplification of the Mode-1 versus Mode-2 dichotomy is not unique. Hessels and Van Lente (2008) also show that this dichotomy does not do justice to the rich variation in R&D.

it sounds almost like a contradiction in terms since mechanics is a domain in which one can hardly claim the existence of new, unexplored phenomena. As we saw, the new role of the Nat.Lab. as a fundamental lab in the post-WWII years was triggered by the new structure of the company as a whole rather than by the promise of the Vannevar Bush doctrine. In other words, safeguarding an independent position in the company was more important than a belief in the almost guaranteed industrial profits of fundamental research. As a consequence the distinction between fundamental and applied therefore was often more rhetorical than real. This is something to be kept in mind when studying the policy and management documents of industrial research laboratory, and other R&D organisations.

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Multi-Level Complexities in Technological Development: Competing Strategies for Drug Discovery

Matthias Adam

An Obstinate Dilemma in Early Drug Development

For the successful development of technology, it is often crucial to pay close attention to a number of different complex circumstances. Development can be particularly demanding if the complexities lie on different levels of a system. For instance, the design of a technology can require, first, that the technological artifact intervenes in a highly sophisticated way on a specific part of a system. Second, this intervention might have to produce effects in the system as a whole that are mediated by complex systems mechanisms. In such a case, complexities on two levels have to be dealt with: *complexity of (local) intervention* and *complexity of systems effects* (or *systems complexity*).

For drug development, both types of complexity play a major role. Pharmaceuticals typically act on target proteins, such as enzymes, receptors or ion channels. The interaction between (potential) drugs and these molecular targets is often highly complex and subject to comprehensive theoretical modeling, chemical design and empirical testing in drug development. This problem is local in nature, being associated with specific proteins and their manipulation by drug molecules. At the same time, the intended and unintended effects of pharmacological interventions lie on the level of entire systems such as cells, organs or the organism as a whole. These global effects are often mediated by complex networks of mechanisms and can be extremely difficult to predict from the local interventions. Yet, successful and efficient drug development demands that the two levels of complexity are accounted for in the development process from early on.

In this paper, I will trace the relevance of these two levels through the recent history of early drug development. A number of different strategies has been developed that can be understood as competing attempts at coming to grips with either or both levels of complexity. Traditionally, *empirical search strategies* dominated

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much of drug discovery, as large numbers of randomly chosen substances or chemical modifications of existing drugs were tested in animal models. Novel drugs were typically identified empirically on the basis of their observable systems effects, independent from a scientific understanding of the underlying molecular interventions. Yet, particularly since the 1970s, enormous progress in biochemistry and molecular biology initiated a fundamental reorientation of pharmaceutical development. From the 1980s onwards, a more science-based paradigm of drug discovery was widely adopted under the heading of *rational drug design*. In particular, the chemical interactions of drug candidates with target proteins became subject to close scrutiny, aiming at a targeted design of novel drugs with well-defined molecular action. Despite some important successes, the overall efficiency of the rational drug design paradigm for drug discovery remained contested. As a consequence, empirical search strategies returned to the focus of attention of much of pharmaceutical research. Since the early 1990s, *high-throughput screening* was developed as a highly efficient method to test ever larger numbers of substances empirically. Yet, both rational drug design and high-throughput screening are to a large degree concerned with local problems, i.e. with the activity of drug candidates ("lead substances") on their targets and with the optimization of their local activity profile. Since the millennium, growing concern on this overall orientation of drug discovery can be observed. More holistic approaches to drug discovery based on the emerging *systems biology* have been proposed instead. They aim at focusing more closely on the relevant systems properties from early on.

The analysis of these competing strategies reveals a serious dilemma for early drug development. Rational drug design and high-throughput screening share a strong focus on singular molecular targets. With these methods, early drug design thus concentrates almost exclusively on interventive complexity. In contrast to this, the systems biology approaches primarily pay attention to whole systems and thus to systems complexity. The systems effects of pharmaceutical interventions had also been the major point of reference for traditional drug discovery. A closer look at these different strategies shows that there are serious obstacles to integrating the study of interventive and systems complexity in a systematic, methodical way. For instance, the methods of rational drug design and high-throughput screening often presuppose that the interventive target is isolated from its natural context to a high degree. It seems therefore inevitable that these approaches do not give equal prominence to the study of systems effects. In contrast to this, the systems biology strategies focus on systems complexity. Yet, it is unclear how these approaches could retain a sufficient grip onto the details of drug-target interaction and thus on interventive complexity (see section "Systems Biology Challenges Mainstream Drug Discovery").

I will argue that due to some fairly general properties of biological networks and the available options for pharmaceutical intervention, drug development is captured in a methodological dilemma that is considerably obstinate. In general, since systems effects remain largely unpredictable from local interventions, the modeling and testing of drug-target interactions cannot "reach up" to systems effects, while the investigation of systems effects cannot be tracked down to a molecular level on which it could direct the chemical design of drugs. This dilemma sets limits on the degree to which drug discovery and development can be turned into a systematic enterprise at all, for instance by being guided by a scientific understanding of underlying mechanisms or by exploring the options for pharmaceutical intervention in a methodical way. There are thus reasons to assume that at least in typical cases, drug development remains dependent on coincidence, serendipity or plain luck to bridge the gap between (empirical and/or rational) development methodology and actual therapeutic success.

Rational Drug Design and Its Limits

Traditional drug discovery up to the 1970s drew largely on empirical search and serendipity. Serendipitous findings are usually understood as useful hints or results gained in investigations that were originally directed at something rather different. Hugo Kubinyi compiled a list of more than 50 examples where important biological activities of substances were found serendipitously (Kubinyi, 1999). In many cases, the substances were intended for quite different uses, and their pharmacological potential surfaced unexpectedly in the course of the studies or was found by accident. Among the results from such findings are many ground-breaking drugs, such as the first antipsychotic chlormoprazine, the anticoagulant agent warfarin, or the first platinum-based anticancer-drug cisplatin.

Beyond serendipity, methods of empirical search played a major role in most cases of traditional drug development. In a comprehensive study of the development of innovative drugs between the 1940s and the 1970s, Robert A. Maxwell and Shoreh B. Eckhardt found that screening contributed to the development of 25 drugs out of their 32 cases (Maxwell and Eckhardt, 1990, 394). Within the screening contributions, the authors distinguish untargeted from targeted screening. For untargeted screening, a random selection of substances is tested for pharmacological activity in a biological test system (such as a cell- or organ-based assay or an animal model). Such substances can come from the historical libraries that pharmaceutical companies have assembled throughout their history, they might be gained from natural extracts, or they can be the result of unsystematic chemical synthesis (Adam, 2008b). For random screening, no prior clues are presupposed that test substances might be useful. Instead, random screening aims at the chance identification of such substances in the first place.

In comparison to this, targeted screening already starts from a prototype substance with some known pharmacological features. By way of chemical variation and testing, drug researchers aim to find derived substances with improved or modified characteristics that suit their purposes. For instance, one might try to improve the selectivity of the substance or to increase its potency. Often in these cases, the screening is iterated: from the variants of the original prototype, the most promising substances can be selected as starting points for further variation (Adam, 2008b).

Empirical search strategies such as random or targeted screening and iterative trial and error are widespread in technology development (Pitt, 2001; Thomke et al., 1998; Vincenti, 1990, 159–166). Usually, their role is compensatory: they are used because there is not sufficient information on the mechanisms underlying the technological intervention that could guide the design process in a targeted way. In traditional drug development, the situation was similar. Often, the protein targets were not known at all or had not been singled out when the search began, or if they were known, there was no specific information on their chemical and spatial structure that could direct the synthesis of potential drugs. In addition, there was often insufficient knowledge of the network of mechanisms that led to the disease. Empirical search by screening substances for their systems effects could be conducted even if the target of pharmacological intervention and the pathologically relevant system were only poorly understood (Maxwell and Eckhardt, 1990, 409). The typical approach both to interventive and systems complexity in traditional drug development was thus to find new drugs more by empirical trial and error rather than through scientific understanding.

The prospects of a more rational approach to drug development were discussed in the drug discovery community at least since the late 1960s. Such discussions were inspired by some (at their time) exceptional cases in which a more targeted, knowledge-driven development process was claimed to have been realized (Adam, 2005; Belleau, 1970; Hitchings, 1969). However, a realistic perspective for rational drug design as a standard approach emerged only with important scientific advances in biochemistry, molecular biology and gene technology during the 1970s. In the course of the so-called "biotechnological revolution", the background and methods available to drug discovery developed considerably. More and more potential molecular targets became known; through gene-technological cloning and expression of human protein in bacteria or yeasts, these targets were available for research and empirical testing; advances in X-ray-crystallography paved the way for the elucidation of the three-dimensional molecular structure of such proteins. As a consequence, a systematic epistemic access to protein targets and their chemical interactions with potential drugs came into reach of drug development. It therefore became conceivable to design drugs in their chemical structure on the basis of detailed knowledge of a target protein, its structure and its biological function. A prominent example for rational design of the 1970s is the development of the antihypertensive drug captopril. As one of the first cases, information gained through crystallographic studies was successfully used to model in spatial and chemical detail the active site of the target and its interactions with ligands. The chemical design of the drug was directly guided by this information.[1]

Altogether, rational drug design promises particularly to tackle the complexity of intervention in a targeted way. Based on detailed knowledge of molecular structures and an understanding of drug–target interactions, the optimal chemical design of drugs is sought to be identified. The modeling of the spatial relations and chemical interactions between drug and target therefore often stand at the center of the studies. In methodological respect, rational drug design aims to integrate scientific research into molecular mechanisms and chemical structures with the development

[1] Cushman and Ondetti (1991). For a detailed reconstruction of the case and the role of the interaction model, see Adam (2005).

of useful therapeutics. It is essential for the approach that inferential relations can be established between molecular knowledge and the chemical design of drugs. Yet, rational design is not a purely deductive approach. The identification of promising targets and the elucidation of their molecular properties often go hand in hand with empirical tests of drug candidates. Guidance from existing scientific knowledge is then supplemented by specific information that is gained through the development process. Repeatedly, targeted screening remains important for these purposes. If the inferential relations are sufficiently close, the fundamental knowledge can both contribute to the development process and be supplemented or confirmed through empirical testing (Adam, 2005).

The methods of structure-based rational development were broadly adopted in the pharmaceutical industry. In the early years, structural information for the most part still relied on homologs (Congreve et al., 2005). For instance, the developers of captopril could not make use of direct structural information on their protein target ACE (angiotensin converting enzyme), but used the available information on a related bovine enzyme instead (Cushman and Ondetti, 1991). Already in the mid-1980s, however, the development of the first next-generation antihypertensive drug, losartan, included direct structural information on its target angiotensin II (Adam, 2005), while in 1989, the complete structure of HIV protease became accessible and was subsequently used for the development of HIV protease inhibitors (Congreve et al., 2005). The number of protein structures published in the Protein Data Bank grew exponentially since the 1970s, from a total of 70 substances in 1970 via 500 in 1980 and 13,600 in 2000 to 48,000 in 2007 (PDB, 2008).

Rational drug design raised high expectations among drug researchers in the 1980s. It was hoped by many that it could substantially reduce the dependence on chance or serendipitous findings, paving the way to a much more orderly and predictable development process (Drews, 1999, 121–122). In addition, many pharma managers believed that cutting-edge scientific research became indispensable to develop innovative new drugs. As a consequence, drug development in the pharmaceutical industry became much more science-oriented than it was before. As one drug researcher described the situation in the early 1980s, “it was fashionable to invest in basic research, so we did” (Cockburn et al., 1999). Such expectations and the corresponding changes in research management seem quite natural in a situation in which a science-based, rational development process emerges as a serious alternative to existing, chance-dependent methods of empirical search. In general, however, it is enormously difficult in drug development to assess the success of such strategic decisions in a timely manner, since it regularly takes more than 10 years until decisions on research approaches and development technology have effects on the clinical introduction of new drugs (Schmid and Smith, 2004). In Kubinyi’s view, “the drug discovery scene is covered with a mist of myths, hype and false conclusions . . . Whenever a new concept or technology emerges, people get excited, jump on it and expect that new drugs will result more or less automatically” (Kubinyi, 2003, 665). In fact, when one takes stock of the results that rational drug design has delivered in the two decades after its broad adoption in the 1980s, the evaluation of the approach turns out to be rather mixed.

Rational methods have been widely adopted in the pharmaceutical industry for two main purposes: the optimization of existing lead substances (i.e. the chemical modification of existing prototypes with the aim to improve, e.g., potency, selectivity or pharmacokinetic properties), and the discovery of new lead substances. There is a broad consensus that rational methods of modeling drug–target interactions have contributed on a broad scale to lead optimization, and that rational methods have a huge impact on this step of drug development (Congreve et al., 2005; Hardy and Malikayil, 2003). In contrast to this, the outcome with respect to lead discovery is rather modest. On the one hand, Hardy and Malikayil have identified more than 40 substances in clinical development which have been discovered with the help of rational, structure-guided methods (Hardy and Malikayil, 2003, similarly Kuhn et al., 2002). On the other hand, only a relatively small number of these substances have so far been brought to the clinic. Tom L. Blundell and co-workers have identified only ten drugs that have emerged from structure-guided design (Congreve et al., 2005). Since there are three HIV protease inhibitors and two neuraminidase inhibitors among them, these drugs altogether address only seven different protein targets. Even if Blundell might have used rather strict criteria for inclusion in the list,[2] it indicates that there remain serious difficulties for the rational design of novel drugs.

Some of the difficulties for rational drug design are exemplified by an important method of rational target identification, so-called "virtual screening". In virtual screening, the binding and affinity of potential drugs to target proteins is assessed computationally. This is a rational method for lead discovery since the binding properties are predicted on the basis of structural information on the target protein (Klebe, 2006). The very approach to screen for leads computationally already shows that the chemical design of new lead substances often cannot be derived directly from the structure of the target protein alone. Instead, given compounds are checked "in silicio", i.e. by computer simulation, to determine whether they would bind to the target. The method of virtual screening itself faces two major problems, the docking and the scoring problem. The docking problem concerns the task to correctly predict the binding orientation of the substance in the active site of the target. Conformational flexibility both of the test compound and the target protein often complicates this task considerably. Still, the best docking programs correctly dock about 70–80% of compounds (Congreve et al., 2005, 899; Klebe, 2006, 582–583). The scoring problem concerns the task of comparing the substances that dock to a given target with respect to their affinity. The aim of scoring is to identify the substances that bind with the highest affinities to the target and are therefore most promising as lead substances. To deal with this problem, one has to calculate the strengths of the chemical bonds between compounds and target. In principle, these

[2] Not included are, for instance, tegaserod (Buchheit et al., 1995), as well as many "me-too" drugs, i.e. more or less close followers to existing drugs, which are regularly designed on the basis of detailed molecular knowledge.

could be calculated from first principles based on quantum mechanics or approximate force fields. Yet, according to Klebe, these calculations are computationally so demanding that "screening large samples of docked solutions to estimate binding affinities is still far beyond tractability" (Klebe, 2006, 588). Therefore, empirical scoring functions are typically used which are derived from empirically determined affinities. These functions have often only limited generality, making the accuracy of the scoring dependent on the relevance of the empirical reference set. In addition, many fundamental phenomena of compound-target-binding are not yet sufficiently understood, such as the role of water molecules or changes in protonation states. Such phenomena are highly relevant for binding and would therefore have to be included in a comprehensive model of drug–target interaction. Yet, they require more fundamental research before they can be taken into account (Klebe, 2006, 589; Kubinyi, 2003, 667).

This shows how difficult it is in drug development to derive useful hints on potential drugs even from detailed knowledge of the interventive target: a direct derivation of the chemical structure of drugs is often out of reach; a simulation of the affinity of given substances has to be based on empirical generalizations of limited range; important mechanisms of drug–target interaction are still little understood. Altogether, the promise of identifying optimal drug molecules on the basis of detailed molecular knowledge of protein targets turns out to be fairly difficult to fulfill (cp. Drews, 1999, 122). More often than not, the inferential relations between existing fundamental molecular knowledge and drug design are insufficiently tight to allow for far-reaching rational guidance at least of lead identification. There thus remains a considerable gap in pharmacology between fundamental knowledge and technology development (Adam, 2008a). Yet, so far this only shows that from a basic scientific point of view, pharmaceutical interventions into the organism are complex indeed.

High-Throughput Screening as Alternative and Complement to Rational Design

While with the trend towards rational drug design in the 1980s traditional screening approaches tended to be supplanted by more knowledge-based strategies, random empirical search returned forcefully to the focus of attention of drug development early in the 1990s. This apparent relapse in research methodology was largely driven by dramatic improvements in the test technology. Test efficiency was highly increased and costs of testing accordingly reduced. From about 1991, the approach was called "high-throughput screening" (Burch and Kyle, 1991). For a considerable time, high-throughput screening was considered mainly as an alternative (and competitor) to rational design. Yet, since about 2000, the two approaches are more often taken to be complementary (Good et al., 2000; Ratti and Trist, 2001), and they can actually be combined efficiently to deal with interventive complexity.

Random screening in traditional drug discovery was often based on organ or animal models. The use of these models was increasingly criticized not only due to ethical concerns, but also because of low success rates and high costs (cp. Chabner and Roberts, 2005; Böhm et al., 1996, 138–139 and 434–435). The return of random screening was enabled by the miniaturization of the experiments and their comprehensive automation (Burch and Kyle, 1991). State-of-the-art high-throughout screening (as of 2005) tests compounds against the isolated target protein wherever possible. Several hundred tests are performed in parallel in small wells on one plate. The protein target, the test substances, and any additional assay substances are added automatically, and the results are read out directly from the plate. The assay technology is optimized so as to allow the whole test process to be performed without intermediate steps of separation and washing. Typically, test substances are retrieved from the library and dissolved automatically. In addition to existing substance libraries, combinatorial chemistry provides large numbers of new substances by synthesizing them mechanically from a set of chemical building blocks. According to Schering researchers Oliver von Ahsen and Ulf Bömer, a throughput of up to 100,000 substances per day can be achieved once the assay for the screening campaign has been developed and validated. Regularly, libraries of up to one million substances are then screened (von Ahsen and Bömer, 2005).

From an epistemological perspective, the high degree of isolation of the interventive target from its natural environment is particularly noteworthy. According to von Ahsen and Bömer, biochemical assays which make use only of the isolated target protein form the "gold standard" in high-throughput screening, and are also preferred to cell-based assays (von Ahsen and Bömer, 2005, 481–482). The aim of high-throughput screening is to identify substances with a specific molecular activity, e.g. the inhibition of a certain enzyme, and the assay is specifically designed to detect exactly such substances. High-throughput screening therefore presupposes that protein targets are known, that their therapeutic potential is validated and that they are available for experimentation. In contrast to traditional empirical screening in organ or animal models, the experimental set-up excludes the identification of substances with unknown targets as well as serendipitous discoveries of biological effects. Instead, the results of a high-throughput screening campaign typically feed into an analysis of the structural and chemical features of substances with the sought for molecular action and of drug–target interaction. High-throughput screening thus not only seeks to identify promising lead substances, but also collects knowledge on the complex pharmaceutical intervention (Adam, 2008b). Yet, while rational design aims to infer the solution to the complex problem from a fundamental understanding, high-throughput screening attempts to optimize the odds for finding promising solutions by chance.

Despite of the opposing methodology of rational design and high-throughput screening, both approaches often contribute jointly to the chemical and structural modeling of drug–target interaction and the discovery and optimization of drug candidates. Regularly, there are parallel efforts to elucidate a protein structure, to identify promising substances by virtual screening and to develop assays for high-throughput screening (Good et al., 2000; Ratti and Trist, 2001; Schwardt et al., 2003). In various ways, the results from each approach can enrich the other, for instance when findings from virtual screening are tested experimentally, when structural conditions for affinity determined by screening are included in the chemical modeling, or when the molecular structure of complexes of the target with empirically discovered ligands are determined.

The combination of rational and empirical methods is particularly close in so-called fragment-based screening. The idea here is first to identify, with high-throughput screening, small molecule ligands (with molecular weights below 200–300 Da) that bind to the target protein. Subsequently, their mode of interaction with the target is elucidated with nuclear magnetic resonance or crystallography. Suitable fragments are then linked, by way of rational design, to larger, drug-size compounds, whose affinity is then again tested empirically. Step by step, lead substances can thus be developed by combining chance empirical findings and structure-based design. Many examples of successful application of fragment-based screening both to lead discovery and to lead optimization have been cited (Erlanson, 2006).

Rational design and high-throughput screening can thus be used as complementary approaches in early drug development. The two approaches can be readily integrated because they are both concerned with drug–target interaction. Both methods concentrate on molecular structure and action in isolation and largely ignore the wider biological context. This allows for an efficient combination of capacities in experimental search and knowledge-based modeling.

Systems Biology Challenges Mainstream Drug Discovery

The common focus of rational drug design and high-throughput screening is on interventive complexity. However, this concentration of early pharmaceutical research on drug–target interaction has increasingly been challenged. In particular, a provocative paper by biotechnology pioneer David F. Horrobin from 2003 triggered a broad debate in the drug research community on the overall orientation of pharmaceutical research (Horrobin, 2003). Horrobin argued that to a large degree, current research resembles a *Glasperlenspiel*, i.e. a game which is intellectually demanding and internally consistent, yet carries little relevance for real medical problems. Many authors took up Horrobin's critique and added their view of how pharmaceutical research is misled by its strong emphasis on interventive complexity (Butcher, 2005; Kitano, 2007; Kubinyi, 2003; Shaffer, 2005; van der Greef and McBurney, 2005). Much of the impetus of the critique comes from the widely shared perception that at least since the mid-1990s, the pharmaceutical industry has been going through a severe productivity crisis. Despite of an exponential growth of the expenditures for drug research over recent decades, the number of new substances that were approved each year by the US Food and Drug Administration as novel pharmaceuticals decreased from 53 to 22 between 1996 and 2006 (FDA, 2007; Nightingale and Martin, 2004). Among these new drugs, only two or three substances each year actually addressed new molecular targets (Congreve et al., 2005). It is a shared assumption of the contributors to the debate that drug discovery can only be “rescued” through a fundamental reorientation. They propose various new approaches to drug discovery that have in common that they are inspired by systems biology.

The critique of the prevalent paradigms of drug discovery and the arguments in favor of the new systems-biology approaches shed light on the relation between interventive and contextual complexity in pharmaceutical research. They show, on the one hand, that the focus of early drug development on isolated targets and their interaction with drug candidates, characteristic for drug development since the 1980s, has led to a neglect of the complex causal context into which one aims to intervene. On the other hand, while the systems biology proposals pay closer attention to contextual complexity, it is not yet clear how the existing high standards of investigation into drug–target interactions can be maintained in this approach. The discussions surrounding the systems-biology approaches thus indicate which obstacles exist for combining the study of contextual and interventive complexity.

The central critique that is leveled by the systems biology camp against mainstream pharmaceutical research is that it follows a “reductionist” approach (Horrobin, 2003, 153; van der Greef and McBurney, 2005, 961; Kubinyi, 2003, 665). This reductionism is taken to become manifest in two respects: in the concentration of drug discovery (and biomedical research in general) on the study of isolated components of the organism; and in its orientation on single targets as the objects of pharmaceutical intervention. Altogether, mainstream drug researchers follow the ideal of finding a single locus that plays a central pathophysiological role, and which is to be manipulated by a precise pharmaceutical intervention. From the perspective of the systems biology approaches, this ideal is fundamentally mistaken.

According to Horrobin, the study of the biochemistry of cells in vitro can reveal properly only what he calls the anatomical biochemistry, but not the functional biochemistry. We might find out which biochemical steps are present and therefore which pathways are possible. Yet, he argues, which pathways are actually instantiated in vivo depends on the natural context, including the circulation, the nutrient and oxygen supply, and the environment of hormones, which is lacking in in vitro studies. In addition, the effects of keeping cell cultures in antibiotics or in a lipid environment that differs from the natural environment are not sufficiently taken into account. If, as Horrobin claims, most diseases are based on defects in the functional rather than the anatomical biochemistry, the information that is most relevant for the development of pharmaceuticals cannot be gained through de-contextualized investigations (Horrobin, 2003, 152–153).

With respect to targets, Hugo Kubinyi points out that many important drugs possess rather unspecific profiles of action and are effective through a balanced effect on several targets (Kubinyi, 2003, 665). In addition, many drugs act indirectly, often at a distant site, rather than on targets that are central to the pathophysiology. In some cases such as the cholesterol-lowering statins, drugs interfere with a variety of mechanisms, and it seems surprising from the perspective of mainstream drug development that they do not have more severe side effects. More generally, it is argued that the prediction of the biological action of a substance on the basis of its activity on targets in isolation is highly problematic. Already the validation of the targets, i.e. the verification of the crucial role of the target in the pathology, is very difficult. The use of gene-knockout-mice for this purpose often delivers inconsistent results already in different strains of mice, and also the transferability of results from mice to humans is uncertain. The concentration of early drug development on substances that selectively address single targets, as practiced by rational drug design and high-throughput screening, is therefore criticized as being too narrow and of doubtful relevance (Horrobin, 2003, 152; Butcher, 2005, 461).

It is a central objection of the systems biology proponents against the established approaches that these cannot do justice to the complexity of biological systems. Various dimensions of complexity are adduced (van der Greef and McBurney, 2005, 961). In particular, the robustness and fragility of mechanistic networks is taken to be a feature of complex biological systems that is important for drug action. According to Hiroaki Kitano, complex biological systems are robust against a broad range of perturbations and therefore also against many modes of pharmaceutical intervention (Kitano, 2007). Robustness can be based, for instance, on negative feedback loops such that if a drug changes the level of some molecule, a feedback loop compensates for this change. Alternatively, due to fail-safe mechanisms, systems can continue to function even if a pathway is blocked by a drug, since alternative pathways take up its role. According to Kitano, the robustness of a system typically comes along with specific points of fragility, such as systems control for feedback loops. If drugs perturb such points, they can have great efficacy (or cause serious side effects).

Kitano argues that from the perspective of systems biology, many of the current drugs appear rather exceptional in that one substance successfully targets a point of fragility of a system. Diseases such as cancer, diabetes or autoimmune disorders prove difficult to treat pharmaceutically in this way. He attributes these difficulties to the robustness of the human organism on different levels, which requires more complex modes of pharmaceutical intervention. As one step to take into account the robustness and fragility characteristics of complex biological systems, he proposes testing combination therapies much more systematically. If his account of robustness is correct, one could expect that the effect of two drugs in combination can be much larger than the addition of their single effects. For instance, a neutralization effect due to a fail-safe mechanism could be eliminated by blocking the alternative mechanism as well.

Jan van der Greef and Robert McBurney (2005) propose the use of what they call "systems response profiles" in drug development. To be able to evaluate the effect of drugs on the organism as a whole, a wide range of molecular parameters from bodily fluids, cells or tissues are to be assembled with bioanalytical techniques. Diseases are analyzed through how they change these parameters. Drug development would then be aimed at substances that restore the profile of the healthy state. Drug candidates are evaluated for the changes they induce in the parameters, i.e. for their system response profile. According to van der Greef and McBurney, system response profiles could be used, for instance, to systematically identify promising combination therapies on the basis of the drugs' individual system response profiles and the profile of the disease. The approach grounds drug development on molecular information, yet rather than focus on specific targets, it is directed at the overall effect of drug candidates on the organism.

Another approach, developed by Eugene Butcher (2005), also concentrates on the biological effects of test substances rather than on the manipulation of specific targets. Butcher proposes using cell systems to screen for novel drugs. Such cell systems consist of various cell types and incorporate many of the pathways that are taken to be central to the disease. They are intended to model the disease biology and to mimic its complexity. Butcher takes it to be a main advantage of the method to be able to screen against a broad range of potential targets which do not have to be identified or validated in advance. He claims that compared to this approach, screening against single targets as practiced in high-throughput screening is too costly and slow. In addition, with cell systems assays, potential drugs can be selected for their biological effects, which he takes to increase the chances of finding unexpected modes of pharmacological intervention. It is claimed to be a major advantage of the approach to allow for such serendipitous findings (Butcher, 2005, 463).

To a considerable degree, the proposed alternative approaches to mainstream drug discovery revive methods of traditional early drug development, such as biological screening (Butcher, 2005), classical medicinal chemistry (Kubinyi, 2003), and clinical observation (Horrobin, 2003). Accordingly, the proposals have been advertised as a "return to the fundamentals of drug discovery" (Williams, 2004). This does not mean, of course, that they actually return to the methods of the 1970s rather than using the techniques of modern drug development. They build, e.g., on efficient screening methods (Butcher, 2005) or molecular analytics (van der Greef and McBurney, 2005), and some proponents also intend to integrate rational methods of modeling drug–target interaction (Kubinyi, 2003). Still, the contrast both to existing rational design and to high-throughput screening approaches is striking. For instance, while detailed knowledge of the targets is fundamental both to rational design and high-throughput screening, this is taken to be dispensable or is left to subsequent research in systems biology based drug discovery. As seen above, state-of-the-art high-throughput screening considers the maximal isolation of the target in biochemical assays as "gold-standard", while cell-based assays are taken to be problematic because of the possibility of "off-target hits" when the test substance binds to other targets than the one into focus (von Ahsen and Bömer, 2005, 481). The systems biology approaches, by contrast, seek to include rather than exclude the complexity of the disease biology and count on unexpected "hits". Since it is taken to be highly problematic to infer biological effects from molecular action and since important "emergent properties" (Butcher, 2005, 465) are seen to arise on the systems level, the study of isolated targets is considered to be largely futile. The systems biology approaches are directed towards drugs that act on many targets or towards combinations of specific drugs with the potential for "more-than-additive" effects (van der Greef and McBurney, 2005, 964–965). Altogether, the novel approaches aim at controlling the complexity of biological systems through complex interventions (Kitano, 2007, 208).

This is not the place to attempt to decide the dispute between the system biologists and mainstream drug discovery.[3] The systems biology approaches are still at an early stage, and proof-of-principle for these approaches still has to be delivered (van der Greef and McBurney, 2005, 966). It is therefore hard to estimate the prospects of the novel approaches: whether they are just another myth or hype, or can decisively contribute to overcoming the pharmaceutical industry's productivity crisis. Still, the objections against mainstream drug discovery cannot be easily discarded, and even proponents of isolationist methods concede that reductionist strategies have been too simplistic (Shaffer, 2005). The doubts about the development aim "single-substance-single-target" are substantial and make reference to very general features of biological networks. In addition, if the claims concerning the insufficient relevance of highly isolated experimental settings for medical practice are only in parts adequate, huge research efforts would be rendered questionable.

At the same time, however, the systems biology approaches leave important questions open. Also systems biology drugs would have to act through interaction with molecular targets. While the proposed search for useful combinations of existing drugs might deliver some interesting new options, the systems biology approaches would ultimately have to indicate how new lead substances can be identified and optimized (Adam, 2007). Yet, there are serious doubts that non-isolationist approaches would be efficient in discovering lead substances. With in vivo screens, for instance, hits with low yet improvable affinity or not yet optimal pharmacokinetic properties are likely to remain undetected even though they could be promising leads (Lipinski and Hopkins, 2004, 860). In addition, it is unclear so far how targeted optimization of leads is possible in a systems biology setting. A lead substance that comes out of a systems biology based search is likely to have a multitude of targets, with which it is likely to interact in a number of different ways. In addition, due to the holistic search strategy, the single molecular activities and their respective contributions to the overall biological effects might not be transparent. It would seem difficult, under these conditions, to establish meaningful structure-activity relations that would allow for a targeted optimization of the biological effect. As a consequence, an elucidation of the molecular activity profile might prove necessary for optimization after all (Butcher, 2005, 466).

Is Emergence the Problem?

These considerations make two things clear that are particularly important for the purposes of this paper. First, it is unlikely that the strong focus on interventive complexity that is manifest in mainstream early drug discovery is efficient. Instead, in

[3] Some of the advocates of the new approaches have declared competing financial interests (Butcher, 2005; van der Greef and McBurney, 2005), so one has to be aware of the possibility that the dispute might also be about markets for research technology (cp. Shaffer, 2005).

its attempt to solve the local problem of how to intervene into the diseased organism first, the systems effects of these interventions fall out of sight. In addition, many pharmaceutical options that cannot be reduced to the single-molecule-single-target pattern, but require more complex modes of intervention never come into view. Second, the systems biology approaches might address systems complexity from early on in drug development. Yet, they leave open how the grip onto the detailed molecular interactions can be maintained even though epistemic and manipulatory access to these details remain indispensable for lead identification and optimization. These two observations together indicate a significant dilemma for the early stages of drug development. Sufficient attention to the local level and interventive complexity on the one hand, or to the systems level and contextual complexity on the other hand seem to be possible only at the expense of one or the other. However, an integrated attempt on both levels would be required for methodical early drug development.

As shown above, the systems biologists have described the problem in terms of reduction and emergence (see, e.g., Kitano, 2007, 202; Butcher, 2005, 465; Horrobin, 2003, 153; van der Greef and McBurney, 2005, 961; Kubinyi, 2003, 665; Van Regenmortel, 2004). In their analysis, the problem with mainstream drug development is that the behavior of biological systems cannot be reduced to the behavior of their molecular parts, but is emergent to the molecular level. If this were true, the sketched dilemma for drug development would be based on the very nature of biological systems. Generally speaking, reductionism holds that there is a one-sided dependence of the whole on its parts such that the properties and the behavior of the whole are determined by the properties and the behavior of the parts. This includes that at least in principle, system properties can be fully explained by the properties of the parts, while emergent system properties could not be thus explained or predicted (cp. Carrier and Finzer, 2006, 272; Van Regenmortel, 2004, 1016). If system properties were emergent relative to molecular properties, it would be a matter of principle independent from the state of scientific knowledge and technological capacities that systems effects cannot be predicted and explained on the basis of molecular knowledge alone. The methodological obstacles to combining our studies of complex molecular interactions and of complex biological systems would be based on an ontological divide between the two realms.

Yet, the specific arguments that are adduced in favor of the systems biology approaches in drug discovery do not preclude reductionism. As seen in the previous section, systems biologists take issue with the concentration of mainstream drug discovery on *isolated* parts of complex biological systems and with its ideal of *single-target*-drugs. In terms of reductionism, the crucial discrepancy between mainstream and systems biology drug development is therefore whether system properties can be explained from the properties of isolated parts and whether systems effects are predictable from local molecular interventions. Systems biologists argue that the context is essential to define the relevant molecular properties and that it is crucial to consider the networks of mechanisms that mediate the effects of local interventions. Complex systems effects are therefore taken to require interventions at a variety of loci, which can have a more-than-additive overall effect. These arguments are compatible with the assumption that the molecular parts and their (molecular) interactions in their entirety determine and explain unidirectionally the system properties.[4] The arguments are rather directed against *local* reductions and not against reductionism as a general position on the relation between molecular level and biological system properties. The possibilities are left open that the relevant context of the molecular components of the system can in principle itself be characterized on a molecular level and that system properties such as the formation of feedback-loops, failsafe mechanisms or non-linear combination laws can be explained from the properties of the parts and their molecular context. There is thus no need to assume that the dilemma for drug discovery has an ontological basis in emergent system properties.

Instead, the problem is methodological in character. The local problem of identifying and optimizing tools for pharmaceutical intervention can be treated systematically with rational design and high-throughput screening if the efforts are concentrated on specific drug–target interaction. Yet, due to the complexity of biological systems, predictions of the overall effects of such interventions are often impossible, and it is therefore largely a matter of luck whether potent and selective molecular agents turn out to be therapeutically valuable and safe drugs. The alternative approach of concentrating on systems effects may allow the description of desirable systems effects and perhaps breaking them down to a broad range of necessary interventions at a variety of molecular targets. Yet, it remains unclear how substances with a thus specified profile could be identified or optimized in a systematic way. While one level of complexity is addressed methodically in either case, a solution to the other level seems to be dependent on lucky coincidences or otherwise unpredictable sources of knowledge or interventive capacities. No method is in sight to tackle the complexities on both levels equally. This problem arises from fairly general features of biological systems. Effectively, a complex mode of intervention on specific parts of a system (the protein targets) and a complex interplay of the parts to bring about systems behavior are sufficient to produce a fairly obstinate dilemma for technological development.

It has repeatedly been described as a characteristic feature of pharmaceutical research and development that it needs to draw together scientific and technological resources from a number of different areas – chemical, biological, and clinical–, while its success often remains dependent on chance or serendipity (Maxwell and Eckhardt, 1990, 411). It is no surprise that novel development approaches aimed at overcoming this situation by integrating the heterogeneous knowledge base and thus making the drug development process less dependent on coincidences (Drews, 1995, 936). Certainly, each of the approaches sketched in this paper can claim its successes

[4] An exception among the cited systems biologists is Van Regenmortel (2004). Among other things, he claims that biological *functional* properties are essential to systems behavior, and these properties could only be explained on the basis of evolutionary history and environmental factors. Yet, Van Regenmortel's cases come from the development of vaccines. As biological pharmaceuticals, they might raise different problems with respect to reductionism as the development of synthetic chemical drugs, which is the focus of attention of the other authors.

or promises. The obstinate dilemma, however, marks the point where they seem bound to fail in their attempt to change that basic condition of drug development.

This has far-reaching consequences on the epistemology of pharmaceutical research and development. At specific points of the subject area, various scientific and technological approaches can be integrated in a very fruitful way, as the combination of rational design and high-throughput screening shows. Yet on the whole, the subject area is too complex, and the different approaches are too limited and domain-specific to enable a comprehensive epistemic and technological access. Both scientific and technological methodical endeavors therefore remain isolated attempts, to an important degree, to understanding and control of the subject area. Technological success thus regularly depends on coincidences that show up unexpectedly and are little understood.

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Theory and Therapy: On the Conceptual Structure of Models in Medical Research

Martin Carrier and Patrick Finzer

The Cascade Model: Control Presupposes Understanding

The Scientific Revolution of the early seventeenth century epitomized a new conception of knowledge. The pioneers of the new science, like Galileo Galilei, Francis Bacon, and René Descartes, emphasized the practical relevance of gaining knowledge about nature and conceived the notion of applied science. By understanding nature's workings, we are able to spot opportunities for intervening in nature and to change its course according to human needs and aspirations. The new idea that emerged in the Scientific Revolution was that the epistemic penetration of nature is the basis of controlling nature (Bacon, 1620, Bk. I., §3, §110, §117, §129; Descartes, 1637, IV.2, 101).

There is much to support this view. First, it is plausible to expect that disclosing causal chains will reveal options for shaping the production of the effect. Likewise, theoretically understood relations can be generalized more easily so that connections to other phenomena can be established. Such interrelations among phenomena provide additional opportunities of intervention. In sum, theoretical understanding can plausibly be expected to improve the prospects for identifying further useful relations. Second, this assumed dependence of targeted intervention on scientific understanding is confirmed by examples from the history of technology. The invention and development of the vacuum tube (the TV tube) and the construction of light emitting diodes (LEDs) proceeded on the basis of insights into the interaction of electromagnetic fields and charged particles. The Baconian idea that intervening in nature relies on understanding nature is now sometimes called the *cascade model*; knowledge flows smoothly from the universal principles to the concrete solutions. The model suggests that the control over nature accrues from disentangling the underlying causal fabric.

The cascade model is to be distinguished from the so-called *linear model*. The linear model assumes that technological progress is a consequence of fundamental

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research. Basic research leads to applied research which smoothly transforms into development and ends up with implementing the novelty (Stokes, 1997, 3; Godin, 2006, 639). The two models diverge from one another in two respects (Carrier, 2010). First, the cascade model refers to substantive or logical dependence, the linear model outlines a temporal sequence. The linear model suggests that it is of no use to attack a practical problem by research narrowly targeted at this problem. Rather, research performed without thought of practical ends needs to be at the origin, and only the subsequent step can aim at a specific practical problem. The latter is solved by translating scientific discoveries into products and procedures. The cascade model acknowledges, by contrast, that fundamental insights can be gained in the course of practical projects so that understanding, necessary for accomplishing practical success, can be produced in the context of application without a foregoing step of basic research. Second, the linear model assumes that technological change is hot on the heels of scientific progress, whereas the cascade model recognizes that the established body of scientific knowledge constitutes a huge repository of technological options that can be tapped at various locations, not alone at its more recent additions (Rosenberg, 1991, 337). According to the cascade model, technological development is essentially science-based, but seldom relies on the most recent findings in basic research.

Scientific understanding can be achieved by causal analysis or by theoretical unification. That is, phenomena or processes are understood by showing how they are brought about or by demonstrating that they instantiate a general pattern. Both variants concur in that understanding specific instances is assumed to be dependent on bringing to bear generalizations and spelling out comprehensive approaches. The cascade model takes this generic approach also to be the royal road toward controlling nature and to make her serve human purposes. Practical achievements are accomplished by subsuming the particular problem at hand under an appropriate general label and by bringing a nomological machinery to bear on its solution.

Emergentism: The Limited Grip of General Theory

In contrast to these considerations, technology development proceeded largely independently of scientific understanding in a number of cases. The practical aspirations of the Scientific Revolution were not realized for quite some time. The Industrial Revolution of the eighteenth century moved on without significant input from the sciences and was largely fueled by familiarity with existing technologies, engineering ingenuity, and tinkering at the bench (Stokes, 1997, 35–36). The steam engine preceded the first attempt to account for its operation by half a century. Likewise, Thomas Edison invented the light bulb without relying on theorizing and rather employed trial and error at a large scale. Conversely, the epistemic penetration of a research field does often not translate into workable devices. In the second half of the nineteenth century, Claude-Louis Navier and others developed the set of fundamental equations of fluid dynamics now known as the Navier-Stokes equations.

These equations describe the effects of the conservation of momentum, energy, and mass on the motions of liquids or gases, and are supposed to capture fluid flow completely. However, their practical fertility has proven to be fairly limited on various occasions. For instance, they failed to offer significant assistance in constructing airplane wings. When the Wright brothers in 1903 invented the first controlled flying machine that was heavier than air, no one, including the valiant pioneers themselves, could account for the technological feat they had been able to accomplish. Their wing design was essentially based on wind tunnel experiments using trial and error. The Wright brothers, just like their more systematic successors like Gustave Eiffel, developed wing shapes by testing a wind foil profile in the wind-tunnel and making changes according to earlier findings. It was only such groping exploratory experimentation that permitted more specific estimates of critical quantities like drag or lift. Scientific insight is conspicuous by its absence.

Such cases suggest that theoretical understanding is not always the pivot of technological achievement. They square well with philosophical claims to the effect that comprehensive theory often fails to account for the richness and the details of experience. In Nancy Cartwright's view, overarching laws or high-brow theories are too idealized to admit access to concrete phenomena. Theory-based models produce overgeneralizations and fail to capture the particulars of the data. Moreover, such flaws are hardly ever fixed by adding theory-induced corrections. Instead, the empirical performance of models is usually improved by bringing to bear experiential regularities and practical approximations. Cartwright's examples concern money bills swept away by the wind, complex electric amplifiers and lasers. Her claim is that descriptive adequacy is the privilege of small-scale accounts which are tightly locked onto specific problems.

A model may be called "phenomenological" if it is shaped conceptually by the demands of the problem-situation and the circumstances at hand. Consequently, phenomenological models are not necessarily completely independent of theory, but they contain comparatively few elements that transcend the particulars of the explanatory challenge to be dealt with. The explanatory burden is largely borne by assumptions specific to the realm in question such as low-level observational generalizations, parameter adjustments or correction factors (Cartwright, 1983, Chapter 2–3, 6, 8, 1994, 1997, 1998). Tidal flow is a case in point. The prediction of the tides for a particular harbor is not based on the known causal mechanism underlying the phenomenon but is rather achieved by performing a Fourier analysis of the tidal oscillations observed in the past at the pertinent location. The reason is that the influence of a multiplicity of factors relevant for the quantitative details of tidal flow (such as coastline, water depth, currents) can hardly be assessed on first principles so that the phenomenological analysis is more robust empirically (Sauer, 2004).

We call such a view of scientific modeling "emergentist." Emergentists contend that realms of experience and levels of complexity are largely separated from one another. The phenomena are assumed to be specific in different fields and at different levels of organization. As a result, drawing on the principles that govern the behavior of the constituents will contribute little to the elucidation of the properties of organized wholes. The general laws that cover the undistorted behavior of the fundamental entities are not of much use for capturing the features of composites subject to the usual intersection of causal influences (Carrier, 2009, 25, 2010).

The emergentist position is not restricted to applied research but is rather intended to expound the general limitations of bringing to bear theoretical principles. The chief claim is that it is highly non-trivial to hook up theory with evidence and that the only way to get a grip on the phenomena is by making use of specific models that are closely bound up with a particular problem. Still, the ensuing message is that understanding by drawing on fundamental principles is of no avail for solving practical problems. Practical challenges should be confronted directly by addressing the particulars of the situation in question and without taking a detour through nomological approaches of high generality. Fundamental truths only sparsely bear practical fruit (Carrier, 2004, 1–3, 9–14).

The Interactive View: Theory-Based Structures Adjusted Empirically

Cascade model and emergentism represent stark contrasts regarding the relationship between theory and experience or between generic and specific traits. The cascade model takes the general to be primary in epistemic respect. We understand a phenomenon or process by realizing that it is an instance of a more comprehensive scientific kind which plays a role in other explanatory contexts as well. Emergentism, by contrast, takes individual phenomena and the variety of experience as primary and sees the chief epistemic commitment of science in taking account of this richness in detail. In what follows we wish to adumbrate an intermediate position and examine its viability using examples from medical research. Studying the connection between theory and therapy holds some promise in that intricacies of the relationship between the general and the particular can be expected to show up here in a salient way.

The first thing to be noted at this juncture is that emergentism fares badly against the backdrop of what is called the "model debate." This debate was prompted by Cartwright's emergentist position and has unfolded since the mid-1990s. The chief results of this debate can be summarized to the effect that, first, it is much more difficult than anticipated to bring general principles to bear on experience, but that, second, such principles are still essential in that they shape models in conceptual respect. General principles and comprehensive theories need models as mediators for bridging the gap between overarching laws and the subtleties of experience. The pivotal point is that models turn out to be much more complex than assumed earlier. In addition to initial and boundary conditions, they need generalizations and empirical adjustments of various sorts – as stressed by Cartwright. However, as Margaret Morrison was the first to emphasize, these additional elements often merely modify a theory-based conceptual framework. The conceptual structure of the models is shaped by general theory although the outcome produced is influenced significantly or even dominated by the necessary empirical adjustments (Morrison, 1999; Winsberg, 2003; see Carrier, 2004, 9–12).

Consider the "orifice problem" in hydrodynamics as an example. This problem concerns the calculation of the amount of liquid that pours out of a container through a hole. The received treatment appeals to the conservation of mechanical energy and takes the kinetic energy of the jet to be equal to the potential energy of the fluid in the tank. Yet the observed amount of discharge is much smaller than this estimate based on first principles; the theoretical prediction can be up to 40% off the mark (depending on the circumstances). This deviation is standardly taken care of by appending a correction factor. The qualitative explanation of the diminished flow is fairly obvious: in streaming out, the liquid converges on the opening so that a kind of fluid congestion is built up. This congestion encumbers the flow through the hole so that the amount of emitted liquid is diminished. But no reliable quantitative estimate of the reduction can be given on first principles. Rather, the correction factor is assessed empirically for various orifice shapes (Bod, 2006, 14–15).

This example suggests that the idealized conditions to which theoretical principles apply may deviate significantly from what is observed in practice. However, rather than abandoning theory-centered approaches, scientists use them for structuring the problem-situation in conceptual respect. Theory is used for highlighting significant features, such as the height of the tank, and for distinguishing them from irrelevant aspects, such as the container shape. As the example reveals, this holds true even in those instances, in which the theoretical account is substantially wrong regarding the predicted outcome.

The interactive view takes the features of this case to be indicative of the relationship between theory and evidence in large domains of experience. That is, non-theoretical factors like unexplained properties and corrections play an important role in constructing specific accounts, but the models used for representing the phenomena are conceptually shaped by higher-order theories. The interactive view agrees with the cascade model in granting theory a key role in shaping the models. In contrast to the emergentist approach, models are not assumed to be constructed afresh in order to cope with particular challenges. However, the interactive view deviates from the cascade model in granting situation-specific factors a much greater weight in concrete explanations and joins emergentism in the claim that nature is too multifaceted to be captured by comprehensive principles without remainder. Yet the emergentist approach overshoots the goal by dismissing the essential role of theoretical analysis in accounting for the particulars of experience. The best way to deal with multifarious experience is by bringing to bear general principles and to correct for their shortcomings by empirical adjustments (Carrier, 2009, 29, 2010, 176–178).

The interactive view, just like the cascade model and emergentism, does not refer specifically to applied research but rather outlines what it takes to apply theoretical principles to experience. However, the challenge involved becomes more pressing in applied research. The reason is that fundamental research can often confine itself to the pure case, in which the phenomenon at issue appears without distortion. In fact, if the viability of a theory is to be examined, focusing on the pure case is advisable epistemically. If disturbing influences are dominant, possible anomalies are hard to attribute to the theory at issue so that at lot of loopholes are left. Such considerations also underlie the epistemic privilege of experimentation. Experimental tests of theories proceed in a strictly controlled environment from which distorting factors are shielded off as much as possible. This is different in applied research. The latter cannot avoid being torn into the entanglement of diverse disturbances. Distortions and side-effects are important in practical contexts; they cannot be neglected for this reason. Applied research is bound to face complexity.

We grant at once that complexity needs to be dealt with in large parts of epistemic science, too; we do not claim that models in applied research are distinguished by a particular conceptual structure. Rather, the properties of models vary across a continuous spectrum. At one end, the relevant theoretical idealizations approximately hold, the disturbances are weak, and the result of the calculations is in fair agreement with the observations. At the other end, the models used are more intricate and tangled and contain elements that transcend the theory in question or are even inexplicable by any theory. When it comes to mastering complexity, models need to resort to different theories, include unexplained regularities, and draw on knowledge specific to the problem-situation at hand. Such *local models* are more heterogeneous and less easily generalizable than the theoretical models designed to cope with simplified arrangements. They are still conceptually shaped by general theory, to be sure, but the interstices left are gaping and need to be filled by patchwork accounts, parameter adjustment and ad-hoc corrections.

This is the view that emerges from considering various examples of applied research (Carrier, 2004, 2009, 2010). Our intention is to investigate whether this framework is also suitable for appropriately capturing the epistemic characteristics of medical research. We focus on a particular research field, namely, the assumed relation between chronic inflammation and cancer.

Biological Understanding and Medical Treatment

Medical treatment is often taken as a field in which understanding and intervention are largely decoupled. The assumption is that medical drugs are selected according to their observed efficacy in treating certain ailments, without any deeper understanding of what is responsible for their salutary effect. This is certainly true to some extent, but opposite tendencies figure prominently in the history of medicine as well. In the Hippocratic-Galenic humor theory, one of the most important ways of treatment was to prescribe a certain individually adjusted diet. According to this conceptual framework, humans contain four essential fluids (black bile, yellow bile, phlegm, and blood) whose imbalance was associated with certain diseases. The amount of each humor in the body was supposed to be affected by a number of factors, among them diet. Accordingly, the humoral balance was assumed to be redressed by the intake of certain sorts of food, and the precise composition of the diet was determined in light of a theory about the human body. In addition, the four humors were integrated into the prevailing account of the structure of matter and thus intertwined with what we would call chemistry. In sum, Hippocratic-Galenic medicine suggested a scheme for understanding diseases and for deriving remedies from this understanding.

Conversely, during the Scientific Revolution physicians turned to systematic investigations of the human body with the intention to improve treatment. The history of the *Academia Naturae Curiosum*, that was later called *Leopoldina* and is now the German National Academy of Science, is symbolic of the prevailing assumption that adequate medical treatment needs to be rooted in biological understanding. In 1652, four physicians from Schweinfurt, who represented the totality of the academic healing competence of the city, formed an association for studying human physiology systematically. Medical doctors of the period suffered from the competition of surgeons and barbers, of quacks and healers with indigenous knowledge about plants and herbs, whose therapeutic efforts were no less successful but far less expensive than their own. The newly formed research body was directed at improving the efficacy of medical prescriptions by exploring the underlying biological relations. Knowledge about human physiology was sought in order to enhance the effect of medical intervention (Toellner, 2002, 17–19).

Both examples bear witness to the preponderant assumption that appropriate intervention in the human body needs to be based on understanding its functioning; they reveal that the cascade model was influential in medical thought, too. Yet this is a far cry from showing that the control of diseases was actually founded on understanding their nature. A look at medical practice seems to teach a quite different lesson, namely, that success in treatment was largely based on trial and error. Progress in vaccination was made by Edward Jenner and Louis Pasteur by relying on observed correlations and without any understanding of why the suggested means were efficacious. The same holds for parts of pharmacological research up to the present day. For instance, the most widespread procedure used in drug research throughout the twentieth century is schematic screening. A large number of potentially effective substances are administered to model organisms or tissue test systems and their effects are registered. When a successful medication had been tracked down by a procedure of this sort, it was in no way automatically clear how the drug operated. As a matter of fact, this is true of a significant fraction of the drugs in use today. Aspirin had successfully relieved headache for almost a century before its biological mechanism was finally disclosed, and it did not work better after its physiological impact had been elucidated. In sum, a lack of understanding or erroneous ideas about the modes of operation of vaccines, sulphonamides, aspirin, or penicillin prevailed for a long time.

Likewise, "deep-brain stimulation," a treatment option of tremor in Parkinson's patients, was introduced by relying on mistaken beliefs about its physiological effect. Deep brain stimulation involves the surgical implantation of a "brain pacemaker" which emits high-frequency electric pulses. Initially, there were reasons to believe that the therapeutic effect was due to the inactivation of brain cells whereas it turned out later that, to the contrary, the pulses enhance cell activity (Lozano and Kalia, 2005). In such cases, medical progress obviously did not rely on understanding.

However, modern pharmaceutical research is rife with cases that point in a different direction. In some such cases theoretical guidance proved indispensable. That is, investigating the phenomena without prior understanding, however tentative, of possibly relevant factors would have thwarted the discovery of empirical relationships that appear striking and hard to miss with hindsight. The discovery of captopril in the 1970s is a case in point. Captopril is a so-called ACE-inhibitor that lowers blood-pressure by inhibiting the enzyme ACE ("angiotensin converting enzyme") which was supposed to play a role in regulating the relevant physiological processes. Industrial researchers started with an empirical screening of substances that looked suitable for blocking the ACE enzyme. However, trial and error failed to come up with a useful agent. Alternatively, they studied the interaction between enzyme and inhibitor in other cases and arrived at the causal hypothesis that bonding with a zinc ion produced the inhibition. This model was applied hypothetically to the issue in question and used as a guide for screening compounds that exhibited the desired chemical structure. This time the screening was successful – unlike the previous theoretically uninformed search. The discovery of captopril depended essentially on an understanding of role and structure of ACE (Adam, 2005, 523–525; Adam et al., 2006, 440–441).

What also emerges from the consideration of this example is that the physiological assumptions in question did not enable researchers to derive the formula of the agent substance sought for. These assumptions only served to narrow the field of potential agents by delineating key elements of its chemical structure. Trial and error was still needed to fill the interstices left by the theory and to single out an appropriate substance. But hypotheses about causal mechanisms involved and about inhibiting the crucial pathway proved indispensable for looking at the right places. This case provides initial support for the assumption that medical research operates with local models and in accordance with the interactive view. This finding makes a closer look worthwhile.

Chronic Inflammation and Cancer

We wish to analyze the conceptual nature of the models invoked in medical research on the relationship between chronic inflammation and cancer. The hypothesis that a connection exists between the two conditions goes back to Rudolf Virchow (1863) who discovered inflammatory cells in cancerous tissue and suggested that cancerous growth emerges from chronic inflammation (Balkwill and Mantovani, 2001, 539). In the subsequent decades, clinical observation testified to some sort of connection between inflammation and cancer. Chronic bacterial infection with *Helicobacter pylori* was found to be associated with gastric cancer; parasitic infections with *Schistosoma* or liver flukes are known to heighten the risk of cancer. Papilloma viruses were ascertained to cause the vast majority of cervical cancer and the development of liver cancer could be linked to the infection with hepatitis viruses (Parsonnet, 1999, 4). Likewise, non-infective agents that induce chronic inflammation go along with an increased risk of cancer, too. For instance, cigarette smoke is recognized as a risk factor for chronic bronchitis and the development of lung cancer.

Epidemiological correlations are in need of additional arguments for elucidating the direction of causal influence. Treatment involves an intervention in the process underlying the disease and might indicate the direction of causation. Nonsteroidal anti-inflammatory drugs (NSAIDs), such as aspirin, ibuprofen and many others, reduce inflammation and are also beneficial in preventing some forms of cancer. A number of studies have found that taking NSAIDs diminishes the risk of contracting colon cancer (Gupta and DuBois, 2001, 12) and other tumors (Thun et al., 2002) significantly. Additional evidence comes from clinical studies in patients with familial adenomatosis polyposis coli (FAP). These patients suffer from a germ-line mutation that causes the development of intestinal polyps early in life that exhibit a high risk for transformation into cancers. The NSAID sulindac has been documented to be able to reduce the size and number of those polyps in FAP patients; the polyps recur when therapy is terminated (Giardiello et al., 1993; Labayle et al., 1991).

Accordingly, clinical observation suggests that a number of inflammatory diseases enhance the risk of contracting cancer (Shacter and Weitzman, 2002, 217–220; Nam et al., 2004, 1–10). There is strong epidemiological support for the assumption that chronic inflammation is an important causal factor in the onset and progress of malignant tumor growth. A current estimate suggests that approximately 20% of all malignancies are initiated by a chronic inflammation following an infection (Parsonnet, 1999, 4; Balkwill and Mantovani, 2001, 539).

However, the success of therapeutic intervention based directly on this observed causal correlation is less than convincing (Lotze, 2004, 189; Lotze and Herberman, 2004, 30). The bottom line is that cancer cannot be treated successfully by administering NSAIDs. Beneficial influences occur only in some forms of cancer, not in others, and in preventing the disease, not in curing it. In general, the therapeutic effect is fairly limited. In addition, anomalies emerge. If cancer is assumed to be tied up closely with inflammation, malignant tumors should be subject to an immune response. But apparently they are not. Consequently, the observation-centered or phenomenological approach sketched here runs into serious deficiencies and shortcomings that reveal the need for uncovering the micro-causal chains that underlie the manifestation of the disease. This is where theory-shaped models make their appearance.

The most-contrastive approach to phenomenological modeling is to switch into the top-down mode and let therapeutic intervention be guided by the cascade model. In this vein, the first step toward successful treatment is understanding malignant growth in terms of first principles. This is a vision advocated by Douglas Hanahan and Robert Weinberg: "We foresee cancer research developing into a logical science, where the complexities of the disease, described in the laboratory and clinic, will become understandable in terms of a small number of underlying principles" (Hanahan and Weinberg, 2000, 57). What is meant by "logical science" here is that medical knowledge about cancer should not be a collection of observed causal relations in which most elements are isolated facts that could have easily turned out otherwise, but rather a system of tightly interwoven causal chains, framed by a few general principles in whose light the observed relations receive their appropriate place and due significance.

Here is a quick glance at the general framework Hanahan and Weinberg put forward. Cancer is often conceived as a process that takes place at the level of individual cells; a cell undergoes a mutation and starts to proliferate. A tumor is nothing but an array of particular cells that have gone out of control. But this view overlooks the amount of interaction between a tumor and its environment without which the tumor could not survive. For instance, one of the conditions for excessive tumor growth is the neutralization or block of the regulatory system that constrains cell reproduction. A carcinoma cell manages to achieve this effect by taking adjacent cells into its service. Carcinoma cells co-opt their normal neighbors by inducing them to release augmented rates of growth stimulating signals. Likewise, the coordinated formation of new blood vessels, or angiogenesis, is essential for the continued growth of the tumor. Tumor cells interact by using a large number of angiogenic factors so as to induce and sustain the growth of new blood vessels in a coordinated way (Hanahan and Weinberg, 2000, 58–60, 64).

Such pathways to tumorigenesis reveal that a reciprocal adjustment of different cells is required. Cancer development involves a change in the pattern of interaction between tumor cells and their cellular environment and among the tumor cells themselves. Tumors thrive on the collaboration between different kinds of cells. This picture, as Hanahan and Weinberg argue, involves a reconceptualization of cancer research which depicts malignant growth as a holistic process. They evoke the spirit of the cascade model in claiming that with "holistic clarity of mechanism, cancer prognosis and treatment will become a rational science. . . . It will be possible to understand with precision how and why treatment regimens and specific antitumor drugs succeed and fail" (Hanahan and Weinberg, 2000, 67).

In this general framework, the connection between cancer and inflammation can be expected to receive its appropriate place, too. Inflammation promotes infiltration of new cells, releases growth-promoting factors and thus produces a micro-environment that is favorable to unfettered proliferation (Mueller and Fusenig, 2004). Inflammation is involved in wound healing that produces new tissue; yet cell reproduction comes to a halt after the healing process is completed. By contrast, in micro-environments that contain inflammatory cells and cytokines (that attract inflammatory cells), cell-division continues unabatedly. Tumors are like wounds that do not heal (Dvorak, 1986). The stimulation of malignant growth by inflammation exemplifies the interactive nature of cancer that is highlighted by the holistic model. Inflammatory cells and cytokines present in the tumor and its environment foster tumor growth and contribute to suppressing the immune response against the tumor. A paradoxical reversal of effect is involved here: the host immune cells trigger the inflammatory process, on the one hand, but suppress immune reactions, on the other. This reversal is puzzling and insufficiently understood at present (Hanahan and Weinberg, 2000, 60; Balkwill and Mantovani, 2001, 539–540, 543; Coussens and Werb, 2002, 860; Lotze, 2004, 190–191; Balkwill et al., 2005, 211–212).

This example is supposed to make plausible that, first, the connection between cancer and inflammation fits well into the general holistic framework and that, second, this connection can be expanded down to the molecular level. In fact, the causal processes involved can be elaborated in great detail (that we need to skip here). However, third, anomalies persist. We mentioned the problem of the missing immune response: tumors are supposed to be produced by inflammation, but evade inflammatory immune responses. This problem is now reconceptualized at the molecular level, but remains unsolved. In order to complete the picture, let us add, fourth, that the models that emerge at the molecular level are afflicted with lacunae that can only be filled by drawing on experience.

For instance, malignant cells are able to avoid apoptosis. Apoptosis is the controlled process of cell death in which the material constituting the cell is recycled and no adverse effects occur (in contrast to necrosis, the premature cell death, that has detrimental consequences). Apoptosis is triggered when a cell deviates from normal behavior, as a tumor cell certainly does. Yet apoptosis is abrogated in carcinoma cells and the death signals are ignored (Finzer et al., 2002, 2004). It is observed that the tumor suppressor protein p53 is inactivated in a large fraction of malignant cells on which fact the assumption is based that p53 is a key factor in eliciting the apoptotic machinery (Hanahan and Weinberg, 2000, 62; Balkwill and Mantovani, 2001, 541). This observation does not follow from the general approach but needs to be added to it. A lot of bare facticity is required so as to fill the interstices of the theory-shaped model.

The link between inflammation and cancer suggests a number of approaches to therapeutic intervention. In the framework under consideration, tumorigenesis is promoted by chronic, long-term inflammation. This opens up two therapeutic avenues: first, prevent inflammation from entering a chronic state. Epidemiological studies indicate that the agent Mesalazine (5-ASA), an anti-inflammatory drug for the treatment of inflammatory bowel disease, reduces colorectal cancer incidence in patients using it over extended periods of time (Van Staa et al., 2005; Velayos et al., 2005). Second, given a chronic state, reinitiate acute inflammation. Creating an acute inflammation in the tumor's micro-environment stimulates anti-inflammatory responses that should prompt an immune reaction against the tumor (Lotze and Herberman, 2004, 26–32). Efforts to develop a so-called therapeutic vaccination are intended to achieve precisely this aim. These therapeutic approaches are based on a theoretical understanding of the relevant processes, to be sure, but they only point in a rough direction and do not entail a specific treatment. Wide gaps are left open by the theoretical models that need to be filled with appropriate detail. Although research on therapies is guided by theoretical understanding, the relation between the two is a far cry from the entailment relation that is envisaged by the cascade model. When it comes to working out a therapy, the overarching physiological approach always needs to be spelled out by taking recourse to the particulars of experience. Thus the interactive view is able to capture salient features of cancer research. Local models indeed play a significant role.

Individualized Medicine and the Limits of the Cascade Model

This brief discussion suggests that the interactive view and the local models it features have a place in medical research. There is an additional reason, specific for medical research, that limits the scope of theory-based models and grants much space to variable details. The reason is the diversity of humans at the level of genes and proteins that has become apparent in the past decade and has created the vision of an individualized medicine.

Cancer research is a high point of this vision. Cancer has turned out to be a collection of different diseases. Tumors afflicting different organs often have discrepant cellular properties and manifest symptoms and may respond quite differently to the same treatment. In addition, individual differences between carcinomas of the same type have attracted the attention of medical research. The pathways that cells take to become malignant are highly diverse. Tissue samples of cancer of the same type can differ significantly at the molecular level. For instance, a changed p53 protein may be present in only a fraction of tumors that are histologically identical otherwise. Even the kind of changes among the p53-proteins varies considerably among tumors of the same type (Liu and Bodmer, 2006; Walker et al., 1999). In addition, the order in which certain molecular changes occur in various tumors and the sequence of progression the tumor undergoes differs widely even among tumors of the same type (Hanahan and Weinberg, 2000, 66).

In particular, breast cancer is divided up into a multitude of categories according to the precise pathological constitution of the tumor. For instance, breast cancer is partitioned into hormone-dependent and non-hormone-dependent tumors. The former exhibit a superexpression of hormone receptors and need estrogen and progesterone for their growth. These hormones stimulate cell proliferation by binding with the receptors. The associated treatment proceeds by blocking the hormone receptors using suitable molecules so that hormones lose their effect on the carcinoma. A subclass of the non-hormone-dependent tumors is characterized by the superexpression of the gene HER2. HER2 codes for a cell receptor that activates a pathway leading up to increased cellular reproduction. This type of breast cancer is treated by administering a monoclonal antibody that selectively blocks the receptor in question.

Although this rough overview only suggests a finer array of subclasses within the coarser, symptom-guided category of breast cancer, it goes some way toward an individualized conception of disease. An additional step in this direction is taken by genetic tests. For instance, mutations of the tumor-suppressor genes BRCA1 and BRCA2 have been linked to hereditary breast cancer (Palma et al., 2006); patients afflicted with such gene defects receive a particular treatment (ranging from frequent mammographies to prophylactic mastectomy). Testing for further genes is underway, such as recent attempts to monitor the activity of a larger number of relevant genes in the cancerous tissue under consideration. The test result is supposed to yield an individual expectation value of tumor recurrence in breast-cancer patients and should thus allow for an individually adjusted therapeutic response (Chang et al., 2003).

Accordingly, in more recent approaches to medicine, the individual genetic or physiological makeup of patients is taken into consideration. As a result, diseases that were considered uniform entities previously are now construed as a collection of divergent ailments, with the result that the treatment may be different in each case. This tendency toward individualization in medicine is tantamount to a shift from what might be called a Platonic view toward what could be designated an Aristotelian understanding of disease and its ontology.

Platonism takes general properties and universal features as the basic characteristics of nature and shifts individual variations to the margins whereas Aristotelianism considers individual phenomena as primary and regards generic approaches as expression of pragmatic constraints. In a Platonic understanding in the sense relevant here, diseases are conceived as universal, multiply instantiatable entities: the same disease can be shared by a large number of sick people. This is how we understand illnesses when we say that many patients have contracted the same disease that is epitomized by a specific bacterium or virus. By contrast, Aristotelianism in the sense relevant here might be taken to assume that individual patients and their suffering represent the basic entity. Diseases are collections of such particular illnesses which may bear a relation of family resemblance to one another. That is, there are a number of symptoms and physiological responses is shared between each pair of patients stricken with the same disease, but no general, defining characteristic can serve to collect all instances of a certain disease in one class while excluding counterinstances belonging to other diseases. The reason is the variability of symptoms and physiological indicators associated with a disease and the overlap of such properties among instances of different diseases.

This transformation in the understanding of what a disease is, is reflected in the conceptual nature of the theoretical means used for describing diseases. According to the interactive view, only the conceptual backbone of relevant explanations can be expected to be provided by overarching principles. In particular, the Hanahan and Weinberg vision of medicine as a science in which treatment is derived from first principles cannot be expected to become reality. Put differently, the cascade model is of no avail for illuminating the relationship between theoretical modeling and therapeutic intervention in medical research. Contextual factors like genetic constitution, unwelcome interactions between agent substances and body tissue, age or pre-existing condition may block the pathway from understanding to control.

This context-dependence confirms from a different angle the importance of the space left by the overarching principles for taking account of experience. Medical research suits the interactive view and its emphasis on local models particularly well. The individualized notion of disease matches well with a conception of modeling that emphasizes the space left for the adjustment of models to the particular cases at hand. But the interactive But the interactive view also stresses that theoretical understanding, limited as it is in most cases, is still crucial for intervening reliably and without detrimental side-effects. To be sure, bare facticity will retain its importance in that the enhanced general understanding of a disease at the same time makes us aware of the individual variations. Yet in light of the interactive view, this is a predicament of science as a whole. Understanding in general proceeds by elaborating the generalizable features of the phenomena and filling in the details as the phenomena present them to us.

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Materials as Machines

Bernadette Bensaude-Vincent

Over the past decade the phrase "molecular machine" became a buzzword in biology, chemistry and nanotechnology. Whereas twentieth-century physicists got lots of funds and big machines for exploring the composition of the universe and the ultimate components of matter, in the past two decades public funds have been allocated to making tiny machines. The quest for structural units gave way to the quest for nanomachines performing desired tasks.[1]

The major implication of this shifting goal is that it blurs the boundary between nature and artifact. Every material is characterized by what it does rather than by its molecular structure. Substances that used to be defined by their overall structure are redefined by their performance.

How are we to understand this changing perspective on the molecular world? How individual units of inorganic and organic matter, usually named and characterized by their chemical composition and their inner structures came to be viewed as machines?

The purpose of this chapter is not to tell the whole story. This would require a thick volume as it is a complex process involving a variety of actors such as instruments, concepts, military and economic competition. My ambition is just to convey a view of the conceptual changes that occured over the past 50 years, which radically changed our relation to the material world. For seek of clarity I distinguish three steps in this complex process: (i) first, with the emergence of materials science in the 1960s, material structures have been functionalized; (ii) later on, in the 1980s a systemic approach to materials prevailed over the conventional linear sequence structure/properties/function; (iii) Finally, with the emergence of nanotechnology the functionalization of individual units tends to prevail over systems approach.

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[1] See for instance Kintisch, E. 2006. Embracing small science in a big way. *Science* 13, 29 September 2006, 1872–1873.

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From Structures to Functions

In chemistry, a substance is usually defined by its chemical composition expressed in its name. This was the requirement that presided over the reform of the language of chemistry by Guyton de Morveau, Lavoisier, Berthollet and Fourcroy in 1787.[2] Former names of substances used to refer to their discoverers (Glauber salt) or to their usages in pharmacy (e.g. astringent principle, sedative salt, hepatic liquor), or dyes (e.g. ocher, Prussian Blue). The vocabulary coined by generations of artisans chemists was replaced by systematic names referring to the nature and proportion of elements in the compound. According to Lavoisier, the names should mirror the objective world of susbtances rather than telling anecdotes about the people who discovered them or use them. Since the nineteenth-century the definition of chemical compounds requires not only the elemental composition as expressed in the condensed formulas (for instance, CH_4) but their molecular architecture expressed by their structural formulas.

However in the twentieth century physicists and chemists became more and more concerned with functions. Ironically, the access to structures allowed by new instrumentation was the prime mover. In the interwar period X-Ray diffraction of metal structures helped establish a relation between their microstructure and their macroscopic properties. The determination of microstructure became the prime concern of physical metallurgy and the notions of crystal lattices, of dislocation, of defect, provided a key for understanding the macroscopic properties of metals. The connection between microstructure and mechanical properties was thus probed and the models and theories elaborated by physicists were put at work for designing new materials.[3] Quantum mechanics later provided the theoretical foundations for understanding the microscopic pictures of solids. Solid state became an object of investigation in itself. Solid-state physicists discriminated between the properties depending on the idealized crystal pattern and the properties dependent on "accidents" of the inner arrangement or of the surface of the solid.[4] This focus on structure-sensitive-properties in the study of crystals can be seen as the main pathway, which led to materials science.[5] For instance, Robert Cahn, who pioneered Materials Science in

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[2] Crosland, M.P. 1962. *Historical Studies in the Language of Chemistry*, New York, NY: Dover Publication, 2nd ed. 1978. Bensaude-Vincent, B. 2003. A language to order the Chaos. In *The Cambridge History of Science, Vol. V. Modern Physical and Mathematical Sciences*, ed. M.J. Nye, 174–190. Cambridge: Cambridge University Press.

[3] Smith, C.S. 1959. The development of ideas on the structure of metals. In *Critical Problems in the History of Science*, ed. M. Clagett, 467–498. Madison: University of Wisconsin Press; Smith, C.S. 1963. Four outstanding researches in metallurgical history. *American Society for Testing and Materials* 1–35:11–14; Cahn, R.W. 1987. Solid state physics and metallurgy. In *Solid State Science. Past, present, Predicted*, eds. D.L. Weaire, and C.G. Windsor, 79–108. Bristol: Adam Hillger.

[4] Bensaude-Vincent, B. 2001. The construction of a discipline: Materials science in the USA. *Historical Studies in the Physical and Biological Sciences* 31(part 2):223–248.

[5] Weart, S.R. 1992. The solid community. In *Out of the Crystal Maze. Chapters from the History of Solid State Physics*, eds. L. Hoddeson, E. Braun, J. Teichman, and S. Weart, 617–666, 623. Oxford, New York, NY: Oxford University Press.

Great Britain, describes its emergence as the result of the changes that solid-state physics brought about in metallurgy. This claim rests on the evidence that metallurgy departments in a number of academic institutions were renamed "metallurgy and materials science" around 1960 and a few years later materials science became an autonomous entity.[6]

However this storyline presenting materials science as the "natural" outcome of the autonomous evolution of scientific disciplines is an oversimplification that blurs two major aspects.

First, the discipline named Materials Science emerged in US universities around 1960 as a cluster of several well established departments, like metallurgy, mechanical, chemical and electrical engineering, with more recent fields such as solid state physics and electronics. The label "Materials" put the emphasis on the utility of material structures. The notion of material refers to a substance, which is useful or of value for human purposes. Significantly the American official report on the state of the art in 1975 was entitled *Materials and Man's Needs*. It defined materials as « substances having properties which make them useful in machines, structures, devices, and products. »[7] It insisted that making new stuff useful for something was the main goal of the new discipline. The notion of materials combines physical and chemical properties with social needs, industrial or military interests; they are composites or compromises between natural data and social constraints. Materials blur the boundary between society and nature. Without this emphasis on functionalities the field of Materials Science looses its consistance. Indeed what could be the coherence of a field of research, which includes such diverse subjects as wood, concrete, paper, polymers, metals, semi-conductors, and ceramics? The generic concept of materials encompassing such dissimilar stuffs presupposes that they share a common feature: they are all materials for. . ..[8] Their commensurability is based on the functional equivalence of certain structures and properties for a specific function.

Second, from this coupling of natural and human aspects embedded in the definition of materials follows one characteristic feature of Materials Science. Knowing

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[6] Cahn, R.W. 2001. *The Coming of Materials Science*. Amsterdam, New York, NY: Pergamon. See also interviews of materials scientists on the website http://www.sfc.fr/Material/hrst.mit.edu/index.html

[7] National Academy of Science. 1975. *Materials and Man's Needs, Supplementary Report of the Committee on the Survey of materials science and engineering (COSMAT),* Washington, DC, Vol. 1, 3–1.

[8] From an epistemological stand-point the generic notion of materials is an oxymoron. Matter is a generic and abstract notion, whereas the notion of material refers to singular entities. Historically the construction of an abstract and general concept of matter conditioned the science of nature in general. Otherwise physics would be a "zoology" of materials, like stamp collection. Moving beyond the multiplicity and the variety of individual and phenomenological substances was the key to "modern science". In other terms, materials were an obstacle that had to be overcome. This notion associated to concrete, sense qualities, and to human interests is a typical example of what Gaston Bachelard called "epistemological obstacles" in *La formation de l'esprit scientifique* (Paris, Vrin, 1938). It thus seems that natural science had to give up the study of materials (or to leave it to chemists!) in order to become a rational and mathematical science.

and producing are never separated. Cognitive purposes and the technological interests are intertwined. Materials science couples scientific research with engineering application of the endproducts. In the USA this domain is known as « materials science and engineering » (later abbreviated as MSE) and the report of the National Academy makes it clear that «one should speak of materials science and engineering as an *it* rather than *them.”*[9] The same report defined MSE: « Materials science and engineering is concerned with the generation and application of knowledge relating to composition, structure, and processing of materials to their properties and uses. »[10]

Although the 1975 report on *Materials and Man's Needs*, referred to an abstract *homo sapiens* more concrete needs prompted the emergence of materials science. Looking at what occured at the institutional level, we see that the plural entity « materials » first appeared in the language of science policy makers, under the auspices of a bottleneck for advances in space and military technologies. Whereas during World War II, the critical needs were still addressed in terms of one strategic material (synthetic rubber, or plutonium for instance), in 1957, when the response to Sputnik encouraged heavy investments in ambitious space programs, the U.S President Science Advisory Committee singled out materials in general as a priority. The idea that all materials were strategic emerged in the context of the Cold War as a means for building up sufficient industrial capacity for future emergencies.

Through its Advanced Research Project Agency (ARPA), the Department of Defense (DoD) developed contracts with a number of universities for developing new materials fitted for space engines. The program was twofold. It included generous funding of university research with the intention of military exploitation, thus providing academic scientists with equipment that they could never afford. It strongly encouraged interdisciplinarity through interdisciplinary labs modelled after the Nuclear and Electronics Labs. Instrumentation acted as another major driving force that orientated research on materials along with social and military needs.

Thus the emergence of the generic concept of materials resulted from military and political pressures for identifying the potential functionalities in material structures rather than from a smooth disciplinary evolution toward science-based technologies. In fact, materials scientists were able to meet the social demands. They have designed high-performance materials with never-seen-before resistance to high temperature and chemical corrosion for spacecrafts. They managed to achieve this goal by developing a new approach, known as “materials by design”. Given such and such functions to be performed by the wing of this airplane, design the best structure combining the set of properties required for performing those functions. The requirements list moves from function to properties and finally to structure. Thus function became the priority in the design process while the material itself is

[9] National Academy of Science. 1975. *Supplementary Report of the Committee on the Survey of Materials Science and Engineering (COSMAT).* Washington, DC, Vol. 1, 1–3.
[10] Ibid., 2.

the outcome. The material is no longer a prerequisite imposing constraints on the process of design, it is designed for specific performances.

In fact, chemical industries, especially designers of synthetic polymers, pioneered the age of materials by design because they experienced that actual materials are never meant to perform one single function. Rather they have to perform a set of functions involving mechanical, thermal, electrical properties.... As conventional organic polymers did not present properties such as high-temperature stability, electrical or thermal conductivity, that could expand their market, chemical engineers developed composite materials with desirable properties.[11] Those materials made of resin matrix and reinforcing fibers required a new specific approach to their design. Emphasis was put on performance, since they are tailored for a specific task in a specific environment. In contrast to conventional materials having standard specifications and a global market, composites created for aerospace and military applications are developed according to the functional demands of the end-product and the services expected from the manufactured product. Instead of supplying commodities to be finalized by the customers, composite materials are the end-products of a cooperation between customers and suppliers.

From Materials to Systems Approach

Although the first generation of materials scientists and engineers replaced the age-old design paradigm – what sort of artifact can be achieved with available materials – in favor of the inverted perspective, where the goal is set in advance and the necessary means are developed, they nevertheless kept a linear model. And in most textbooks, the linear sequence -given a set of functions let's find the properties required and then design the structure combining them- still constitutes the conceptual core of materials science, which characterizes the way of "thinking materials". However, a number of materials engineers developed a new approach to materials in the 1980s. The triangle of basic notions -structure, properties, and performances – left behind one parameter, which is essential for engineering materials: the processing.

Once again, the new way of "thinking materials" was encouraged by the social context in the USA. In the early 1980s, international competition prompted a radical change in US science policy responding to what the challenge of materials processing and manufacturing from abroad. In order to foster industrial innovations and rival with Japan, the Bayh-Dole University and Small Business Patent Act established a uniform patent policy for all federal agencies in 1980.[12] The aim of this

[11] Bensaude-Vincent, B. 1997. *Eloge du mixte, Matériaux nouveaux et philosophie ancienne*. Paris: Hachette Littératures.

[12] The Bayh-Dole University and Small Business Patent Act of Dec 12, 1980. Henderson, J.A., and J.H. Smith. 2002. *Academia, Industry, and the Bayh-Dole Act: An implied Duty to Commercialize*, CIMIT, October 2002.

legislative measure was to encourage collaborations between industrial companies and universities, to have academic inventions developed for the market. It even imposed a duty for all researchers working under contracts with the government to pursue the commercialization of their government-funded inventions. Thus, after the blurring of disciplinary boundaries in the 1960s, the boundary between academia and business was also blurred in the 1980s.

This institutional change had an interesting epistemological impact on MSE. Academic material scientists repeatedly urged for a reorientation of research from the quest for new materials towards processing. MIT, with a long tradition in engineering and applied science, was at the forefront of the re-orientation. In the late 1970s, Thomas Eagar argued that it was more important to improve processes than to invent new materials. He worked hard and successfully to create the Materials Processing Center, inaugurated on February 1, 1980.[13]

"Designing new materials with curious properties is fun for the materials scientists and engineer but it does not often yield results of major commercial or social benefit. American companies must spend their resources learning how to manufacture existing materials economically, not searching for exciting new materials. But if we spend our resources on processing selected new products of high reliability and low cost, we will all be winners."[14]

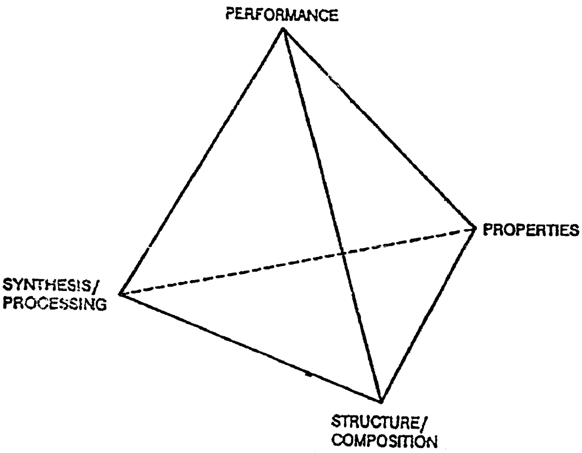
The attention to processes required that the traditional linear approach give way to a systems approach. Any change made in any of the four parameters – structure, properties, performances and processes – can have significant effect on the performance of the whole system and may require a re-thinking of the whole material. Merton C. Flemings, who served as the first Director of the Materials Processing Center at MIT, from 1980 to 1982, used to visualize the holistic way-of-thinking materials by drawing tetrahedron, with structure, properties, performances and processes at the four summits.[15] He thus suggested the need of continuous feed-back loops between the various specialists involved in their design. Processing affects a material's performance, but the required performance often determines the processing employed; processing affects structure, but structure determines what type of processing is chosen, etc. Since 1989 the tetrahedron has occasionaly been used in both research reports and textbooks.[16]

[13] MIT Office of the President Records 1943–1989, AC 12, Box 87.

[14] Eagar, T.W. The real challenge in materials engineering. In *Materials Revolution. Superconductors, New Materials and the Japanese Challenge*, ed. T. Forester, op.cit. supra., 241–253, quot. on p. 253.

[15] The first published occurrence of the was in National Research Council, *Materials Science and Engineering for the 1990s*. Report of the Committee on Materials Science and Engineering. Washington, DC: National Academy Press, 1989. See also Flemings, M.C., and R.W. Cahn. 2000. Organization and trends in MSE education in the US and in Europe. *Acta Materialia* 48:371–383.

[16] For instance, Allen, S.M., E.L. Thomas. 1999. *Structure of Materials*. New York, NY: Wiley.



Thus systems approach assisted with all sorts of computer softwares became a standard practice in the design of new materials and the teaching of MSE. In this perspective, materials are the products of a complex process of repeated backs and fros between structure, properties, performances, and process rather that the result of a linear reasoning. They can no longer be described as functional structures, or more precisely as functions embodied in molecular structures. As much as creations of the mind they are created by chemical and physical forces operating on surfaces and volumes of material reagents.

From Systems to Machines

Four decades after the foundation of the earliest materials departments, and despite the proliferation of materials centers and materials teaching programs, there is no evidence that Materials Science is reaching the stable state of a "mature" discipline.[17] Since the 1990s, nanotechnology has emerged as a fulcrum of science policy. In a way, nanoscale science could reinforce the coherence and vitality of MSE for at least three major reasons: (i) nanoscience is also a generic concept. Virtually all sorts of materials can be nanostructured; (ii) nanoscale research is as interdisciplinary as MSE and also develops across the border between science and technology; (iii) like MSE, nanoscience has been driven by instrumentation. Whereas in the 1980s chemists had shifted their attention from atoms and molecules to condensed phases in order to design new materials, in the 1990s they shifted back

[17] Bensaude-Vincent, B., and A. Hessenbruch. 2004. Materials science: A field about to explode? *Nature Materials* 3(6) June 2004:345–346.

towards individual molecules thanks to the availability of near-field microscopes STM and AFM.[18]

However the current expansion toward the nanoscale brought about new research interests. In the 1980s, materials scientists were aiming at designing smart materials or intelligent materials (systems responding to their environment). Nanotechnologists are striving to design tiny machines, molecular motors, rotors, levers, wheels, switches....[19] At the nanolevel, machine seems to be a more relevant notion than system. Molecules or macromolecules are no longer viewed as constituent elements for synthesizing materials. They are viewed as artifacts performing specific tasks. This new perspective came to prevail in the 1990s, prior to the launching of nano-iniatives in the USA in Europe and Japan. By contrast to what has been described for materials science, the conceptual shift from systems to machines was not prompted by new science policy measures. Rather it seems that the notion of molecular machines, as advertised by Eric Drexler in *Engines of Creation*, for instance, was attractive enough to foster national initiatives and big investments.

For lack of in-depth study on the rise of the machine metaphor, I just point to its possible niches. It seems to me that it came to prevail simultaneously in two different communities: among the engineers who promoted the concept of molecular manufacture and among molecular biologists who have been using the program metaphor for decades.

In a way, the engineering milieu was very near the community who developed systems approach in materials science. Drexler was still at MIT in 1986 when he published *Engines of Creation*. But there is a variety of sub-cultures at MIT and the community Drexler belonged to did not share the same culture nor the same values as the Chemical Engineering Department. Drexler was working in Marvin Minski's Lab, in the Artificial Intelligence (AI) department. Although both laboratories were founded in the same period and equally claimed to be interdisciplinary,[20] the AI Lab was full of computer programmers while the MSE lab was headed by chemical engineers trained to work on materials processing. The AI Lab was interested principally in research on neuronal networks, vision, language, mechanical motion and manipulation, which they view as the keys to more intelligent machines. In the 1970s, Minsky developed with Seymar Papert « The Society of Mind Theory », based on the assumption that intelligence emerges from non intelligent parts.

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[18] Mody, C. 2004. How probe microscopists became nanotechnologists. In *Discovering the Nanoscale,* eds. D. Baird, A. Nordmann, J. Schummer. Amsterdam: IOS Press, 119–133. Baird, D., A. Shew. *Probing the History of Scanning Tunnelling Micrsoscopy*, ibid. pp. 145–156.

[19] Bensaude-Vincent, B., and X. Guchet. 2007. Nanomachine: One word for three different paradigms. *Têchne, Research in Philosophy & Technology*, 10, n°4, Fall 2007, 71–89.

[20] Research at MIT in the field of artificial intelligence began in 1959. In 1963, the (then) "AI Group" was incorporated into the newly-formed Project MAC, only to split off again in 1970, as the MIT Artificial Intelligence Laboratory. In 2003, the AI Lab (as it is commonly abbreviated) was merged with the Laboratory for Computer Science, the descendant of Project MAC, to form CSAIL.

The contrast between the rhetorics surrounding MSE and AI is striking. To be sure, materials scientists just as Drexler often use the phrase "the coming of a new era". In their case, it is a new era of science-based engineering that would replace ages of empirical practices of design. They claim, for instance, that they are « designing a new world » by combining the information pool generated by reductionist analysis with the component of design for which the systems approach was crucial.[21] However MSE was never presented as the exploration of a brave new world. The pragmatic turn dominates. Industrial competitiveness, energy and "social needs" are the main legitimation of research investments. By contrast in the Artifical Intelligence community science and science-fiction are closely associated and grandiose visions about a brave new world are ordinary language. Minski acted as an advisor for the movie *2001 A Space Odyssey*, and later co-authored a science fiction thriller *The Turing Option* about a superintelligent robot.

In this context, the nanomachine is an ideal mechanism designed from bottom-up. Given a specific function to perform – such as assembling or rotating – design the molecular structure that will be able to perform it. The performance is divided up into elementary mechanical operations – such as pick and place, or rotate, for instance; then an appropriate molecular structure has be found to perform each unit operation; and eventually all the parts will be assembled. Drexler described molecules as rigid building blocks, similar to the parts of tinker toys to be assembled like the elements of Lego construction sets. The functions performed by the various parts of molecular machinery are also essentially mechanical. They position, move, transmit forces, carry, hold, store, etc. The assembly process itself is described as a "mechanosynthesis", positioning the components with a mechanical control. In Drexler's molecular manufacture the process of design is linear and his nanomachines are just embodiements in materials structures of the designer's intentions. They are creatures of the mind prior to be materialized. In fact Drexler's molecular assemblers have been proved unfeasible by a number of scientists who raised so many objections that his view of nanomanufacture appears as unrealistic fiction.[22] However his model of bottom-up process is far from being discredited. Molecular machines are currently designed to perform mechanical functions as well as logical functions that use the power of the Scanning Tunnelling Microscope to visualize and manipulate atoms and molecules.[23] They all follow the same conventional design

[21] Olson, G.B. 2000. Designing a new world. *Science* 288:993–998 (12 May 2000).

[22] Smalley, R. 2001. Of chemistry, love and nanobots. *Scientific American* 76–77 (Sept 2001). And Drexler, E. lettre ouverte au Pr. Smalley, *Nanodot*, April 20, 2003 (www.foresight.org/NanoRev/Letter.html). Whitesides, G.M. 2001. The once and future nanomachine. *Scientific American* 78–83 (Sept 2001). Jones, R.L. 2004. *Soft Machines*. Oxford, New York, NY: Oxford University Press.

[23] A typical example is the collaborative work of an IBM Zurich team and CEMES in Toulouse on molecular wheel. See Gimzewski, J.K, C. Joachim, et al. 1998. Rotation of a single molecule within a supramolecular bearing. Science 281(5376):531–533 (July 24, 1998).

principles with an outsider designer (like a clockmaker) planning and simulating the machine prior to its embodiement in a material structure.[24]

Molecular biology is an alternative source of inspiration for nanomachines. Nanotechnology and molecular biology seem to share the same epistemological credo: that each material unit, an invidual molecule or macromolecule is a device performing a specific task or operation: moving, rotating, copying or computing in the case of logic machines. Enzymes, and proteins are redefined as biological machines. More generally the cell is viewed as a kind of factory or warehouse full of small machines. S. Zhang from MIT, describes its components by analogy with current human technologies: ribosomes are assembly lines, ATP synthase is a generator, protease a bulldozer, polymerase a copy machine, etc.[25]

Such analogies did encourage research initiatives at the intersection between nanotechnology, biophysics and biotechnology. For instance, the Bioengineering Nanotechnology Initiative launched in 2002 by the US NIH is committed to integrating engineering and physical sciences with the life sciences. Understanding the ways of nature and exploring new technological avenues for health care merge into one single research program. In this program, it is tacitly expected that the access to the "fundamental" level secured by molecular biology will provide us with THE bottom-up method that nature and technology can share.

Synthetic biology is another booming field based on the view of living systems as a collection of tiny machines. Its goal as charted in the first Conference held in 2004 is "understanding and utilizing life's diverse solutions to process information, materials and energy.[26] In vivo and in vitro programs of synthetic biology may differ in their strategies and technological potentials, but they both rely on the same basic view of living systems as collections of structural units – such as DNA sequences – performing a specific function. In keeping with Francis Crick's central dogma, each function is dependent on one structure, and the relation is oneway. Since the basic structural elements are few, in vitro synthetic biology strives to make cell-free syntheses of DNA, RNAs, proteins. An ultimate goal would be a library of independent and interchangeable parts ("Registry of Standard Biological Parts") and to take advantage of the multitude of possible combinations between them with great flexibility to perform a specific function everywhere. The goal is to take advantage of the multitude of possible combinations to help make biomaterials as well as understand the origin of life. The program may be feasible but it will presumably stumble on a major obstacle, the collective behaviors of biological units in cell environments. In brief, there is no place for any function that is not assignable to a specific unit.

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24 See Bensaude-Vincent, B., and X. Guchet. 2007. Nanomachine: One word for three different paradigms. *Technê, Research in Philosophy & Technology,* 10, n°4, Fall 2007, 71–89.

25 Shuguang, Z. 2003. Fabrication of novel biomaterials through molecular self-assembly. *Nature Biotechnology* 21(10):1171–1178 (Oct 2003).

26 *Nature* vol. 438, 24 November 2005, 417–18. Foster, A.C., and G. Church. 2007. Synthetic biology projects in vitro. *Genomic Research* 17:1–6.

As long as such programs tend to capture an essential structural element and rely on it while neglecting all the messiness created by molecular agitation at the nanoscale, they are not really leading to a new technological paradigm. Whatever the achievements and promises of synthetic biologists in nanomedicine, from a philosophical perspective they look extremely conventional.

In conclusion materials are not just "epistemic things". They are material agencies characterized by what they do or perform. Just as biological materials, synthetic materials are multifunctional and designed to work in a messy, noisy openworld. By contrast the molecular machines or nanorobots so far designed by scientists and engineers are products of the mind, materialized principles or reified views. They perform only one task and only in highly protected laboratory conditions.

I have argued that the different views of materials as systems and materials as machines do not refer to a contrast between engineering and science. Rather the contrast is between two engineering cultures, the culture of chemical engineers and materials scientists on the one hand, and the culture of information technology, Artificial Intelligence, synthetic biology on the other hand.

I have emphasized the social and economic agendas that prompted the emergence of Materials Science and the shift from the linear view of materials as structures leading to functions to the holistic view of materials as systems, with a view of counteracting the standard deteminist accounts told by many actors of the field. The same could be done for the standard accounts of the emergence of nanotechnologies, based on Feynman's famous prophetic words "There is plenty of room at the bottom". However in the latter case, the concept of nanomachine anticipated and possibly stimulated financial investments in nanotechnologies.

Looking at how it worked could tell us something about how it could work. This too brief survey of the shift from materials to systems then to machines could raise skepticism about the future of the dominant paradigm of materials as machines. Instrumentation could again act as driving force for shifting again the attention of nanoscientists and biologists towards systems. In particular a promising pathway has been opened up by Femto second spectrometers and electron microscopes, that provide access to the dynamics of chemical reactions.[27] There is presumably more to explore and exploit in materials and biomaterials than suggested by the current view of nanomachines.

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[27] Ahmed Zewail, who was awarded the Chemistry Nobel prize in 1999 for his study of the transition states of chemical reactions using femto second spectroscopy claims that he is opening a new era: whereas molecular biology rests on Francis Crick's dogma, i.e. "if you want to understand function you have to understand structure", now functions are directly accessible through interpreting dynamics.

Part II
Changing Conditions of Scientific Research: The Role of Instruments

Holism and Entrenchment in Climate Model Validation

Johannes Lenhard and Eric Winsberg

Recent work in the domain of the validation of complex computational models reveals that modelers of complex systems, particularly modelers of the earth's climate, face a deeply entrenched form of confirmation holism. Confirmation holism, as it is traditionally understood, is the thesis that a single hypothesis cannot be tested in isolation, but that such tests always depend on other theories or hypotheses. It is always this collection of theories and hypotheses *as a whole*, says the thesis, that confront the tribunal of experience. But in contrast to the way the problem of confirmation holism is typically understood in the philosophy of science, the problems faced by climate scientists are not merely logical problems, and nor are they confined to the role of anything that can suitably be called auxiliary hypotheses. Rather, they are deep and entrenched problems that confront the scientist who works with models whose component parts interact in such a complex manner, and have such a complex history, that the scientist is unable to evaluate the worth of the parts in isolation.

In what follows, we want to argue for two central claims about complex computational models – with a particular emphasis on models of the earth's climate. The first claim is about *holism*. We will argue that recent efforts in the sphere of climate model *inter-comparison* reveal that modern, state-of-art climate models are what we call "analytically impenetrable." We will spell out this notion with more care, but the intuitive idea is that, as a practical matter, it has become impossible for climate scientists to *attribute*[1] the various sources of relative successes and failures to particular modeling assumptions.

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[1] The word "attribution" also occurs in the prominent phrase "attribution of climate change" which stands for the question whether observed climatic change is caused by humans. We do not use the word in this way in this paper.

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The second claim is about *entrenchment*. In particular, we argue that entrenchment can be identified as one of the principal causes of holism. Here, we want to argue that climate models are, in interesting ways, products of their specific histories. Climate models are developed and adapted to specific sets of circumstances, and under specific sets of constraints, and their histories leave indelible and sometimes inscrutable imprints on these models.

The validation of complex computational models is the central issue of the epistemology of computer simulation. The computer science literature often distinguishes between verification and validation as two aspects of the evaluation of simulation. We will speak somewhat more coarsely and treat validation and evaluation as the same. How do we know when a complex computer model is good enough, or reliable enough, for a task for which we hope to depend on it? The issue of the validation of simulations is a particularly interesting one for the epistemology of science, because issues of validation take center stage in simulation in a way in which they rarely do in other modalities in the sciences. It brings to light features of the epistemology that might be absent, but more likely simply hidden, in other modeling and theoretical practices.

To a first approximation, we can think of the validation of a model in the following way: a model is validated when we are convinced that there is an appropriate fit[2] between the dynamics of the model, on the one hand, and the dynamics of the real world system to be modeled, on the other. To be sure, such a conception of the validation of simulation models is somewhat simplified. In particular, simulations are often used to generate predictions about phenomena in domains where data are sparse. Hence, while appropriate fit is of course what we want in a model, we want more than fit with those features of the real world system that are immediately observationally accessible to use. That a model is valid, therefore, is rarely established solely by comparing it to the world. As we have argued elsewhere (Winsberg, 1999, 2001), the sanctioning of simulation models depends on a number of features in addition to fidelity of the simulation's output to known real-world data. It also depends on fidelity to theory, to accepted computation method, and a host of other factors. In this paper, however, we want to set these complications aside, and focus, in particular, on the role of comparison with data in the validation of simulations. We also want to focus, in this paper, on a particular facet of validation. We want, in particular, to think about situations in which models fail to be adequately validated – at situations, in other words, where the behavior of the model is known *not* to be close enough to the behavior of the world for its intended purpose. Deviation from desired model behavior, then, remains a topic to be dealt with.

This, after all, is the state of affairs known to obtain with regard to most global climate models. There exist a dozen or so of such models run by research centers worldwide. Each has its specific strengths and weaknesses in certain respects. The series of assessment reports of the Intergovernmental Panel on Climate Change

[2] "Appropriate" in the sense that, for the intended purpose of the model, the model is close enough to the world in the intended respects and to the intended degree of accuracy.

(IPCC) documents how adequacy of the overall picture is thought to be produced by a synopsis of a plurality of models. In such cases, the issue of model validation is, in effect, the issue of *model improvement*: The purpose is not to confirm that one particular model exerts an adequate fit, but rather to improve the predictions of a single model by comparing them with those of other models. To put the central question succinctly: when a complex models fails to be adequate, is it possible to identify the various components of the model that contribute to its relative successes and failures?

It is precisely in these contexts, however, in which a serious form of confirmational holism rears its ugly head. The problem of confirmational holism, or the so-called Quine-Duhem problem, can be illustrated by an example. Suppose that we have the hypothesis that all metal rods expand when heated. An alleged falsification of this hypothesis comes from the observation of a rod being heated and not expanding. Confirmational holism comes from the realization that such an observation's credibility depends on a sound understanding, grounded in certain theories or hypotheses, of thermometers and measuring instruments. Any seeming conflict between our original hypothesis and our data could either be the fault of the original hypothesis, or it could be the fault of these auxiliary hypotheses – hypotheses associated with measuring instruments.

In his original work of 1914, Pierre Duhem expressed this as his non-falsifiability thesis: "if the predicted phenomenon is not produced, not only is the questioned proposition put into doubt, but also the whole theoretical scaffolding used by the physicist" (1954, 185). Furthermore, the experiment tells that there is something wrong, but doesn't tell where the error comes from (loc. cit.), hence the doubt is necessarily holistic. No single hypothesis can be tested in isolation "To seek to separate each of the hypotheses of theoretical physics from the other assumptions upon which this science rests, in order to subject it in isolation to the control of observation, is to pursue a chimera" (1954, 199–200). The term "holism" actually goes back to Willard V.O. Quine who referred to Duhem in his argument against reductionism put forward in his famous "Two Dogmas" (1953).[3] There, he makes stronger claims than Duhem about the impact of holism, especially that "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system" (1953, 43). However, the differences between Duhem and Quine are not important for our purposes here.

On a common understanding of the Quine-Duhem problem, and of confirmational holism, evidence never dictates whether a single hypothesis or theory is confirmed or falsified by a collection of data. Although this point is logically valid, it is usually supposed that good judgment (what Duhem called "bon sens") can decide between such rival possibilities. It is usually supposed, in other words, that the Quine-Duhem problem is a philosophical problem without actual practical implications for the working scientist.

[3] The first version of Quine's article (1951) did not mention Duhem.

In climate modeling a somewhat special Quine-Duhem problem occurs. The holism that arises here is wholly independent of whatever hypotheses or theories sanction the reliability of the observational base upon which validation occurs. Even in situations in which the reliability of the data against which simulation output is being compared are not in doubt – that is, even if we imagine a situation where, for example, the data concerning historical record of ice-ages against which the simulation's output will be compared are not open to question – where there is no concern about the reliability of the auxiliary hypothesis used to generate these data – there is still a serious problem of confirmational holism.

Climate Simulation

Suppose, for example, that we have a computer simulation of the climate whose simulated dynamics can be compared to its real world counterpart – our planet's climate – in at least important respects. An iconic example of this kind of comparison is the purported fit between the history of the global mean temperature and the output of various global climate models, applied to the past. Figure 1, from the IPCC's Fourth Assessment Report, is a recent example.

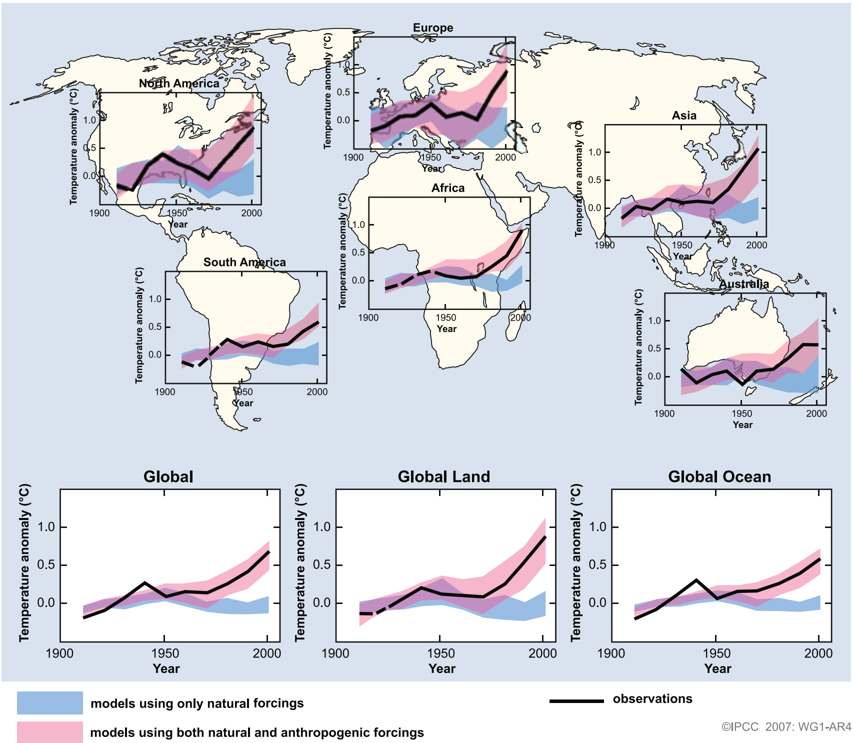

Fig. 1 Climate Change 2007: The Physical Science Basis. Working Group I Contribution to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change, Figure SPM.4. Cambridge University Press. Courtesy of IPCC

Of course, the "real world" side of the comparison – the historical mean temperature – is itself a re-construction out of a vast array of different sources. It is, to use a bit of technical terminology coined by Woodward and Bogen, a *phenomenon* – a highly massaged and negotiated description of the behavior of the world that is *inferred* from a variety of sources (Bogen and Woodward, 1988). A corresponding vast array of theoretical and instrumental resources stands behind the line on the graph that is labeled "real world climate." And of course, whether agreement or disagreement between model and world count as evidence for or against the model depends entirely on the credibility of the data conferred by those resources. As we noted earlier, the Quine-Duhem problem, and the problem of confirmational holism, is typically thought to be about these very theoretical resources that stand behind the inferences to these "phenomena." But we shall not be concerned with those issues here – we are more concerned with issues related to the relationships between the models themselves, on the one hand, and the fully reconstructed "phenomena," whatever they turn out to be, on the other.

When it comes to climate models, one cannot overemphasize the degree to which the credibility and assumed reliability of the models comes precisely from the good fit between the output of these models and this reconstructed historical record. Figure 2 displays a case of such output as reported in IPCC's fourth Assessment Report (2007).

A variety of political, economic, and policy scenarios is part of this complex picture (d). Graphics (a)-(c) display a variety of scenarios that determine the boundary conditions of the simulations. Climate scientists, themselves, of course, are not in the business of making political and economic forecasts. What they do, instead, is to

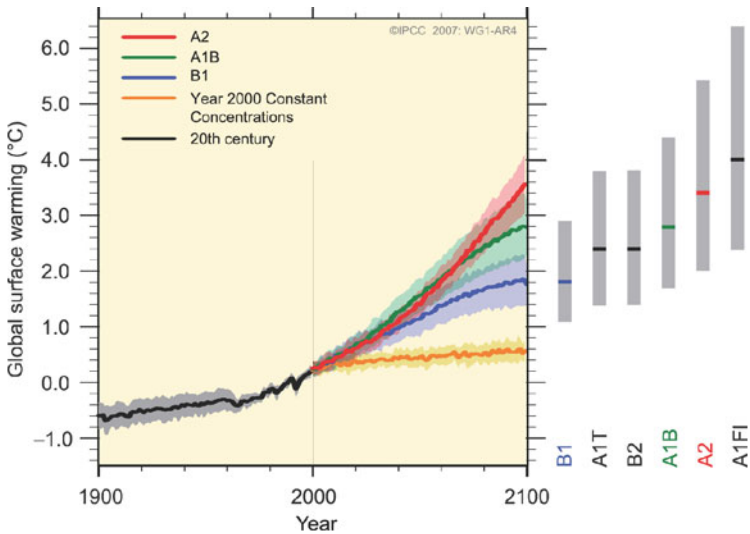

Fig. 2 Climate Change 2007: The Physical Science Basis. Working Group I Contribution to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change, Figure SPM.5. Cambridge University Press. Courtesy of IPCC

make a variety of simulation-based predictions of global temperature using a variety of particular assumptions about future greenhouse gas emissions. This accounts for part of the uncertainty in the predictions. There is still, however, another kind of uncertainty lurking in the background of part (d). Even for one particular scenario – one set of assumptions about economics, politics and policy (in short, about emissions) – a range of possible values is reported, not a single predicted temperature. This range stems from a plurality of individual models that can be run given one particular political/economic scenario. Each of these models gives a different forecast and the collection of forecasts gives the range reported here. The entire process is much like that of "collecting all the opinions of valuable witnesses". First you canvass political/economic/policy experts about what to expect in terms of emissions, and then you canvass a second panel of expert – the models – about what kind of climate change to expect in response. The first kind of plurality is generally appreciated by science and the public; at the same time it is acknowledged to be irreducible – there are simply no trustworthy oracles to consult about emissions. However, the second kind of plurality is the more important and at the same time more hidden one. And it is precisely this aspect of the uncertainty that climate scientists would like, in principle, to reduce.

The earth's climate can be thought of as system consisting of a variety of subsystems: the atmosphere, oceans, ice cover, etc. The overall climate dynamics is brought about by the interaction of all these subsystems. Climate models, in turn, are correspondingly modular. There are model modules for the oceans, ice cover, cloud formation, rain dynamics, etc. And so one way to think about climate model improvement is in terms of the contributions to model output that come from these various modules. However, the approach to improve each module separately has to face serious and even insurmountable problems.

Some words about modularity and climate models. The historical origins of climate analysis are rooted in models of the circulation of the atmosphere – general circulation models (GCMs) that have been developed since the mid 1950s. The theoretical core of these models is built by the so-called fundamental equations, a system of partial differential equations from the physics of motion and thermodynamics. With the growing interest in climate change in the 1980s, a process of substantial growth of these models was starting, because more and more facets of the climate system had to be included while aiming at a comprehensive picture. The growth both included the resolution of more sub-processes, like the dynamics of aerosols in the atmosphere, and also the addition of sub-processes in parameterized form. In short, there exists a large variety of paths of growth and the different climate models followed different paths during their development.

Modularity and Pluralism

One aspect of the development of more comprehensive models is of particular importance. A multitude of sub-models had to be included into the atmospheric GCMs that had little to do with the theoretical physical basis of the atmospheric circulation, e.g. ice cover, circulation of the oceans, or land use. The coupling of atmospheric and oceanic circulation models is recognized as one of the milestones of climate modeling because both components had their independent modeling history, including the independent calibration of model performance. Putting them together was a difficult task because the performance of the two sub-models now interfered one with the other. Today, atmospheric GCMs have lost their central place; coupled models entertain a deliberatively modular architecture and comprise a number of highly interactive sub-models (cf. Küppers and Lenhard, 2006). The results of these modules are not gathered independently and after that get synthesized, rather data are permanently exchanged between all modules during the runtime of the simulation. Thus the overall dynamics of one global climate model is the complex result of the interaction of the modules – not the interaction of the results of the modules.

Against this background of the modularity of climate models we want to describe the problem of validation, i.e. the question of how well a model simulates the actual climate dynamics. One particular model can of course be compared with certain aspects of the observed climate history. The most prominent one is the global mean temperature. The model can simulate its course over past times and the output can be compared with the reconstruction of climate (temperature) history. Paul Edwards (2001) has pointed out that this reconstruction cannot be derived from data directly but depends on models in various ways. However, as has been said, we simplify matters, neglect the issue of conformational holism in the traditional sense and assume that this reconstruction is straightforward.

But GCMs can also be checked against more local and recent patterns, such as the intensity of tropical winds, precipitation patterns, etc. Relatively speaking, these comparisons are a straightforward validation strategy that can assess systematic errors of the simulation model and enables stepwise improvement. This strategy is well established in simulation modeling practice.

A central problem arises, however, as the complexity, multi-dimensionality and modularity of the models grow. An achievement with respect to one metric of model comparison, produced by complicating the model with a new feature, say a tropical precipitation adjustment, or by substituting one module with another, may not lead to amelioration with respect to another metric or may even make comparisons on that metric impossible or meaningless. Changing the model in such and such a way may improve prediction of tropical winds, but it may simultaneously degrade prediction of precipitation patterns.

There are, furthermore, many possible avenues to pursue for improving model performance. Each modeling group follows their own path. In the end, there is a variety of GCMs on the market: major climate research institutions tend to have one or even several of their "own" GCMs. And each one has its characteristic successes and failures. Adopting a John-Stuart-Mill-kind of view, this plurality can be seen as a virtue to foster competition and to end up with even better results even if unanimity is not attained.

Wendy Parker, in her recent paper "Understanding Pluralism in Climate Modeling", presents us with an illuminating discussion of model pluralism along these lines. She acknowledges that the up-to-date complex climate models cannot be compared in a straightforward manner: "they represent physical processes acting in climate system in mutually incompatible ways and produce different simulations of climate."(350) That means, according to Parker, that modellers have different opinions of how to represent the relevant physical processes. Furthermore, she rightly remarks, there are insufficient data to be able to resolve the plurality using criteria of empirical adequacy. Here, we would simply like to direct attention towards an additional cause of the observed plurality.

On Parker's analysis, mutually conflicting assumptions lead to what she calls an "ontic competitive pluralism" (362). This account at least implicitly suggests that we can accurately identify the causes of the various differences in models outputs in terms of the differences in the assumptions the model authors make about physical processes. It is precisely this, however, that we want to deny. We think this view oversimplifies matters and we will argue that incompatibility is brought about by the very process of complex computational modelling. Our claim about conformational holism is in effect a scepticism about whether the researchers are really able to identify these cause. And thus, we are suggesting there are simulation-specific reasons, reasons having to do with the ways in which computation models are actually implemented, as opposed to reasons having simply to with basic climate science, for model pluralism. Thus confirmational holism is making the multi-model approach unavoidable and is brought about more by the exigencies of dealing with complex simulation models than by rational, though conflicting, choices of researchers.

Analytical Understanding Impossible

The complex internal composition and massive modularity of climate models is the principal source of the problem. Climate models are made up of a variety of modules and submodels. There is a module for the general circulation of the atmosphere, a module for cloud formation, for the dynamics of sea and land ice, for effects of vegetation and many more. In addition, each of them includes a mixture of principled science and parameterizations. And it is the interaction of these components that brings about the overall observable dynamics in simulation runs.

Putting the modules together, moreover, is no easy task. Typically, the specific form of the model that integrates these submodels is crafted over a long process of piecemeal mutual adjustments of the parameters, changes in parameterization schemes, and algorithmic implementations of the different components. The course of development of these models is close to organic – it would not be a stretch to liken to their development to an evolutionary process. Like in evolution, function is optimized to the particular circumstances, the particular data sets available for comparison, and particular criteria of evaluation, under which optimization occurs.

We argue that the best way to understand the historical nature of GCM optimization is in terms of a concept introduced by William Wimsatt in his recent book: that of "generative entrenchment". Wimsatt's discussion of this concept arises in the context of understanding how techniques from adaptive design function as “a way of increasing the reliability of structures built with unreliable components” (Wimsatt, 2007, 133) According to Wimsatt: “Adaptive design is a layered organization of kludged adaptations acquired sequentially and assembled on the fly. . .” (2007, 133).

The term “kludge” or “kluge” initially stems from programmers’ colloquial language and is an extremely useful one here. Andy Clark stresses the important role played by kludges in complex modular computer modelling in general. A kludge is “an inelegant, ‘botched together’ piece of program; something functional but somehow messy and unsatisfying”, it is – Clark here quotes Sloman: “a piece of program or machinery which works up to a point but is very complex, unprincipled in its design, ill-understood, hard to prove complete or sound and therefore having unknown limitations, and hard to maintain or extend” (Clark, 1987, 278).

Kludges have been incorporated into the body of philosophy of science by scholars like Clark and Wimsatt who are inspired both by computer modeling and evolutionary theory. The important point in our present context is that kludges typically function only in the context of a whole system, i.e. for the performance of an entire GCM simulation, whereas they have no meaning in relation to the submodels and modules considered in isolation, or, perhaps more importantly, in relation to that module’s potential employment in some other GCM. “What is a kludge considered as an item designed to fulfill a certain role in a large system, may be no kludge at all when viewed as an item designed to fulfill a somewhat different role in a smaller system.”(1987, 279)

Suppose, in other words that I want to improve the predictive accuracy of my GCM by coupling a sub-model of ice cover to my existing model. I may begin with some principled assumption about the physics of ice formation and melting. But what is typical in climate modeling is that by the end of the day, I will incorporate features into the sub-model, or into the interface of the sub-model and the rest of the GCM, that are “complex, unprincipled in [their] design, ill-understood, hard to prove complete or sound and therefore having unknown limitations”. The modules of GCMs, in short, inevitably become “kludged,” and the fact that they increase the accuracy of one GCM is no guarantee whatsoever that would work as well or at all in another.

The notion, therefore, of generative entrenchment is particularly useful way of understanding the epistemological situation in which climate models often find themselves. Wimsatt explains it as follows: “A deeply generatively entrenched feature of a structure is one that has many other things depending on it because it has played a role in generating them.” (2007, 133)

The multitude of possible parameterization schemes and choices of parameters and their balanced interaction in modular models are classic examples of kludged adaptations that are tied, in a fundamental way, to modeling features that have become generatively entrenched. Such features contribute to the difficulties of gaining what we call *analytic understanding* of complex simulation models – an understanding of which sub-components of a simulation are responsible for its various successes and failures – because during the modeling process, the kernel of code, the choice and adjustment of parameterizations, and the peculiarities of controlling the interaction of modules typically get *adapted to* generatively entrenched features of the particular GCM for which they have been crafted.

The point, in sum, is that comprehensive climate models – from the first atmospheric GCMs up to the coupled versions of Earth System Models – have grown organically over several decades of development. And the growth has been a process of give and take between theoretical motivation and practical exigency. Whether a new module adds to or subtracts from the overall reliability of the model may have more to do with some generatively entrenched features of the model than it does with that module's generic "goodness of fit", considered in isolation. When a vegetation module is added to a GCM and adds to the GCM's reliability, how much of this should we attribute to the general features of the module itself, as it might be abstractly characterized, and how much should be attributed to very locally tailored attributes of the module – the kludges – that have been used to fit and adapt the module to the generatively entrenched features of the GCM? Features, which, presumably, will not necessarily play a role in competing climate models.

Back to the validation of GCMs: If our claim about holism and entrenchment is correct they should visibly shape the way GCMs are validated. It is possible, of course, to test the performance of these models under a variety of conditions. And different models perform better under certain conditions than others. But if model A performs better at making predictions on condition A, and model B performs better under condition B, then optimistically, one might hope that a hybrid model – one that contained some features of model A and some features of model – would perform well under both set of conditions. But what would such a hybrid model look like?

Ideally, to answer that question, one would like to attribute the success of each of the models A and B to the success of particular ones of their submodels – or components. One might hope to believe, for example, that a GCM that is particularly good at prediction of precipitation is one that has, in some suitably generalizable sense, a particularly good rain module. We call success in such an endeavor, the process of teasing apart the sources of success and failure of a simulation, "analytical understanding" of a global model. We would say that one has such understanding precisely when one is able to indentify the extent to which each of the submodels of a global model is contributing to its various successes and1 failures.

Unfortunately, analytic understanding is hard or even impossible to achieve. The complexity of interaction between the submodels in GCMs, and the degree to which these submodels are adapted, via kludges, to generatively entrenched features of the GCM, is so severe that it becomes impossible to independently assess the merits or shortcomings of each submodel. One cannot trace back the effects of assumptions because the tracks get covered during the kludgeing together of complex interactions. That complex climate models are sometimes characterized as "balance of approximations" (Lambert and Boer, 2001, cited in Parker, 2006, 359) is in line with our analysis. The ideal of analytic understanding is profoundly impeded by what appears to be a particularly vicious form of confirmational holism. A closer look at model validation as it is actually done in climate science and especially in the so-called model intercomparison projects will support these conclusions.

Validation of Climate Models

With the growing prominence of climate issues in the public, there has been a great deal of pressure coming from the policy arena to make the process of model validation more rational, and to obtain unequivocal diagnoses. In particular, policy makers are keen to get from their climate scientists not only prediction, but predictions that are accompanied by quantitative assessments of margins of error and of uncertainty (QMU). As a result of these pressures, specific model comparison projects have been launched. Because prediction uncertainty has been linked to model plurality (nothing highlights uncertainty more than a plurality of predictions), the community has had to find ways to deal with validation that take into account the existing plurality of models – and the plurality of predictions that emerge from these models.

A key site where these sorts of activities have taken place is the Lawrence Livermore National Laboratory. There, the "Program for Climate Model Diagnosis and Intercomparison" (PCMDI) has been set up in 1989, with the goal of using model intercomparison as a method of supplementing existing modes of validation.

The official PCMDI website states: "The PCMDI mission is to develop improved methods and tools for the diagnosis and intercomparison of general circulation models (GCMs) that simulate the global climate. The need for innovative analysis of GCM climate simulations is apparent, as increasingly more complex models are developed, while the disagreements among these simulations and relative to climate observations remain significant and poorly understood. The *nature and causes of these disagreements must be accounted for in a systematic fashion* in order to confidently use GCMs for simulation of putative global climate change." (PCMDI, 2008, our emphasis)

In other words, the goal of intercomparison is to uncover significant differences between models, and to analyze those difference in such a way as to *understand the sources of those differences*. The hope is that this could lead to model improvement on the basis of such improved understanding. Prima Facie, this expressed hope stands in tension with our claim that entrenchment and holism preclude analytical understanding. So let us view at some examples. Among the intercomparison projects that have been launched at Livermore are the Atmospheric Intercomparison Project (AMIP), its follower, the Coupled Model Intercomparison Project (CMIP) and the Aqua-Planet Experiment Project (APE).

AMIP

The AMIP project was launched in 1989, the same year as PCMDI, as a worldwide undertaking under the auspices of the World Climate Research Programme. It "undertook the systematic validation, diagnosis and intercomparison of the performance of atmospheric general circulation models. For this purpose all models were required to simulate the evolution of the climate during the decade 1979–1988, subject to the observed monthly-average temperature and sea ice and a common prescribed atmospheric CO_2 concentration and solar constant." (Gates et al., 1999)

The simulations were run with certain prescribed boundary conditions – standard scenarios – to make the performances of different simulation models comparable. The simulation output (whose volume can be measured in terabytes), including the calculation of certain diagnostic measures of performance for all contributing models, were then made available in a standard format by the Livermore Lab. AMIP was quickly accepted as a project of the global climate science community and "virtually the entire international atmospheric modeling community (. . .) contributed the required standard output . . ." (Gates et al., 1999) The computational phase ran for several years until the data were completed in 1993. After that, a couple of diagnostic subprojects began use these data for validation purposes. Optimism ran high:

> AMIP offers an unprecedented opportunity for the comprehensive evaluation and validation of current atmospheric models, and is expected to provide valuable information for model improvement (Gates, 1992).

It came out, for instance, that "a large-scale error common to all current atmospheric GCMs is colder than observed air in the lower troposphere in the tropics and in the upper troposphere in higher latitudes." (Gates, 1992) However, results of this kind were thought to be only the first preliminary step. Based on the observed differences in model performance, the important thing was to make inferences about the performances of the various sub-components of the models and to attribute the diagnosed strengths and weaknesses of the different models. This, however, turned out to be much more difficult than initially expected. The process of intercomparison took several years and helped to locate and diagnose differences in performance – that was surely a success (and a huge organisational effort). In discussing the "present status" of AMIP in (1992), Gates noted that "while much important information on the model's individual and collective performance will be provided by these statistics, insight into the models' portrayal of specific physical mechanisms requires a deeper and more revealing diagnosis of the results." The question of *attribution*, however, of which particular mechanisms implemented in the models – for instance particular parameter choices or parameterization schemes – where responsible for performance remained largely unsolved – even in later years.

Nevertheless, attribution remained a core goal of AMIP, and the more optimistic stance remained common that intercomparison was the right way to proceed: "In such endeavors, attempts to attribute differences among the simulations to specific model properties require, as a minimum prerequisite, the accurate and comprehensive documentation of these features." (Phillips, 1996; 1191, see also PCMDI report No. 24)

While documentation proceeded, difficulties with *attribution*, and with what we have called *analytic understanding* of the models persisted. In their voluminous 1998 review of AMIP, Gates et al. conceded that there where still errors revealed by the intercomparison. Some had been reduced during the last years, but many remained nearly the same. The goal of using intercomparison to understand the nature of these errors remained a goal, but it was postponed until the next project. They wrote programmatically:

> In order to understand better the nature of these errors and to accelerate the rate of model improvement, an expanded and continuing project (AMIP II) is being undertaken in which analysis and intercomparison will address a wider range of variables and processes, using an improved diagnostic and experimental infrastructure.

To summarize the AMIP project, it had two goals:

- First, comparison: make available a technical platform at the Livermore Lab, based on standardized data of model performance so that models' performance could be compared.
- Second, attribution: conduct an analysis that could attribute differences in performance to differences in the model components and mechanisms.

While the first goal was a success, the second was a failure. Our thesis is that it was a systematic failure not a contingent one. Our diagnosis of this failure is that it is best understood as a form of confirmational holism arising from the need modelers face to adapt their efforts, often with kludges, to generatively entrenched features of GCMs.

CMIP

The conclusions we draw from our study of AMIP persist as we shift our focus to its more recent sibling: CMIP, the "Coupled Model Intercomparison Project" (CMIP), another one of PCMDI's intercomparison projects. It followed similar lines as the AMIP, but used the up-to-date flagships of simulation modelling, and used coupled atmosphere-ocean models. Phase III of this project provided the data to be shown in the newly released Fourth Assessment Report of the IPCC (AR4, 2007). The project description stressed the organizational and networking aspect for the climate science community. One of the central original goals – deepened understanding of simulation mechanisms via attribution – disappeared nearly entirely from the proposals. What this seems to indicate is that the climate science community has began to tacitly accept a kind of holism about complex simulations that renders analytic understanding of these models out of reach. We admit that there is no complete proof for this claim. It is of course possible that time and effort had not been sufficient yet to reach the kind of understanding that we are suggesting is practically impossible. But we find this unlikely, hence we hold that the conclusions of CMIP 3 reflect a kind of disillusion on the part of climate scientists with regard to attribution, and, in short, believe that acceptance of a very deep kind of confirmation holism is inevitable.

APE

A third intercomparison project reflects the disillusion and tries to maintain the goal of understanding/attribution by reducing complexity. The "Aqua-Planet Experiment

Project” (APE) arose out of the problems the researchers had run into with AMIP (cf. Neale and Hoskins, 2000a). The APE proposal tries to solve that problem by radically simplifying the boundary conditions: the whole simulated planet – “aqua-planet” – now is covered by water. “In this way, the model’s physical interactions are retained whilst the complexity associated with many surface inhomogeneities are discarded.” (Neale and Hoskins, 2000b, 108) It is the basic approach of APE to keep the parameterization schemes and simplify solely the boundary conditions. The updated documentation of APE formulates quite cautious. Again, the authors stress the value of obtaining a benchmark for comparison whereas the more important goal – the understanding of the causes of differences in model performance, in short: attribution – is postponed to a later stage (see APE, 2008).

Conclusions

The original goal of these projects had been to diagnose strengths and weaknesses of different climate simulation models on the market. But it was precisely in this context that the concrete problem of confirmational holism emerged. The overall performance of the various models could be compared, but the model comparison projects had hoped to do more. They had hoped to be able to identify which, among the various modules, submodels and parameterization schemes that were being employed by the various complete models, were responsible for the various aspects of the successes and failures of the complete models. But this proved not to be feasible. It was impossible to re-trace differences and to single out the culprit of a particular property in terms of modeling assumptions, module inclusion or exclusion, or algorithm implementations. The complex interaction of simulation modules, including kludged adaptations, during which the climate dynamics evolves, covered the tracks. This is an important reason, so we argued, why observed differences in model behavior between various models could not be successfully attributed to flaws or successes of the various sub models. It is well-known, for example, which GCMs are good in reproducing wind patterns, but it is not possible to locate the cause for this in code. And hence the researchers were not able to improve part of their models by the knowledge gained through comparison with other models.

We can now bring together two of the central claims of this paper: The first claim is that climate modelers confront a particularly intractable form of confirmational holism – their complex and highly modular models of the earth’s climate are analytically impenetrable. The second claim is about entrenchment as a putative cause for this holism: the various ways in which particular climate models succeed and fail, the ways in which they exceed and lag their peers in performing the predictive tasks to which they are put, is plausibly influenced by their history – of the circumstance under which they were developed.[4]

[4] We are aware of the fact that this claim has some plausibility at the current point but not certainty. The latter calls for a more thorough historical-philosophical study.

Another concept from Clark's work is useful here: what he calls the principle of the "historical snowball", an informal principle formulated by geneticist and physician Francois Jacob: "Simpler objects are more dependent on (physical) constraints than on history. As complexity increases, history plays the greater part."(Clark, 1987, 280)

Think of Dumbo the Elephant, the Disney elephant character whose ears grow so large that he could fly. We know, of course, that in the real world, elephants will never fly. Even though there are various evolutionary adaptations which enable certain creatures to fly, none of these will ever work for an elephant. That is because there are other features of elephants (in particular: their bulk) – features that evolved in particular evolutionary circumstances in response to particular environmental pressures – which make adaptations like wings (or big, floppy ears) useless. A wing is an adaptation for an insect, but not for an elephant.

We propose to see climate models and the efforts of the various model intercomparison projects in a similar fashion. A particular module which is "adaptive" for one GCM (in the sense that, given the present barrage of benchmarking tests available: it improves performance) may not be adaptive for another GCM – indeed it may degrade performance. And it is the particular histories of the GCMs, the "environmental pressures" these models faced as they were developed (read: what the modelers were trying, in particular, to get the models to achieve, and the particular data sets they were using to benchmark their models as the models were being developed) that explain these differences. The features of those models that became generatively entrenched through those histories are the features that make the elephants unable to fly and the insects unable to knock down trees – no matter how many wings we give the elephant, or how many tusks we give the insect.

Put together, these two conclusions become particularly salient when we think about model pluralism and model uncertainty. Think, again, of the procedure of "collecting the opinions of all the valuable witnesses." There are recent trends in climate science which suggest that the range of predictions made by the available arsenal of climate models corresponds, in some way or another, to a probability measure over those various possible outcomes (cf. IPCC, 2007 or, for a more skeptical position regarding the feasibility of this endeavor, Smith, 2002). There is some justification for this: the principal justification is that policy makers desperately need to know these probabilities, and we know of no other way to generate them.

But against a background of these practices, it is very important to remember the history that produced the particular arsenal we happen to have at our disposal, and to reflect on the possible effects this history has on that arsenal, and the epistemic limitations we face in uncovering and understanding those effects.

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Computational Science and Its Effects

Paul Humphreys

Introduction

The rise of computational science, which can be dated, somewhat arbitrarily, as beginning around 1945–1946,[1] has had effects in at least three connected domains – the scientific, the philosophical, and the socio-technological context within which science is conducted.[2] Some of these effects are secondary, in the sense that disciplines such as complexity theory would have remained small theoretical curiosities without access to serious computational resources. Other effects, such as the possibility of completely automated sciences, are longer term and will take decades to alter the intellectual landscape. I shall provide here some examples of fine-grained philosophical effects as well as examples of more sweeping social and intellectual consequences that will suggest both the different ways of thinking that these methods require and a hint at how far-reaching they are.

First, we need a framework. In their paper "Complex Systems, Modelling, and Simulation", Sylvain Schweber and Matthias Wächter (2000) suggested that the introduction and widespread use of computational science constitutes what they call a "Hacking Revolution" in science and that Hacking's use of "styles of reasoning", a concept which originated with the historian of science A.C. Crombie, can give us

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[1] I identify its origins with the use of electronic computers to perform Monte Carlo calculations at Los Alamos and John Mauchley's suggestion that ENIAC could be used for difference equation simulations, rather than for just routine arithmetical calculations. See Metropolis (1993), 127 for the second point. I do not vouch for the accuracy of Metropolis's recollections on this point although the exact historical turning point, if indeed "exact" ever makes sense in historical claims, is unimportant. For those interested in technoscience, I note that the innovation had its origins at Los Alamos and other military research institutions rather than in industrial applications.

[2] There are other domains it has affected, but I shall restrict my discussion to these three.

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useful insights into these methods. Schweber and Wächter have many useful things to say about simulations and related methods, but Hacking's framework does not sit well on computational science. Let me say why.

Hacking revolutions have four principal characteristics: First, "they transform a wide range of scientific practices and they are multi-disciplinary". In this, they are different from the more familiar Kuhnian revolutions or the shifts in theoretical research programs suggested by Imre Lakatos, which tend to be limited to single scientific disciplines. Computational science satisfies this first condition, most notably because its methods are largely trans-disciplinary. Secondly, a Hacking revolution leads to new institutions designed to foster the new practices. The Santa Fe Institute is an example of this second feature.[3] The third characteristic is that "the revolution is linked with substantial social change". Changes in the social structure of science are hard to separate from more general societal changes introduced by computers, but it is true that the social structure of science has been affected by the easy electronic exchange of ideas, the dominant role of programmers in a research group, and remote access to supercomputers. The fourth characteristic is that "there can be no complete, all-encompassing history of such revolutions".

Although there is merit in the concept of a Hacking Revolution, I shall not use it here for two reasons. The first is that Hacking revolutions share their second and third components with Kuhnian revolutions (because of the tight link between the intellectual and sociological aspects of Kuhn's position, these components are satisfied almost by default in a Kuhnian revolution.) And the fourth condition is almost trivially true of any such historical episode. This leaves only the multi-disciplinary aspect, which is important but lacks fine structure. Secondly, let me make a distinction between *replacement revolutions* and *emplacement revolutions*. Replacement revolutions are the familiar kind in which an established way of doing science is overthrown and a different set of methods takes over. Emplacement revolutions occur when a new way of doing science is introduced which largely leaves in place existing methods. The introduction of laboratory experimentation was an emplacement revolution in the sense that it did not lead to the demise of theory or of observation. Similarly, the rise of computational science constitutes an emplacement revolution. This is not to say that theory and experiment are not affected by computational approaches, because certain theoretical methods have now been taken over by computational methods, and many experiments are now computer assisted, but theory and experiment have not been abandoned and considered scientifically unacceptable in the way that the replacement revolutions of Copernicus over Ptolemy, Newton over Descartes, or Darwin over gradualism resulted in the untenability of the previous approaches.

What of styles of reasoning? Here are six cases, originally identified by Crombie, that are cited by Hacking as examples of the genre:

[3] Although the Institute has recently announced that because complexity science is now well established, it must move in new directions.

(a) The simple method of postulation exemplified by the Greek mathematical sciences.
(b) The deployment of experiment both to control postulation and to explore by observation and measurement.
(c) Hypothetical construction of analogical models.
(d) Ordering of variety by comparison and taxonomy.
(e) Statistical analysis of regularities of populations, and the calculus of probabilities.
(f) The historical derivation of genetic development (Hacking, 1992, 4).

Hacking then says: "Every style of reasoning introduces a great many novelties including new types of objects; evidence; sentences, new ways of being a candidate for truth or falsehood; laws, or at any rate modalities; possibilities. One will also notice, on occasion, new types of classification, and new types of explanations.... Hence we are in a position to propose a necessary condition for being a style of reasoning: each style should introduce novelties of most or all of the listed types and should do so in an open-textured, ongoing, and creative way." (ibid., pp. 11–12, slightly reformatted). One could squeeze computational science into this framework because three of the five criteria are satisfied – novelties of evidence, sentences, and possibilities – but, as I shall argue, laws are the wrong vehicle for understanding what is distinctive about computational science, and the novel objects are better understood as novel representations. Moreover, "style of reasoning" has an anthropocentric flavor that is best avoided in this context. Instead, I shall use the term "technique" in what follows.

The Main Issue

Let me put the principal philosophical novelty of these new scientific methods in the starkest possible way: Computational science introduces new issues into the philosophy of science because it uses methods that push humans away from the centre of the epistemological enterprise. In doing this, it is continuing a historical development that began with the use of clocks and compasses, as well as the optical telescope and microscope, but it is distinctively different in that it divorces reasoning, rather than perceptual, tasks from human cognitive capacities. There were historical ancestors of computational science, such as astrolabes and orreries, but their operation was essentially dependent upon human calculations.

Until recently, science has always been an activity that humans carry out and analyze. It is also humans that possess and use the knowledge produced by science. In this, the philosophy of science has followed traditional epistemology which, with a few exceptions such as the investigation of divine omniscience, has been the study of human knowledge. Locke's *Essay Concerning Human Understanding,* Berkeley's *A Treatise Concerning the Principles of Human Knowledge*, Hume's *A Treatise of Human Knowledge*, Reid's *Essays on the Intellectual Powers of Man* are but a few examples; the Cartesian and Kantian traditions in their different ways are also anthropocentric.[4] In the twentieth century, the logical component of logical empiricism broke free from the psychologism of earlier centuries, but the empiricist component prevented a complete separation.[5] Two of the great alternatives to logical empiricism, Quine's and Kuhn's epistemologies, are rooted in communities of human scientists and language users. Even constructive empiricism and its successor, the empirical stance, are firmly anchored in human sensory abilities (van Fraassen, 1980, 2004). There are exceptions to this anthropocentric view, such as Popper (1972) and Ford et al. (2006), but the former's World 3 is too abstract for our concerns and the latter's artificial intelligence orientation does not address the central issues of computational science.[6]

At this point I need to draw a distinction. Call the current situation within which humans deal with science that is carried out at least in part by machines the *hybrid scenario,* and the more extreme situation of a completely automated science replacing the science conducted by humans the *automated scenario*. This distinction is important because in the hybrid scenario, one cannot completely abstract from human cognitive abilities when dealing with representational and computational issues. In the automated scenario one can and it is for me the more interesting philosophical situation, but in the near term we shall be in the hybrid scenario and so I shall restrict myself here to that case. It is because we are in the hybrid scenario that computational science constitutes an emplacement revolution. If the automated scenario comes about, we shall then have a replacement revolution.

For an increasing number of fields in science, an exclusively anthropocentric epistemology is no longer appropriate because there now exist superior, non-human, epistemic authorities. So we are now faced with a problem, which we can call the *anthropocentric predicament*, of how we, as humans, can understand and evaluate computationally based scientific methods that transcend our own abilities and operate in ways that we cannot fully understand. Once again, this predicament is not entirely new because many scientific instruments use representational intermediaries that must be tailored to human cognitive capacities. With the hybrid situation, the representational devices, which include simulations and computationally assisted instruments such as automated genome sequencing, are constructed to balance the needs of the computational tools and the human consumers. We can call the general problem of inventing effective intermediaries the *interface problem* and

4 A Kantian approach can be generalized to non-human conceptual categories, although the extent to which humans could understand those alien categories is then a version of one philosophical challenge faced by computational science.

5 Carnap's *Aufbau* (Carnap, 1928) allows that a physical basis could be used as the starting point of the reconstruction procedure, but adopts personal experiences as the autopsychological basis. The overwhelming majority of the literature in the logical empiricist tradition took the human senses as the ultimate authority.

6 One can usefully borrow Popper's thought experiment in which all of the world's libraries are destroyed and ask how much of contemporary science would be affected if neutron bombs shut down all of the world's computers. Much of "big science", especially in physics and astrophysics, would be impossible to carry out.

it is a little remarked upon aspect of scientific realism when we access the humanly unobservable realm using instruments. Just as scientific instruments present philosophy with one form of the metaphysical problem of scientific realism and its accompanying epistemological problems, so computational science leads to philosophical problems that are both epistemological, a feature that has been emphasized by Eric Winsberg and Johannes Lenhard,[7] and metaphysical.

What Is Metaphysically Different About Computational Science

The essence of computational science is providing computationally tractable representations; objects that I have elsewhere called computational templates.[8] It is an important feature of templates that they are trans-disciplinary. The philosophical literature on scientific laws, with its emphasis on counterfactuals, nomological necessity, logical form, and so on, often does not stress the fact that the fundamental laws of a science are uniquely characteristic of that science. Although Newton's laws applied to any material object in the eighteenth century, they did not characterize biological objects qua biological objects in the way that they did characterize what it was to be a physical object. Nowadays, the Hardy-Weinberg law is a characteristic feature of population biology, and it makes no sense in chemistry or physics.[9]

I mentioned above that laws are the wrong vehicle for understanding computational science. The reason for this is connected with the fact that scientific laws are intimately tied to a particular science and its subject matter, whereas the emphasis of computational science is on trans-disciplinary representations. (There are some candidates for laws of this trans-disciplinary type in complexity theory, such as Zipf's Law, a power law that reasonably accurately describes the distribution of city sizes, network connection densities, the size of forest fires, and a number of other phenomena that are the result of scale-invariant features.) Just as theory and experiment involve techniques that are to a greater or lesser extent subject matter independent, so too does computational science. This cross-disciplinary orientation has at least two consequences that are worth mentioning. First, it runs counter to the widely held view that models are local representations. It is, of course, true that many models are far less general than theories, but the existence of widely used computational templates suggests that the disunity of science thesis that often accompanies the "models are local" thesis is simply wrong about the areas of contemporary science that lend themselves to the successful use of such templates. Secondly, it runs

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[7] See e.g. Winsberg (2001, 2003) and Lenhard (2007).

[8] See Humphreys (2002, 2004 Chapter 3, 2009).

[9] To prevent misunderstanding, I note that although the term "law" is used for such things as the weak and strong laws of large numbers in probability theory, this is a courtesy use of the term "law" because these are purely mathematical results. They lack at least the nomological necessity possessed by scientific laws.

orthogonally to the traditional reductionist approach to understanding. Reduction suggests to us that we can better understand higher level systems by showing how they can be reduced to, how they can be explained in terms of, lower level systems. Computational templates suggest that we can gain understanding of systems without pursuing reduction by displaying the common structural features possessed by systems across different subject domains. In saying this, I am not claiming that these trans-disciplinary representations did not exist prior to the introduction of computational science. What the latter development did was to allow the vastly increased use of these techniques in ways that made their application feasible.

I can illustrate the issue involved using as an example agent based simulations. Agent based simulations are in certain ways very different from what one might call equation-based simulations. It is a common, although not universal, feature of agent based models that emergent macro-level features appear as a result of running the simulation, that these features would not appear without running the simulation, that new macro-level descriptions must be introduced to capture these features, and that the details of the process between the model and its output are inaccessible to human scientists. No traditional modeling methods address the first, second, and fourth features of these simulations . Let me elaborate a little on how the third point plays out in this context. The situation has been nicely captured by Stephen Weinberg: “After all, even if you knew everything about water molecules and you had a computer good enough to follow how every molecule in a glass of water moved in space, all you would have would be a mountain of computer tape. How in that mountain of computer tape would you ever recognize the properties that interest you about the water, properties like vorticity, turbulence, entropy, and temperature?” (Weinberg, 1987, 434). Many of the “higher level” conceptual representations needed to capture the emergence of higher level patterns do already exist in other theoretical representations; they are the starting point for what Ernest Nagel called inhomogeneous reductions (Nagel, 1974). With other agent based models the situation is different because the simulation itself will, in some cases, construct a novel macro-level feature. It is this constructivist aspect of simulations, one that runs in the opposite direction to the traditional reductionist tendency of theories, that is a characteristic feature of agent based models in particular, although it also can be a focus of equation based models. Constructivism was memorably described in Anderson (1972) and is a key element of the arguments presented in Laughlin and Pines (2000).[10] These emergent patterns in computer simulations form the basis for what Mark Bedau has characterized as “weak emergence” (Bedau, 1997) and traditional human modeling techniques will not generate them from the agent base. They can only be arrived at by simulation.

This emphasis on higher level patterns is not restricted to computational science or to emergence. It is a feature of multiply realizable systems and of physical systems in which universality is exhibited. (For a discussion of the relations between

[10] The use of generative mechanisms as an element of constructivism is noted in Küppers and Lenhard (2006).

multiple realizability and universality, see Batterman, 2002.) As another example, Niklas Luhmann, the German sociologist, has persuasively argued for the irrelevance of individual humans in various functional systems.[11] For example, within consumer economies, it is irrelevant who purchases the pack of cigarettes – they can be male, female, Chilean or Chinese, middle-aged or old, white collar or blue collar – all that matters is that the relevant economic communications take place. Indeed, Luhmann's work is a striking example of a research program within which the importance of humans as individuals is severely diminished and the emphasis placed on the autonomy of higher level features. Luhmann was an early advocate of autopoiesis, a process that leads to self-organizing systems. One of the core features of self-organizing systems is that there is no central organizing force controlling the system. The American Stryker forces that are currently operating in Iraq and Afghanistan are a contemporary example of the movement towards engineering systems of this kind. Every member of a squad is issued a radio or other communications device, with the result that information is no longer concentrated in and processed through a central command system, and the lowest ranking infantryman will often be better informed of the dynamically evolving state than will the commanding officer. Because the traditional command hierarchy is still in place, the tension between the two is understandably the subject of much debate.

Computational science can also produce significant shifts in specific sciences. For example, general equilibrium theory, which dominated neo-classical economics for decades, is now being challenged by rival approaches such as agent based microeconomics and evolutionary game theory. These developments are sensible because humans tend to have a good insight into the nature of social and economic relations between individuals and much less of a firm grip on the kind of hyper-idealized grand theory that was once dominant.

What Is Epistemically New About Computational Science

The rise of computational science has allowed an enormous increase in scientific applications. But this expansion has also been accompanied by a shift in emphasis from what is possible in principle to what is possible in practice, with the countervailing result that the domain of science has also shrunk. Let me explain.

In Practice, Not in Principle

One feature of computational science is that it forces us to make a distinction between what is applicable in practice and what is applicable only in principle.

[11] Luhmann's culminating work is Luhmann (1997), which is not yet available in an English translation. I am grateful to Tiha von Ghyczy for conversations about various aspects of Luhmann's thought.

Here the shift is first, from the complete abstraction from practical constraints that is characteristic of much of traditional philosophy of science, and second from the kind of bounded scientific rationality that is characteristic of the work of Simon and Wimsatt (Wimsatt, 2007), within which the emphasis tends to be on accommodating the limitations of human agents. Ignoring implementation constraints can lead to inadvisable remarks. It is a philosophical fantasy to suggest, as Manfred Stöckler does that "In principle, there is nothing in a simulation that could not be worked out without computers" (2000, 368).[12]

In saying this I am not in any way suggesting that in principle results are not relevant in some areas. They clearly are; there are also other issues to which the philosophy of science needs to devote attention. One of the primary reasons for the rapid spread of simulations through the theoretically oriented sciences is that simulations allow theories and models to be applied in practice to a far greater variety of situations. Without access to simulation, applications are sometimes not possible; in other cases the theory can be applied only to a few stylized cases.

Within philosophy, there is a certain amount of resistance to including practical considerations, a resistance with which I can sympathize and I am by no means suggesting that the investigation of what can (or cannot) be done in principle is always inappropriate for the philosophy of science. One source of resistance to using in practice constraints is already present in the tension between descriptive history of science and normative philosophy of science, and in the tension between naturalistic approaches (which tend to mean different things to different people) and more traditional philosophy of science. But the appeal to in principle arguments involves a certain kind of idealization, and some idealizations are appropriate whereas others are not. A long-standing epistemological issue involves the limits of knowledge. Are there things that we cannot know, and if so, can we identify them? There surely cannot be any question that this is a genuine philosophical problem. Of course, it is not new – Kant famously gave us answers to the question. The question of what we can know, or more accurately, what we can understand, has been transformed by the rise of computational science and it is partly a question of what idealizations can legitimately be used for epistemic agents. We already have experience in what idealizations are appropriate and inappropriate for various research programmes. The move away from hyper-rational economic agents in micro-economics to less idealized agents mentioned earlier is one well-known example. For certain philosophical purposes, such as demonstrating that some kinds of knowledge are impossible even in principle, in principle arguments are fine. But just as humans cannot in principle see atoms, neither can humans in principle be given the attributes of unbounded memory and arbitrarily fast computational speed. This is the reason underlying epistemic opacity, one of the key epistemological features of the new methods.

[12] The first versions of Thomas Schelling's agent based models of segregation, and the first versions of Conway's Game of Life were done "by hand", but almost all contemporary simulations require abilities that go far beyond what is possible by the unaided human intellect.

Epistemic Opacity

One of the key features of computational science is the essential epistemic opacity of the computational process that leads from the abstract model underlying the simulation to its output. Here a process is epistemically opaque relative to a cognitive agent X at time t just in case X does not know at t all of the epistemically relevant elements of the process. A process is essentially epistemically opaque to X if and only if it is impossible, given the nature of X, for X to know all of the epistemically relevant elements of the process.[13] The relativization to a cognitive agent is required because in the case of a mathematical proof, for example, one agent may consider a particular step in the proof to be an epistemically relevant part of the justification of the theorem, whereas to another, the step is sufficiently trivial to be eliminable. In the case of scientific instruments, it is a long-standing issue in the philosophy of science whether the user needs to know details of the processes between input and output in order to know that the instrument's outout accurately represents a real entity.

Within the hybrid scenario, no human can examine and justify every element of the computational processes that produce the output of a computer simulation or other artifacts of computational science. This feature is novel because, prior to the 1940s, theoretical science had not been able to automate the process from theory to applications in a way that made the details of parts of that process completely inaccessible to humans. Many, perhaps all, of the features that are special to simulations are a result of this inability of human cognitive abilities to know in detail what the computational process consists in. The computations involved in most simulations are so fast and so complex that no human or group of humans can in practice reproduce or understand the processes. Although there are parallels with the switch from an individualist epistemology, within which a single scientist or mathematician can verify a procedure or a proof, to social epistemology, within which the work has to be divided between groups of scientists or mathematicians, so that no one person understands all of the process, the sources of epistemic opacity in computational science are very different.

One of the major unresolved issues in many areas of computational science is whether the invention of new mathematical techniques might eventually replace some of these computational methods. I have frequently heard the suggestion that if we introduced a new class of functions that were solutions to the existing, currently intractable model, this would not change the way the model relates to the world. In fact it would, because with the availability of analytic solutions, the epistemic opacity of the relation between the model and the application would disappear. Moreover, even if this were to happen, the fact that the computational methods are, during our

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[13] In my 2004, I used only the straightforward "epistemically opaque" terminology. I now think that distinguishing between the weaker and stronger senses is useful. It is obviously possible to construct definitions of "partially epistemically opaque" and "fully epistemically opaque" which the reader can do himself or herself if so inclined. What constitutes an epistemically relevant element will depend upon the kind of process involved.

era, an unavoidable part of scientific method makes them of philosophical interest, just as the use of the Ptolemaic apparatus for computing planetary orbits is still of philosophical interest.

There are aspects of computational science that are simply not addressed by either of the two traditional philosophical accounts of theories. The semantic account of theories operates at too high a level of abstraction to capture important differences in tractability of different syntactic representations of what the semantic account considers the "same" abstract theory. The traditional syntactic account of theories distinguished between some types of theories; those that were recursively axiomatizable, those whose axioms sets are only recursively enumerable, and a few other types. Computer scientists have since added to this classification, in moving from the simple issue of (Turing) computability to measures of theoretical computational complexity, such as P, NP, P-SPACE, and many others. This refinement can be incorporated within the syntactic account of theories. Other issues about the power of different computational architectures that are also relevant to computational science cannot be incorporated into the syntactic approach. It is possible that if operational quantum or biological computers are built, a number of scientifically intractable problems will become tractable, opening up new areas of research. This is not an issue that is in any way addressed by traditional modeling techniques and although philosophical discussions of quantum computing have not been motivated much by issues in the area of simulations, the area is novel and is relevant to computational science (See e.g. Mermin, 2007).

The Link Between Science and Technology

The final issue to be addressed is the way in which progress in various sciences is now tied to technological advances in ways that go beyond the dependencies produced by a reliance on instrumentation. Computer simulations are crucially dependent upon computational load issues, and science must often wait until the next generation of machines is developed for these load demands to be accommodated. Technological issues arise in other ways as well: there are problems of extending models when substantial chunks of code are written in languages that are not compatible with other modules in the software and are thus hard to integrate into later research; the former may require obsolete hardware to run. An overemphasis on more abstract languages such as second-order logic or category theory obscures these features, which are important in the application of massive simulation projects. Philosophers of science are free to abstract from these issues, but then in some areas of science their accounts will simply misrepresent how progress is made.

Even with idealizations, these computational features are relevant. Here is one particular example: Determining energy levels is a core interest for molecular chemists. Physical chemistry employs quantum mechanics as its basic theoretical apparatus, but ab initio calculations of the energy levels are impossible to carry out for any but the smallest molecules. The simple valence bond and molecular orbital models do not provide accurate predictions even for hydrogen molecules, so they have to be supplemented with dozens of extra terms to account for various features. They therefore employ multiple approximations and are heavily computational. So the approximations chosen in the Hartree-Fock self-consistent field approach, a standard method of calculating ground state energies in ab initio quantum chemistry, are inextricably linked with the degree to which those calculations can actually be carried out in practice. On the other side there is now a growing sense that a different problem has arisen; that new techniques need to be developed to effectively exploit the massive computational power that is now available in many areas.[14]

Conclusion

Although some scepticism has been expressed about the novelty of computer simulations and related techniques (e.g. Stöckler, 2000; Frigg and Reiss, 2009; for a response see Humphreys, 2009), there is more than enough evidence to support claims that they constitute an important addition to the techniques of science, on a par with theoretical representations and experiment. The effect of this emplacement revolution in computational methods is a rich source of philosophical problems, metaphysical, epistemological, and representational.

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Expertise in Methods, Methods of Expertise

Carsten Reinhardt

Motivations

In the past few years, the topic of scientific expertise has been taken up again in science studies. Following the political and ideological debates of the 1960s and 1970s about democracy and the legitimating of experts and technocrats, the research tackles familiar themes such as the mirror-image twins of "science for policy" and "policy for science", but also centers on novel issues such as science and the media, and the role of the public expert. Thus, scientific expertise (including the social and political sciences) is an important theme again for sociologists, political scientists, historians, and of course for historians of science and technology. Many of the recent publications concern the role of experts as political advisors, and the interaction of experts and the public sphere. Scientific expertise is a key issue in the concept of the knowledge society (Jasanoff, 1990, 1995; Bach, 1999; Collin and Horstmann, 2004; Fisch and Rudloff, 2004; Szöllösi-Janze, 2004; Engstrom et al., 2005; Maasen and Weingart, 2005).

While the focus of almost all of the current research is on the user- or demand-side of scientific expertise, I wish to shed light on its formation. In an additional perspective, I want to historicize a debate that has so far only concentrated on "very-recent" science. To achieve this, my paper aims at establishing a connection of the history of scientific methods with the history of scientific expertise. For generating expert knowledge, methods are the *sine qua non*. The discourse on the legitimacy of experts and the reliability of their expertise almost invariably centers on the validity and the efficiency of their methods. Furthermore, I contend that scientists specializing in the development of methods take center stage in the history of scientific expertise. Competence in methods is needed to assist in the solution of others' problems. Being not easily accessible, expertise of methods contributes to the demarcation of expert and layperson, and of scientist and non-scientist. Moreover, laypersons most often do not just desire simple problem solving. In addition, and

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here the functions of a scientific expert align with those of members of modern professions, experts take care of representation and the provision of a service. Methods are also crucial here, if they bestow prestige and if they are officially acknowledged as state of the art.

Though historical in perspective and methodology, the paper touches upon epistemological and sociological issues as well. What is a scientific method? How is the innovation process of methods organized? What social domains are influenced by methods, and what social policies are governed by them, and how? Are there differences between "inner-scientific expertise" and expertise in the social sphere at large? Are there interrelations? The first part (sections "Scientific Methods" and "Expertise in a Scientific Context") of this paper concentrates on the topic of methods in the history of science, illustrating it with the example of the impact of physical methods on chemistry in the second half of the twentieth century. The second part (sections "Expertise in a Non-scientific Context" and "Distrust in Science: The Plea for *Normalmethoden*") tackles the notion of scientific expertise, exemplified with a case study on analytical chemistry in mid-nineteenth century.

Scientific Methods

According to my definition, scientific methods are modes of investigation, and cover single procedures of laboratory practice as well as whole knowledge domains.[1] In a more narrow definition, a method is a standardized experiment, a fixed, reliable, and transferable pathway of research encompassing background theory, instrumentation, and experimental knowledge. From the early 1980s, the experimental character of scientific work has received increasing attention by historians, sociologists, and philosophers of science (Holmes, 1992; Golinski, 1998, 133–161; Hentschel, 2000). Of course, methods play a central role in these endeavors, but rarely are they named explicitly as such, and seldom is their formation and diffusion studied as a *scientific activity in its own right*, although there are exceptions to this (Jordan and Lynch, 1998; Suárez, 2001). Many historians see research on methods as the static part in scientific investigations, thus as a necessary, but intermediate stage of research. For them, methods are just means for the solution of scientific problems. Hans-Jörg Rheinberger's scheme of the experimental system, for example, distinguishes "technical objects" and "epistemic things"; methods and instruments being subsumed in the former, while the latter represent the dynamic part of the investigative enterprise (Rheinberger, 1997, 28–29). New knowledge is produced in a dialectic process, taking place between technical objects and epistemic things. I argue that this process

[1] Synonymously with methods, I use the term techniques. Examples for laboratory procedures are numerous. An example for a method as a knowledge domain is nuclear magnetic resonance spectroscopy, a technique having gained such importance and momentum that it has reached almost the status of a scientific discipline. This paragraph relies on my previous work published in much more detail in Reinhardt (2006a).

is also crucial for the development of methods, which are seen here as the final outcome of research, or the end of experiment. Methods may also constitute the result of a scientific research project as they shape its beginnings, and they do not represent just technological routine. It was Gaston Bachelard, who already in the 1950s envisaged the strategy of a community of scientists centering on methods development (Bachelard, 1972, 39). Bachelard saw twentieth-century science as being utterly different from common-sense knowledge. For him, science was based on the dialectics of mathematical theorems and instrumental methods, and this view led to his vision of instruments as *théorèmes réifiés* and his notion of *phénoménotechnique*: instrument-based methods construct the reality they are analyzing (Rheinberger, 2005).

For our purpose, it is useful to distinguish method-oriented and problem-oriented scientists. Method-oriented scientists see the aim of their investigations in establishing methods for use by other scientists. Consequently, they search for problems that can be solved by the method they are focusing on, enhancing its application and diffusion through paradigmatic case studies that emphasize the method's assets. On the other side, problem-oriented scholars look for methods suited for solving their problem at hand. Of course, there exist intermediate stages in this rough categorization, and one has to mention other categories, for example instrument-makers, and research-technologists in the terms of Terry Shinn (Joerges and Shinn, 2001). Now, a historiographical question comes to mind: How is it possible to differentiate between method making and problem solving, if the making of methods entails the solution to paradigmatic research problems as well? It is easier to recognize the difference if both scientific activities are rooted in distinct research fields, or even different scientific disciplines. Such a historiographical opportunity is the introduction of physical methods in chemistry.

Sometimes called the second chemical revolution, the integration of physical instrumentation into chemical research programs had deep and far-reaching consequences for the cognitive outlook and the organizational outreach of chemistry since the mid-twentieth century (Morris, 2002). Nuclear magnetic resonance spectroscopy (NMR), mass spectrometry, infrared and ultraviolet spectroscopy – to name just some of the important techniques – transformed chemistry at three levels: First, they displaced the ubiquitous chemical method – the chemical reaction – in its analytical applications. Second, they crucially strengthened the capabilities of chemists to investigate abstract molecular structures, and their dynamics. Third, they deeply altered the practices at the laboratory bench, rationalizing chemical labor to an extent unimaginable before. Beginning in the 1920s, chemical handbooks and introductory volumes included the term "physical methods" in their titles. They supplemented books of an earlier generation, describing the working modes and research uses of experimental methods largely along chemical means. Thus, physical methods became identifiable techniques for practitioners, connecting theory and instrumentation on the experimental level. Development of and investigations in physical methods of chemistry evolved into research specialties in their own right.

Expertise in a Scientific Context

For success, method-oriented scientists depend on a community of researchers using their techniques. I call this community the clientele. A decisive issue for the use of methods by clients is their standardization. In the case of physical methods in chemistry, four players tried to gain influence: the chemical industry, governmental agencies, instrument manufacturers, and the method-oriented scientists themselves. In most cases, a balance was reached between the users on the one side, and the suppliers of instruments on the other. Neither group could reach dominance, because their respective contributions to the research system were equally decisive (Reinhardt and Steinhauser, 2008). Disputed issues of standardization included the modes of representation; the choices of parameters, data files, and reference substances; and of course the design of instrumental hardware. Standards came in many guises: sometimes standards of achievement were mentioned side by side with standards of representation and standards of procedure. Important means of standard setting were articles, textbooks, and technical manuals. Especially the writing of textbooks was a crucial strategy for spreading novel methods. Thus, method-oriented scientists heavily engaged in teaching, also because this would contribute to their scientific reputation in the long run (Reinhardt, 2006a, 209–224, 375–377).

In the following, I analyze some of the actions and functions of method-oriented scientists in terms of scientific expertise. In order to be acknowledged as experts, scientists have to share their knowledge. How is expertise shared in scientific and technological communities? For our purposes, we can approach scientific communities under two different aspects. First, science regarded as public sphere. In his book *Strukturwandel der Öffentlichkeit*, Jürgen Habermas in 1962 proposed a change in character of the public sphere that took place around 1800: the representational public sphere of the eighteenth century was replaced by the civic public sphere (Habermas, 1990). In his idealized picture, Habermas announced the following key issues: Only rational argument contributed to the authority of opinion; the choices of problems were open; and, in principle, the range of participants was unrestricted: everyone who could follow the rational discourse was admitted. Social historians have questioned Habermas's vision, and especially criticized the focus on the *civic* public sphere only. But for science, we recognize a striking analogy, at least in rhetoric, to the Mertonian norm of organized skepticism and the predominance of rational argument. Second, and somehow in contrast to Habermas's position, we can approach science as an exchange-driven marketplace activity (Kohler, 1991, 88, 127–130; Mirowski and Sent, 2002). As in the trade market, where goods are exchanged against other goods, or money, scientific information and knowledge is traded against information and knowledge, or other resources. Of course, the economy of science differs from the economy of the market, but it is important to be aware that the exchange of scientific knowledge is not as open as Habermas's view would lead us to assume. As a consequence, I see scientific activity in a tension between the two extremes of an open dialogue of equals (the "forum") and the channeled exchange of information and knowledge (the "market-place").

Expertise in methods involves transfer either of information (data) or knowledge (competence), or of both. The transfer takes places between experts and their clients in science and technology. Information transfer means the handing over of data and their interpretation, but the client is not enabled to use the method by him- or herself. Knowledge transfer stands for the training of the client by the expert, in the long run leading to the client's independent mastering of the technique. I analyze the transfer of expert knowledge and information with the help of three categories: service, training, and collaboration (Reinhardt, 2006b). The service model describes relationships where information is exchanged against material resources, or where information is provided to a community of scientists at the request of a funding institution. The training model does not concern education of students, but covers the training of colleagues in science. Collaboration means transfer of knowledge – thus, the ability to master the method – against intellectual resources, mostly knowledge or research means, such as chemical substances. These categories can all be active in the case of one expert at the same time; thus, he or she may have all of these kinds of relationships with different clients. For illustration, I present an example from mass spectrometry in the United States of the 1960s and 1970s (Reinhardt, 2006a, 138–149, 258–265).

Service

Next to its own, "intramural" research laboratories, the leading funding agency in the biomedical sciences, the National Institutes of Health (NIH) financed a large number of health-related research projects. As many questions in chemistry and physics were closely related to biomedicine, NIH has also supported projects in the physical sciences. In the beginning, this "extramural" program of NIH funded research projects only. In the early 1960s, however, NIH officials decided to make available funds just for the set-up of novel kinds of instruments. Thus, NIH began to finance the development of research instruments in centers, which were named facilities, or Special Research Resources. Special Research Resources provided service to academic communities, and in most cases were restricted to a specific geographical area, mainly in universities. In order to be able to continuously update the instrumentation, the scientific employees of the facilities were also expected to undertake "core research". Their third task included the training of other investigators. Most Special Research Resources were equipped to such an extent that their capacity was large enough to cater for a substantial number of users. The reasoning behind this decision of NIH was the fact that instruments increasingly became too expensive to be financed by single research units. As a consequence, the sharing of instruments became mandatory. A side effect of this was that facilities became centers for innovation of scientific methods, and places where scientists could concentrate on advanced research and further improvements of their techniques. By 1977, 52 Special Research Resources had been set up in the United States (Reinhardt, 2006b).

In 1965, the chemist and expert in mass spectrometry Klaus Biemann of the Massachusetts Institute of Technology (MIT) in Cambridge applied for a research resource grant (Reinhardt, 2006a, 112–144). Partially related to his earlier application for an NIH training grant, which was meant to support the education of graduate students, Biemann's grant application was mainly due to the development of a new and sophisticated method called high resolution mass spectrometry (Reinhardt, 2006a, 245–265). Compared with the older methods of doing mass spectrometry, high resolution mass spectrometry required substantial and continuous funding. Biemann was convinced that the new method was an ideal subject for an NIH facility:

> The simplicity of the basic principle ... is so convincing that it has obtained the kind of publicity which stimulates the desire of having such an instrument in each university or laboratory.

But the continuous operation of such an instrument made necessary a huge investment:

> Thus, to make use of such an instrument and to economically digest all the data which it can produce, a relatively large, experienced and efficient group of people is required as well as a quite complex data acquisition system and a considerable amount of time on a large and fast computer. Once this is accomplished a large body of data can be obtained routinely, much more than any given laboratory can require or use (Biemann, 1965; cited in Reinhardt, 2006a, 260–261).

Thus, the automation of the method induced the transformation of its institutional organization: The method became centralized, while information and knowledge were transferred in a service-like function to outside clients.

Training

Here, I wish to understand under training the knowledge transfer between scientific colleagues who thereby become able to use specific methods themselves, thus becoming experts on their own. In 1961, when still in his own beginnings in mass spectrometry, Biemann trained a colleague, the Stanford chemist Carl Djerassi and his group, in mass spectrometry (Reinhardt, 2006a, 144–149). This transfer of knowledge was decisive for the entry phase of Djerassi into mass spectrometry in the early 1960s. Though training on the spot was a customary feature of academic teaching, it most often just involved exchange of postdocs and graduate students. But in the mass spectrometry of that time, a direct interaction between advanced researchers was deemed to be necessary, because there was almost nothing published on the method. Thus, in spring 1961, Biemann taught Djerassi's group the theoretical and practical dimensions of the uses of mass spectrometry in organic chemistry. In addition, Herbert Budzikiewicz, Djerassi's postdoc, stayed for a short while in Biemann's laboratory at Cambridge, Mass., to become acquainted with mass spectrometry. Djerassi even had the idea to have Biemann's MIT-based research group stay at Stanford for a few months. Soon, Biemann and Djerassi became competitors: “Many people told me: ‘How could you have been so stupid to teach Carl Djerassi . . . the technique which you developed, because he will beat you over the head.’ I wasn’t used to think in that way” (Biemann, 1998; cited in Reinhardt, 2006a, 147). Biemann, however, was convinced that he should enable his colleagues to work with his methods. In the long run, Djerassi indeed published several hundred papers in the field and co-authored influential textbooks. Why, then, do scientists engage in the training of their future competitors, as Biemann did? Several reasons suggest themselves. One could point to their feeling of being obliged to obey scientific norms. Also, the need of community-building may have played a role, as in the early 1960s only three laboratories in US universities were adequately equipped to tackle advanced investigations in organic mass spectrometry.

Collaboration

Collaboration is an essential part of science. Here, I focus on a special type of collaboration involving partners in both academia and industry. However, in this case the industrial side is not so much applying academic knowledge but rather is itself the source of research means and scientific problems. Biemann strongly felt the need to obtain interesting samples for establishing mass spectrometry as a novel method, and the pharmaceutical industry was one of the most important donors. One example of this is his contact with the Lilly Research Laboratories of Eli Lilly & Company at Indianapolis, Indiana (Reinhardt, 2006a, 138–139). Eli Lilly was one of the biggest American pharmaceutical companies, and their products included many compounds of interest for the aspiring mass spectroscopist of the 1960s, most notably peptides and alkaloids. Consequently, Biemann tried to establish contact with the Lilly Research Laboratories, though he was initially unsuccessful because of the lack of suitable compounds at hand. Moreover, the scientists of the company did not understand the potential benefits that mass spectrometry could have for their own goals. For Biemann, this was a real dilemma. To obtain suitable test compounds, he had to prove that his methods were useful. But to be able to demonstrate this, he urgently needed samples. As his contact person at Eli Lilly mentioned in a letter: “If you have any published data I would be glad to circulate a copy or reprint amongst my colleagues so that we would know when and where your technique could be called upon” (Reinhardt, 2006a, 139).

Biemann’s second attempt at obtaining samples from a pharmaceutical company was more successful. CIBA Pharmaceutical Products Inc. of Summit, New Jersey did for Biemann something that Eli Lilly would not do: the company’s chemists isolated and purified a compound suitable for mass spectrometry investigations (Reinhardt, 2006a, 139–141). CIBA supported Biemann in a wide array of fields, and after having seen the potential uses of mass spectrometry, Eli Lilly also finally agreed to collaborate. In the long run, Biemann trained researchers from both CIBA and Eli Lilly, and from other companies, in organic mass spectrometry. In exchange for the knowledge that he transferred to the companies he received grants, substances, and important chemical know-how.

The three categories of service, training and collaboration have shed some light on how transfer of knowledge and information was organized in the case of method-oriented scientists. Transfers of these types depended on careers, the organization of methods in centers, the establishment of communities of researchers, and focusing on development of methods as a fulltime scientific activity. We will now ask how expertise of this kind was organized when crossing the boundaries of the techno-scientific realm.

Expertise in a Non-scientific Context

What is (scientific) expertise? Often, historians, sociologists, and scientists alike opine that it does not belong to the scientific enterprise in any narrowly defined sense. According to Martin Lengwiler, expertise is the application of academic knowledge in a non-academic context. Helga Nowotny talks about a "new branch of science", and Alvin Weinberg describes "trans-scientific" problems as problems that can be defined and described in scientific terms, but cannot be solved in science itself (Hartmann, 2006; Nowotny, 1987; Weinberg, 1972). Behind this opinion is the perception that much of expertise concerns the assessment of risk, involves decisions under uncertainty, and is often not politically neutral. But as we have seen in the foregoing paragraph, many inner-scientific processes of knowledge transfer can be understood as being related to an expert-client relationship. Moreover, *defining* a problem often entails the means to its description, and the opportunity to influence, or even decide on, possible solutions. On the other hand, clients shape expert knowledge in decisive ways, as they have to provide the resources and knowledge for the making of expertise.

To a large extent, the social legitimation and authority of science rests on boundary work (Gieryn, 1983). The scientific/non-scientific demarcation is based on the claims of science being able to speak "for nature" in objective, universal, disinterested, and neutral ways. Scientists present themselves as mediators between nature and the social, and they demand to have exclusive, or at least privileged, access to knowledge about nature. In the ideal case, scientists are seen by laypersons as legitimized representatives of the public good, exactly because of their assumed objectivity. If they have been granted competence and legitimacy, they can fulfill this role in other fields than science, e.g., in legislation and the juridical system, administrative tasks, and the amendment of policy. Scientific experts do not only dispose of special knowledge, and the authority to form and to impart it (a definition of scientific discipline), but they also stand between the public sphere and decision makers: they are to be understood as relational phenomenon (Hitzler, 1994, 17–19). Thus, expertise is a form of social interaction that draws on the supply and demand of knowledge. In the Habermasian vision of an open dialogue of equals, social differences should not play a role in this interaction. Public opinion can never be determined by one social group alone, unless a social group would be able to hide its special interests behind universal and objective facts. Exactly at this point scientific experts can have a major impact.

Since the early nineteenth century, the rational scientific discourse was ideally suited to integrate differing standpoints and merge them into a coherent view: the public opinion. In the belief of large parts of society, the sciences constituted the perfect role model for a rational and open society. Somewhat in contrast to this openness was the fact that the admission to scientific ranks was bound by professional restrictions. This situation paved the way for scientists, the majority of which consisted of members of the upper and middle classes, to shape and to represent public opinion in important social problems, while at the same time seeing themselves (and being seen) as independent critics. Natural knowledge and public opinion shared important parameters: Both were regarded as neutral, and both were thought to determine and to serve the public good. Once such a tie had been established it was difficult to break: Critics had to join a scientific discussion about facts and methods; and they had to be legitimized, i.e., qualified according to professional standards. Experts and counter-experts exchanged their opinions, and questions that originally had been political issues were discussed in scientific terms. The image of the scientific expert as the personification of bourgeois values such as progress, individuality and the common good, supported their privileged status. At the same time, state governments took over new and challenging tasks in the regulation of industry and commerce, and were in demand of informed and qualified expertise. The emerging scientific disciplines with their ordered discourse and their claims for prevalence were ideal institutions for satisfying the increasing administrative demand for certified knowledge. Despite state control, scientific experts in the resulting interactions found enough space to create quasi-autonomous fields in- and outside the universities.

The intercalation of the state, the public, and the sciences certainly changed the moral economy of science, understood here as a system of norms and values that governed the discourse of knowledge. What impact did the "demand-side" have on the generation of scientific facts? What congruencies existed between the law, commerce, industry, and administration on the one side and the sciences on the other? In the context of the case study presented below: Have there been special expectations of the courts of law with regard to the accuracy, precision, and kinds of evidence of analytical chemistry? On the other hand, did the values of analytical chemistry, such as empiricism, quantification, and objectivity, shape the ways that juries arrived at their judgments? More generally, how did scientific experts consolidate their status in the triad of state administration, commerce and industry, and the sciences? We can safely assume that experts had their own agenda, and did not just passively fulfill their service. But what was their agenda, and how did they try to make it work?

We might expect the discussion on the feasibility and validity of methods to be bound to an inner-scientific discourse. But even if such discussions took place only between (professional) scientists, the norms and expectations of the system where these methods had to prove their usefulness certainly influenced the inner-scientific discourse. In the scientific laboratory as well as at the bar and in other social systems, methods are used to make, to justify, and to defend decisions. Thus, juries, judges, tradesmen, administrators, and other clients certainly faced the challenge of evaluating the basis of scientific facts. In contrast to expertise in science itself, expertise in a non-scientific context concerns information transfer only. It is not expected that laypersons be trained in using the scientific methods on their own. Thus, the scientific experts keep the decision makers (laypersons, juries and judges, politicians, etc.) in a dependent position. Seen in the three-categories model presented above, expertise in the non-scientific context applies to the service model only. Data and their interpretation are exchanged, but not the expertise itself. As a consequence, methods-transfer is not the issue. The issue is that of the legitimacy of experts, and the validity of their methods.

Distrust in Science: The Plea for *Normalmethoden*

In the early 1800s, analytical chemistry experienced a major boost (Homburg, 1999). The most famous example of this development was quantitative elementary analysis of organic substances, which contributed to changes in theory and sustained the boom of organic chemistry during the nineteenth century.[2] But qualitative methods were also refined and codified, as in the *Anleitung zur qualitativen chemischen Analyse* of Carl Remigius Fresenius (1818–1897). Published in 1841 as a small treatise describing the methodical efforts of Fresenius during his student times, the book was in the fourteenth edition in 1874, having been translated into almost all major languages, and since 1846 was supplemented by Fresenius's *Anleitung zur quantitativen chemischen Analyse*. Fresenius's method of separating and identifying the chemical elements in a systematic course of precipitations and chemical reactions is – in its principles – still taught in the first year of undergraduate chemistry studies today. Fresenius himself saw the advantages of his treatise (for example when compared to systematic handbooks such as Heinrich Rose's *Handbuch der analytischen Chemie*) in his reduction of the sheer mass of the reactions described, in his emphasis on explaining the theory of each method, and in his care of pointing to pitfalls and red herrings (Czysz, 1988, 40).

The main point, however, was its character as an instruction manual for students who, though needing expertise in chemical analysis, were not focusing on an academic career as a chemist. The book was written for pharmacists, manufacturers, tradesmen, physicians, and agriculturists. In this respect, it mirrored the path of Fresenius himself, who was trained as a pharmacist (apothecary) before he began with university studies in Bonn and Giessen. In Giessen, at the laboratory of Justus Liebig, Fresenius not only became acquainted with a multitude of

[2] The apparatus that greatly facilitated organic elementary analysis was the *Kaliapparat*, invented by Justus Liebig in the early 1830s. Liebig (and the historians dealing with the history of this apparatus follow him in this regard) claimed that the *Kaliapparat* changed elementary analysis from being a task requiring great dexterity, time, and experience (thus, being taken care of by specialists or experienced researchers only) to a more-or-less routine job that could (and had to) be mastered by any graduate student in the laboratory. Thus, in this case a novel tool led to methods transfer in a scientific community. See Usselmann et al. (2005) and Rocke (2000).

new methods, but also from 1842 on taught inorganic analysis to beginners. The majority of Liebig's students went into commercial, industrial, and pharmaceutical businesses.[3] In Giessen, Fresenius wrote the second edition of the *Anleitung*, supplemented with a general, propaedeutic part, and supervised its translation into Dutch, French, English, and Italian. The applied character of his work is visible in the fact that Fresenius accepted a professorship in 1845 at the school of agriculture at the Hof Geisberg in the Duchy of Nassau (from 1866 part of the Prussian province of Hesse-Nassau). Lacking the facilities of Liebig's large teaching laboratory, and facing the incapability of the state administration to supply sufficient funding, Fresenius, in 1848, founded his own private teaching laboratory in near-by Wiesbaden. There, he created and used his own laboratory facilities to establish new analytical methods, to teach them, and most importantly to apply them to a wide range of tasks in industrial, commercial, and agricultural fields. More and more, this endeavor became an autonomous, techno-scientific specialty, underlined by the founding of a journal, the *Zeitschrift für Analytische Chemie* in 1862, which soon became the dominating international publication organ of analytical chemistry. In teaching, Fresenius focused on the level below the university, seeing his main job in educating technicians, but also in educating pharmacists.

In following the mainstream of nineteenth-century chemistry, Fresenius regarded chemistry as the science of the substances, their composition and decomposition, and their reactions. Analytical chemistry had the status of a separate sub-discipline, its aim being decomposition and determination of the substances' constitutive elements. The core of qualitative analysis was the production and presentation of substance components in already known forms: "The value of its method depends on two factors. First, the method has to be infallible, and, second, it should succeed as fast as possible" (Fresenius, 1874, 3). Fresenius believed in the invariability of natural laws, and advised the reader to search for causes of mistakes first in the actions of the experimenter and in the experimental conditions, but not in chemical science. Clearly, this advice was apt for routine findings, but not to tackle the unexpected and uncertain.[4] For Fresenius, analytical work was puzzle solving, and the conclusions had to be true and irrevocable.

Although Fresenius in his textbook expressed his belief in reality and truth, he was very well aware that the accuracy and significance of analytical methods had to be established according to collective norms. Moreover, the irrefutability of methods was never to be achieved completely. For instance, in the early 1840s, when Fresenius worked on the second edition of his instruction manual, he participated in the discussion about the accuracy of the analytical methods to detect arsenic, and

[3] In 1842, Fresenius gave a vivid, and rhetorical description of the work in Liebig's laboratory, emphasizing the learning of analytical methods with samples of known composition (the famous 100 substances that every beginner in Liebig's laboratory had to analyze). For Fresenius, as for Liebig, experiment was the key to learning the language of chemistry. See Fresenius (1842).

[4] After mentioning knowledge of chemical theory, orderliness, cleanliness, and dexterity, Fresenius emphasized trust in natural law as one of the preconditions of success in analytical chemistry (Fresenius, 1874, 4).

tried himself to establish a more reliable method than the ones in use. This happened at a time when the German judicial systems were under reform. The old system – based on the inquisition and taking place *in camera* – was replaced by a public trial. In the late 1840s, the participation of citizens was strengthened further when the jury system was installed, staying in place until 1921. The jurors decided on the facts, while the judges were responsible for juridical questions. But there were continuities between the old and the new legal systems, both for example debating the roles of expert witnesses at court. In this process, circumstantial evidence gained much in importance, and chemists to a certain extent replaced the hitherto prevalent physicians and apothecaries. The newly emerging analytical chemistry allowed an "exact" foundation of findings. Qualitative and quantitative data, being claimed to have their roots in natural law, challenged the rather descriptive and individualized stories of physicians (Heilbron, 1994; Poppen, 1984, 223–232; Wesel, 2001, 467–468).

Up to the 1830s, arsenic was the favorite poison for murder. Available in relatively large amounts, it was easy to apply, and hard to detect. Clinical evidence alone normally could not decide on the cause of death in such cases, and thus chemical trials were called upon. The most commonly used method involved the precipitation of arsenic sulfide, and thus its detection as a yellow precipitate. In order to allow for its detection in metallic form, which was more convincing both in terms of inspection and of unambiguousness, the chemist converted the arsenic sulfide into arsenic. To improve on this tedious and dangerous reaction, the British chemist James Marsh in 1836 introduced a test that yielded metallic arsenic via its hydrogen compound, in this way tremendously improving on sensitivity. Justus Liebig acceded to Marsh's test a sensitivity "beyond any imagination", and the test won acceptance in toxicological trials all over Europe. But exactly its high sensitivity caused more problems than it did good: The test detected arsenic of any origin, including impure reagents, soil environment of the corpses, etc. This fact led to a heated controversy in France between the leading toxicologist of that time, Mateu Orfila of the French Academy of Medicine, and his counterparts at the Academy of Science. Only after textbook authors had chosen the Academy of Science's method as standard in 1841, the Marsh test became an icon for analytical chemistry's reliability and sensitivity (Bertomeu-Sánchez, 2005, 2006).

While the issue was seemingly settled in France, German experts still debated the issue. In September 1842, the chemical and pharmaceutical section of the *Versammlung der Gesellschaft Deutscher Naturforscher und Ärzte* (or GDNÄ; Association of German Natural Philosophers and Physicians) took up the issue. Eduard Herberger, a Kaiserslautern-based chemist, started a discussion with the remark that Marsh's method could be applied in legal affairs only as an inductive, and not as a decisive test. The section members joined in the demand that a procedure had to be found that "should serve as a norm for the chemical method for detecting arsenic in legal cases" (Anonym. 1842). In order to establish such a method, the section founded a commission that should report her findings at the next meeting of the association. The model for the norm that the commission members had in mind was the German pharmacopoeia, establishing methods for the manufacture of pharmaceuticals. In recognizing that the state governments would not carry such an additional burden, Herberger in 1843 proposed to send the finished work to the Bavarian and Prussian academies of science for evaluation.

Fresenius had already worked in Liebig's laboratory on a method to distinguish between arsenic and antimony (both metals can be found in the metallic residue of Marsh's method) in an absolutely certain manner, as he claimed (Fresenius, 1842, 98). In 1843, he gave a talk on his and Lambert von Babo's method at the 21st meeting of the GDNÄ, they being the only members of the above-mentioned commission who had done their share in the agreed-upon workload. Interestingly, Fresenius and von Babo regarded Marsh's method as unsatisfactory for their needs: It did not allow to detect arsenic in every possible composition, and it led to contamination with zinc (by itself a poison) and potential confusion in establishing the purity of the metallic arsenic. Moreover, their aim was to establish the quantity of the arsenic found, thus giving additional hints to the judge with respect to its origins. Accordingly, and in order to be able to function as a legal proof, the final outcome of their carefully described method was a sealed glass tube that contained the metallic arsenic found, and supposed to be part of the legal files (Fresenius and von Babo, 1844).

Preceding an article describing the method and published in the most prestigious German chemical journal *Annalen der Chemie und Pharmacie*, Fresenius announced his general thoughts about the position of the chemist-expert at court (Fresenius, 1844a, b). He used his and von Babo's work on arsenic as an example of the usefulness of governmentally guaranteed methods for dealing with "medico-legal" cases. Although, thanks to the advances in analytical chemistry, arsenic poisoning in the early 1840s belonged to the more easily solvable analytical puzzles (in contrast to poisons that were metabolized such as organic substances, or were present in large quantities in the human body such as phosphorus). The detection of arsenic, in Fresenius's opinion, was an endeavor full of doubts, exactly because of the sheer number of methods that were in use:

> If he chooses one of the old methods, and finds no arsenic thereby, it will be said, "How can a chemist apply such a method? Are we not in possession of improved and far more correct methods? Had he used the Apparatus of Marsh he would have found arsenic." Well, suppose he uses the apparatus of Marsh and detects arsenic, the advocate of the accused will undoubtedly object,–"In what estimation are we to hold these results?–results obtained by means of a method liable to every possible deception and error,–a method the imperfection of which is clearly apparent from the fact that almost every day brings forth some new improvement in it?"

In discussing this problem, Fresenius cited the opinion of a "Biedermann", who acts according to his best knowledge, and does not care about public opinion. This may have been good enough for science, but in legal cases, doubts of lay people had to be accounted for:

> But to this objection I reply that all this does not remove the doubts of the non-professional public, who, in judicial proceedings, see suspicions cast upon the methods of the chemist, – methods that are to lead to proofs upon which the liberty, nay, even the life, of a fellow-creature may be dependent. What must the public think of proofs derived from a science which is exposed to so much danger of being distrusted (Fresenius, 1844b, 404)?

Fresenius believed that apothecaries faced similar problems, because they needed the trust of their clients in their remedies. In order to prevent such cases, the administration stepped in and published norms and rules of how to correctly proceed in the manufacture of pharmaceuticals. Fresenius asked for *Normalmethoden*, normalized methods, guaranteed by the government along similar lines as in the pharmacopoeia. Answering the potential critique that the onset of official methods for a field in constant flux was a fruitless endeavor, he replied that this was the case in all legal affairs: The law had to be adapted to the changing circumstances as well, and the state had to make sure that the *Normalmethoden* were constantly set to the best practice of the day.

In his initial attempts, Fresenius's plea for *Normalmethoden* was not successful. But in 1848, he founded his own institution for developing and teaching analytical methods. Thus, in the long run he was successful, though not in the favored way of governmental standards. Fresenius, and his colleagues, instead acted as discipline builders, developing and certifying methods by actions of an expert community.

As for the interplay of scientific and legal evidence, we recognize that both sides emphasized visible and weighable samples, isolated in pure form. In chemistry, this was the standard practice until the mid-twentieth century (when physical methods changed this). Only under such circumstances did chemists-experts think that they had reached unambiguous results in both the sciences and the law.

Prospects

In this paper we have seen how chemical and physical methods became central for the claiming of expertise. Having both taken into account the appropriation of expertise by scientists and by laypersons, we were able to distinguish between two different types of legitimation: by scientific use and training in the first case, and by officially arranged consent in the second. In both cases, traditional disciplinary demarcations were questioned, but then set firmly in place again: In the example of mass spectrometry, acknowledging the status of expertise involved inter-disciplinary exchange of knowledge but subsequent disciplinary training for disseminating the new method. As for the fate of *Normalmethoden*, the analytical chemists first sought refuge in administrative power to bolster their claims, transcending the disciplinary-organized scientific system as a whole, but in the long run had to rely on inner-disciplinary consensus.

Methods are ideally suited for boundary work, and scientists consciously use them to demarcate their territory, and to expand their influence. In my view, method-oriented scientists enjoy a crucial advantage over their colleagues who "simply" focus on the solution of problems: if the respective method gives answers to problems in a variety of fields, the involved scientists can speak to different audiences without leaving their field of competence. Such situations are a necessary condition for the emergence of an autonomous group: in catering for many scientific, professional, technical, and administrative fields, method makers can avoid a one-sided dependence.

At the end of the nineteenth century, the expertise of analytical chemistry and the demand in many social sectors increased to such an extent that a new profession could arise: the *öffentliche Chemiker*, or public analyst. *Öffentlich*/public did not signify the public sphere in Habermas's dictum; it meant being in public service, thus taking over governmental assignments, mostly in the field of mandatory regulations in the fields of pharmaceuticals, foodstuffs, and public health. Public chemists worked in governmental or private laboratories as well as at universities, and they gathered around their own journals and joined in their own associations. Thus, scientific experts found their area of operation not only in science or industry, but also in governmental regulation and social control. In the middle of the twentieth century, physical methods were taken up very early on by analytical chemists operating in industry and the government (Baird, 1993). But groups of experts in the field of chemistry itself also tended to the needs of their colleagues with the provision of suitable methods. They extended the normal features of scientific expertise, consisting mainly in service work, and engaged in training and various modes of interaction with their clients and colleagues.

The nexus of methods and expertise enables us to expand the micro-studies so prevalent in recent studies of experiment and instrument. With taking the demand side into account, and focusing on the resulting interactions, this adds social dimensions of various kinds: the academic-industrial, the expert-layperson, and the manufacturer-user interactions being just some examples. In addition, such a perspective exceeds the interpretation of instrument diffusion as commercialization, though the issue of commercialization of science is an endeavor that is very worth undertaking (Mody, 2006). I hope that the theme of this essay may contribute in opening up not only additional social dimensions, but also in helping to establish the *longue durée* in studies of scientific experiments and methods.

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Recent Orientations and Reorientations in the Life Sciences

Hans-Jörg Rheinberger

Introduction

The history of the molecular life sciences is inextricably interwoven with the development and exploitation of new research technologies. Theoretical breakthroughs depended essentially on technological tools, including the use of model organisms and molecules as tools. At a closer look, the history of the molecular life sciences during the twentieth century appears to be characterized by two decisive shifts of assemblage in the sense Paul Rabinow has given that term (Rabinow, 2004) and, for that matter, of experimental systems, to use my own terminology (Rheinberger, 2006a). Both shifts were essentially unplanned, unpredicted, and unprecedented in the form they took. As we shall see, the context of application underwent a notable change as well.

The First Molecular Shift

The first shift happened in the two decades between 1940 and 1960. Historians of biology generally agree that this period is the time in which molecular biology came into being. It had its crystallization point in the middle of this period, at the beginning of the 1950s, when the structure of the DNA double helix was characterized, and its apotheosis with the deciphering of the genetic code at its end, between 1960 and 1965.

Generally speaking, the "Path to the Double Helix," to quote the title of a book by Robert Olby (1974), a pioneer in the history of molecular biology, was firstly characterized by a series of new analytical techniques. With the sociologist of science Terry Shinn, we can address these techniques as *research technologies* (Shinn and

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Joerges, 2002) that is technologies designed predominantly with research in mind, such as ultracentrifugation, electron microscopy, X-ray structure analysis, radioactive tracing, chromatography, or electrophoresis, to name just some of them. They had their origin in broadly different research contexts that were often rather remote from biology, at least in the initial phases of their development. Secondly, the period was characterized by the transition from the classical biological model organisms of the early twentieth century, such as the fruit fly *Drosophila* or corn, to new models such as lower fungi, bacteria and viruses. And thirdly, it involved and galvanized cooperation between scientists that extended over several disciplines. The molecularization of the life sciences in general and the molecularization of genetics in particular happened – at least such is the hypothesis that I would defend – by no means as a linear continuation of classical genetics, which had advanced to a kind of biological *Leitwissenschaft*, an instantiation of "general biology," over the first decades of the twentieth century. On the contrary, it came to form an assemblage of its own: a conglomeration of actors, things, instruments, and institutions, in which eventually something "emerge[d] out of a lot of small decisions; decisions that, for sure [were] all conditioned, but not completely predetermined." (Rabinow, 2004, 63) Taken together, these decisions formed a new conjuncture. On the methodological level, this shift was characterized by the deliberate import of analytical procedures from biophysics and biochemistry into the analysis of central biological phenomena. The landscape of these technologies did not by any means take shape under the influence of a new theoretical paradigm, but in the end, they engendered one. What stood at the center of this resulting conceptual shift was a new notion of biological specificity that found its expression in the idea of genetic information and genetic program (Jacob, 1970).

Let me briefly characterize three aspects that, on the material level, appear to me to be characteristic of this first molecular shift in the life sciences of the twentieth century. They comprise first, a new form of experimental systems, in vitro systems; second, a new set of model organisms as a particular kind of tool for exploring life; and third, new technologies for reaching the molecular level.

The first point concerns the development of test-tube systems. Such systems came to characterize the life sciences for most of the twentieth century. The differentiation between in vitro and in vivo was established around the beginning of the twentieth century, after biological chemists such as Eduard Buchner and others demonstrated that it was not only the enzymes secreted by glands, but also cellular ferments, that were able to exert their action outside the cells or organs or the intact organism, in the test tube, if supplemented with appropriate ingredients and subjected to specified buffer conditions. To be sure, working on dead bodies, preparing specimens, and creating extracts had been much older practices in the life sciences. But the in vitro systems of the first half of the twentieth century were different in a crucial respect: they claimed to represent artificial environments in which actions that normally went on within the cells of a living body took place outside the body and the cell. As Herbert Friedmann once put it, what was new was "the implicit recognition that extract repeats or mirrors the living system, i.e. *extract repeats or mirrors* [not just substance, but] *process*." (Friedmann, 1997, 108) As such, these systems marked the transition from an organismic and cellular knowledge regime to a subcellular and finally to a molecular biological knowledge regime. In vitro systems are, as a rule, reduced systems. They expose and enhance certain features of a complex metabolic network by eliminating and purifying away other features. In this way, they are also prone to the production of artifacts, a danger that is inherent in the approach. Therefore, a constant qualification is necessary when relating results of in vitro systems back to the in vivo situation. Much of the history of twentieth century biology is marked by this very specific game of checking and rectification. The majority of research technologies that are characteristic of the advent of molecular biology involved – in their cutting edge applications as well as in their own development – one form or another of an in vitro system. I will come back to one of these technologies later.

But before so doing, let me make a few more remarks on the second aspect, namely model organisms and their use in biological experimental systems (on model organisms, see further Gachelin, 2006). In earlier centuries, in particular in the context of natural history, it was the *differences* between organisms that attracted the interest of researchers who devoted themselves to providing a full picture of the overwhelming diversity of life forms. Under the epistemic regime of the early twentieth century, biological differences between research organisms started to be transformed into *tools* that could be exploited for the characterization of the most general features of living beings. In this perspective, the peculiarities of particular organisms are no longer interesting in and of themselves. They are only interesting insofar as they enable the search for features that can be generalized. A model organism, then, is no longer analyzed in its own right: it is investigated for the sake of something that lies beyond it.

Under these conditions, our present notion of what constitutes a biological model organism came into being. If emerging biology around 1800 had meant to ask what is unique to living beings in contrast to non-living entities, emerging general biology around 1900 meant to ask what features were generalizable across all living beings. One could even say that to abandon simple comparison and become an experimental science itself – to make the transition, in the words of a contemporary, Max Hartmann, from "generalizing induction" to "exact induction" (Hartmann, 1927, 5–11) – general biology as envisaged around 1900, in particular the science of heredity, *had* to create model organisms to work with.

It is in this context, from the beginning of the twentieth century, that model organisms started to play an increasing role in research on heredity, the constitution of cells, and embryological development. But this also meant that, in this phase of development of the life sciences, organisms were no longer chosen primarily for their agricultural relevance. In order to function as research tools, model organisms needed, first and foremost, to be embedded in and therefore fit experimental systems, where they could play out their dynamics as living tools. In vitro systems in particular are unthinkable without standardized model organisms. Yeast, whose different strains originated in the brewing and baking business, accompanied the in vitro process from the beginning. But eventually, bacteria such as *Escherichia coli* took over. The entrenchment of these organisms in experimental systems, as a rule, had not only material *prerequisites* in terms of experimental systems, but also material *consequences* in terms of their own constitution. In the course of the work, the model organisms under investigation became modified organisms. *Model* organisms are thus, as a rule, also organisms *modified* for particular research purposes.

The third element of the experimental culture of molecular biology I would like to briefly characterize is research technologies. I will not review their whole array here, but rather concentrate on a particular example: radioactive tracing. Radioactive tracing became a key technology of biochemistry and molecular biology within a decade after World War II, and it illustrates that the development of molecular biology through research technologies – in particular the raw materials they require – is inseparable from the age of atomic physics and with that, from the broader context of the technologies of atomic energy.

The production of radioactive phosphorus, sulfur, hydrogen, and finally carbon – key elements in all biological molecules – was realized in principle with the advent of cyclotrons during the 1930s. But it was only in the middle of the 1940s that these materials became available in larger quantities as an offshoot of early reactor technology and the first atomic piles in the United States. As a consequence, more complex, radioactively labeled molecules could be produced by means of biochemical manipulation and introduced into biological assays. It thus became possible to mark molecules such as amino acids, the building blocks of proteins, or nucleotides, the building blocks of nucleic acids, with radioactive labels. Until their decay, these molecules behaved chemically and physiologically just as their unlabeled counterparts. Consequently, what physiologists had in their hands was a kind of probe that could be introduced into certain chains of metabolism, where it participated in the appropriate reactions. When the isotope decayed, it released a signal that could be recorded, that is, it left a trace at the time and place of its breakdown.

Now, the availability of these biologically relevant, weakly radioactive isotopes in turn had an impact on the measurement technology. In the small amounts optimal for biological assays, they could no longer be measured reliably with traditional Geiger-Müller tubes. I cannot go into detail here on the history of "liquid scintillation counting," a technology without which the wide use of the new isotopes would have remained impossible (Rheinberger, 2001, 2006b, Chapter 9). Here it suffices to say that from a biological perspective, this measurement technology brought not only the signals of weak β-emitters such as ^{14}C and ^{3}H into the realm of the measurable; it also provided, so to speak, a "wet" intersection with test-tube experiments involving organic material. The liquid scintillation counter could even accommodate fluid samples containing a certain amount of water. Under these conditions, most biological samples could easily be made ready for radioactive measurement. The new counting technology came into being in close interaction with the revolutionary transformation of biological chemistry by radioactive tracing in the course of the 1950s, and by the beginning of the 1960s, it had conquered the laboratories. The massive amount of in vitro experimentation demanded to identify the genetic code would have been unthinkable without an automated form of the liquid scintillation counter.

The introduction of radioactivity into the laboratory cultures of biology also had broader consequences for laboratory architecture and even laboratory life as a whole. It became a hallmark of molecular biology. However, the possibility of measuring radioactive traces of minimal strength in biological samples required that the experimental environment remain uncontaminated by the radioactive probe. That condition not only resulted in a completely new laboratory regime with separate spaces for radioactive experimentation; it also had massive effects on the very design and form of the experiments themselves.

These few remarks may suffice to indicate that the technology of radioactive tracing cannot be reduced to either an instrument or a substance. Rather it formed a kind of capillary network that, with its components, penetrated and permeated a whole experimental culture. It introduced an indicator principle into the analysis of metabolic processes, and with that, it oriented biological chemistry as a whole in the direction of an in vitro experimental regime that could not have been developed without tracer technology, for the production of radioactive traces in the test tube also meant the possibility of bypassing a long-held principle of chemical measurement. Classically, chemical substances, to be measured at all, had to be rendered in as pure a form as possible and in sufficient amounts for analytical microdetermination. In contrast, radioactive measurements could be performed without removing the "impure" background of a mixture of all sorts of cellular components – of course, with the accompanying hazards and pitfalls. In addition, sensitivity was enhanced by almost half a dozen orders of magnitude – with the accompanying hazards and pitfalls, too. And as already mentioned, radioactive tracing was also the driving force for the development of new measurement technologies, in fact of a whole research technology industry whose integration into the experimental systems of molecular biology not only altered the size of those systems, but also their structure and disposition. Finally, radioactive tracing became the material point of mediation between the know-how of biologists, chemists, physicists, and engineers. It was thus a technology that in its very material structure – the chemistry of liquid scintillation, the physics of photo-multiplication, the engineering of electronics, and the biology of sample preparation – displayed the new interdisciplinarity of molecular biology in a paradigmatic fashion.

The Second Molecular Shift

The second shift of assemblage, which I want to briefly describe now, took place in the course of the 1970s. It marked the beginnings of what we have come to know as gene technology and genetic engineering. It happened at a point in time when major players of the first phase of molecular biology left the field in search of new challenges, mostly in neurobiology. As we shall see, this shift brought a change in all three aspects described above: it brought completely new kinds of technology into play; it switched from lower organisms to higher organisms as models, in particular man himself; and it led to a new era of in vivo experimentation.

All three aspects are intertwined. The new assemblage is characterized by the introduction of *molecular* tools in the proper sense of the word, that is, technologies in which the biological macromolecules themselves, in particular the two central classes of macromolecules, proteins (enzymes) and nucleic acids, play a major role. Trimmed plasmids and other nucleic acid vectors, restriction enzymes, and the notorious polymerase chain reaction, in which a DNA polymerase is the central actor, are examples of such molecular biological tools. Elsewhere, I have characterized the decisive step of this phase as the transition from the "extracellular" representation of intracellular structures and processes – the first shift – to the "intracellular" representation of an extracellular project – the second shift (Rheinberger, 2000). Classical molecular biology of the first period was biology that operated and was driven by the methods of biophysics and biochemistry, heavy analytical apparatus, big machines as a rule. Gene technological molecular biology continues to make use of heavy analytical instruments, in particular in their automated form, but essentially, it is biology driven by molecular tools that operate in the space of the living cell itself. It is thoroughly constructive and synthetic. As Waclaw Szybalski, one of the contemporary observers and himself an oncologist at the McArdle Laboratory in Madison, stated in 1978 on the occasion of the Nobel Prize award to Werner Arber, Hamilton Smith, and Daniel Nathans for the characterization of the first restriction enzymes: "The work on restriction nucleases not only permits us easily to construct recombinant DNA molecules and to analyze individual genes but also has led us into the new era of 'synthetic biology' where not only existing genes are described and analyzed but also new gene arrangements can be constructed and evaluated." (Szybalski and Skalka, 1978)

From then on, the in vitro culture of twentieth century biology started to be paralleled and supplemented with an in vivo culture of an unprecedented kind – the manipulations were shifted from the test tube right into the cells of the organism. Not that the earlier technologies were simply replaced. On top of them, a new mode of doing biology came into being. To put it all in one sentence, we could claim that if in the first, analytic mode of molecular biology, the determination of a phenomenon – at least in principle – preceded its further application, in the second, synthetic mode, it is application that precedes determination. With that, understanding also often only follows application.

The main reason for this is that the cellular environment cannot be controlled in the same way an experimental test-tube environment can be controlled in terms of a limited number of parameters. This new mode of doing science is therefore intrinsically imprecise and application driven and must by necessity follow a mode of experimentation that the historian of science Friedrich Steinle has characterized as "exploratory." (Steinle, 2002, 2005) This aspect of modern experimentation is certainly not exclusive to "mode two" molecular biology, as we could call the second assemblage. If we follow Gaston Bachelard and his concept of "applied rationalism," it is even constitutive for the modern sciences in toto (Bachelard, 1949; Rheinberger, 2005, 2006b, Chapter 2). According to Bachelard, application is not extrinsic to modern knowledge, it is not something added after the fact to some epistemic core that preexists; it exerts its action at the very level of concept formation itself. Application belongs to the essence of the modern sciences themselves. In *The Formation of the Scientific Spirit*, Bachelard formulates this as follows: "In order to accommodate new experimental proofs, one must [. . .] *deform* the primitive concepts. One must not only study the conditions of application of these concepts, but one must incorporate *the conditions of application of a concept into the very meaning of the concept itself*." (Bachelard, 1938, 61) In "mode two" molecular biology however, applicability is particularly prominent and presents itself in an exemplary fashion.

In summary, we could state with Stephen Toulmin: "A nutshell definition of science – as of anything else – inevitably floats around on the surface. An investigation of any depth forces us to recognize that the truth is much more complex. To understand the ways in which [. . .] scientific ideas differ, in any age [. . .] calls for a painstaking and laborious study: only in this way shall we bring to light the manifold functions that science has performed, performs now, and *might* perform in the future within our whole intellectual economy." (Toulmin, 1963, 15)

In light of this not only intellectual – but first and foremost material epistemic – economy, it will without doubt be rewarding to think in more detail about the dynamics that reoriented the disciplinary landscape of the life sciences during the course of the twentieth century and to try to understand better how accordingly the boundaries between biology, physics, and chemistry have become reconfigured during the age of molecular biology. In addition, the boundaries between biology and medicine are right now, in the age of genetic engineering, also being profoundly reconfigured. Moreover, the admittedly strong hypothesis would have to be considered as to whether the classical disciplines, as shaped during the nineteenth century, have not altogether entered into a process of dissolution, a process in which the molecularization of biology would then only be a particularly prominent example. If this were the case, it would of course not be inconsequential for the possibility – or the increasingly likely impossibility – of understanding the dynamics of the contemporary sciences in the framework of disciplinary histories. Paul Forman has talked in this context about a recent "devaluation of disciplines." He has seen this trend relying not only on the growing problem orientation, but also the growing economization – an aspect on which I cannot further elaborate here – of the contemporary sciences: "This reorientation toward the market [. . .] together with the increasing orientation toward the particular problem, works powerfully to dissolve the scientist's attachment to his discipline, indeed to dissolve the disciplines themselves and their disciplinary authority." (Forman, 1997, 185, 189) To repeat Toulmin's conditional just mentioned, we do not know, at present, what form and function the sciences will take as a result of this process.

Let me end on a final remark about model organisms in medicine that connects to this issue. I have talked about the peculiar fates certain organisms had as tools of research in the in vitro systems of the first phase of molecular biology. Now, with the recent achievements in the molecular life sciences of human tissue cultivation, of cell proliferation in Petri dishes, of test-tube fertilization, of cellular cloning, and of adult and embryonic stem cell manipulation in the reproductive and developmental biology of man, it appears that we are entering in an epoch of the eclipse of *animal models* in medicine and human research. The mode of reaching the molecular level in the test tube and the subsequent molecular re-cellularization of research has created the perspective of an investigation of human specificity without intermediate animal models. Many experiments are now being carried out with human cells directly. The potentials of this research mode, however, in particular with respect to reproduction, embryonic development, and differentiation have also created the need to discuss new boundaries for human experimentation. I said that model organisms are always modified organisms. With man becoming, in a sense, a model organism of his own, "modeling" inevitably takes on the meaning and the form of *human modification*. There is thus a need to discuss how far such modification – in particular genetic modification – can and should go for the sake of exploring new ways of living in the future.

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Transforming Objects into Data: How Minute Technicalities of Recording "Species Location" Entrench a Basic Challenge for Biodiversity

Ayelet Shavit and James Griesemer

Introduction

Joseph Grinnell (1877–1939) was the founding director of the Museum of Vertebrate Zoology, Berkeley (MVZ). He conduced extensive and intensive surveys of vertebrate species distribution throughout California, USA.[1] The problem we track throughout this essay is that Grinnell's carefully laid plans for his museum at the beginning of the twentieth century embodied a notion of locality and associated technology of recording that turned out to be hard to unify with a very different notion that emerged in mid-twentieth century ecology and is embedded in late twentieth century computing practices involving the collection, storage and retrieval of species locality data. Different concepts of space favor different protocols and ways of describing a species' locality, and thus slowly entrench only certain courses of action in practice, which over time hinders the integration of data.

From 1911 to 1920, Grinnell and colleagues surveyed the vertebrates (small mammals, birds, reptiles, and amphibians) in a large transect across central California, including Yosemite National Park (http://mvz.berkeley.edu/Grinnell/yosemite/index.html).[2] In addition to months of planning and years of development since the founding of the MVZ in 1908, the survey work involved 957 person-days of fieldwork, resulting in over 2,000 pages of field notes, 817 photographs, and 2,795 specimens. The work was summarized in a widely-read book, *Animal Life in the Yosemite* (1924), by Grinnell and Tracy Storer. From 2003 to 2005, Grinnell's 21

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[1] On the history of intensive and extensive biodiversity surveys see Kohler (2006).

[2] Accessed June 21, 2008. Field notes document that Grinnell and his colleagues were trapping in Yosemite in 1911. His book on Yosemite (see below), describing the transect discussed here, covers work beginning in 1914. The 2003 Yosemite Report describes the original survey as taking place from 1911 to 1919. The index page at the resurvey website describes it as 1914 to 1920. The 2007 Inventory and Monitoring report describes it as 1911–1920. These date discrepancies are typical of complex, multi-investigator projects and are often due to differences in whether the description concerns field seasons, project authorization or funding cycles, or report due dates.

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sites from his original Yosemite survey were revisited by MVZ personnel in a resurvey project led by the current museum director, Craig Moritz, the former director, James Patton, and with the collaboration of the US Geological Survey.[3]

Grinnell aimed at *comprehensive* faunal surveying and meticulous recording of information about specimens and habitats in field notes, specimen tags, and museum catalogs, procedures which were designed expressly to be as widely useful to researchers of the future as possible, whatever their questions may be. In the resurvey, Grinnell's academic descendants began by taking up his particular questions but in a technological context he could not have foreseen. The twenty-first century resurvey aimed at quantifiable, machine-code-able data which were designed to be internet-accessible and "interoperable"[4] with data from other databases produced for a variety of overlapping purposes. The resurvey's field work began in 2003 and, in 2006, its scope expanded to more field sites within and beyond Yosemite National Park.[5]

The aims of the original survey included establishment of "a new baseline against which future studies could be compared in order to measure faunal change over time. Grinnell encouraged future scientists to make these comparisons" (http://mvz.berkeley.edu/Grinnell/pdf/Yosemite_2003_Report.pdf).[6] The resurvey at Yosemite implemented this goal by attempting to provide new data on old localities and new uses for old data. The comparison included old research questions – which species occupy which locality – with contemporary practical applications and policy implications such as the role of climate in producing faunal change (http://mvz.berkeley.edu/Grinnell/research/index.html).[7] For the purposes of this essay, we focus primarily on comparisons of the original surveys and resurveys, with only brief comments on the complex transformations of ecological sciences in relation to the museum's goals, practices and methods.

We frame our case study of this emerging re-survey problem in terms of differing concepts of biological space. In one important sense, space is a framework "exogenous" to the organisms and species of scientific interest that human investigators impose in order to pursue their interests and goals with the skills, abilities, and resources available to them. Organisms live in this "space" in so far as investigators act and describe them as such, but the nature and details of the imposed framework are not relevant to what the organisms do. For example, if human investigators

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[3] The resurvey project was funded by the NPS (Inventory and Monitoring Program), Yosemite National Park and Patton's private resources.

[4] "Interoperability is the ability of two or more systems or components to exchange information and to use the information that has been exchanged." IEEE (1990, 42).

[5] Funding outside Yosemite was provided by NSF, and within Yosemite by the National Geographic Society and the former director's private resources.

[6] Accessed July 18, 2008.

[7] Accessed July 18, 2008. This was one of Grinnell's original theoretical goals (see Griesemer, 1990). Policy options for intervention due to climate change were not an integral part of either of these studies per se, but the policy implications were clear: see, e.g. Nijhuis (2005, accessed 24 June 2008).

define a system of grid lines – latitudes and longitudes conventionally located with respect to the Earth's poles, equator, and Greenwich England as prime meridian – plus elevations above or below sea level (at some arbitrary date), organisms and species live at points in that coordinate system independently of the *existence* of that socially, conventionally imposed system of description. The organisms do not care about, nor can they exploit, their "lat/long" coordinates as they go about their lives in those "locations."

At the other pole from this "exogenous" concept of space, there is an "endogenous" concept of space that depends on, or bears significant relation to, the organisms themselves, i.e. of places or locations conditioned by what the organisms in question do, what their interests and abilities are, and without regard to the interests, skills, and abilities of any humans that might wish to study them. We call this concept a "species-interactionist" concept of space because the species (and organisms) themselves contribute to the organization of the "coordinate" space in which they live. Their location in this space is determined by their interactions, such as digging burrows or building river dams. In this sense, space is "endogenous" or the *product* of the interaction of the organisms and *their* environments.[8]

In most cases of interest for understanding scientific investigation of organisms, species, and their environments, the relevant perspective on space must be sensitive to the *interaction* of human investigators with the organisms subject to study. One can imagine human investigators placing quadrats in locations following a protocol for random sampling from a grid of exogenously imposed coordinates, but suppose the random number generator specifies dropping a quadrat on a slope too steep for humans to hike down and access? Or, one can imagine human investigators identifying locations along a trail created by the deer species they are following, but the brush becomes too thick for the humans to follow and they have to take a detour, hoping that they will intersect the deer trail further on. *The locations where humans study organisms in nature are intersections of human interests, skills, abilities, and resources with study organisms' interests, skills, abilities, and resources.* In this sense, space is the product of *species–species* interactions and is neither fully imposed by the humans, nor fully independent of them.

Our case study will show how exogenous and species-interactionist concepts of space shape biodiversity research. The MVZ's founding director and his contemporary successors sought "hypothesis-neutral" descriptions of specimen localities so that collected material would be as widely useful to the student of the future as possible, yet this gap between two concepts of space slowed, and sometimes literally stopped, their project. We argue that a resolution of the problem of locality lies in practical accommodation of divergent technical means, different kinds of descriptions, and different concepts of space through alternation of their use in the research

[8] As developmental systems theorists and niche constructionists note, these "environments" include other organisms, so ecological and evolutionary theories involving this sort of concept of environment will be more complicated than traditional theories assuming that environments are specified entirely "exogenously" to the organisms living in them.

process, rather than forcing a choice for the sake of nominal unity. In the next section, we turn to the details of our case study. We follow that with two theoretical sections, first formulating a set of contrasts between the different concepts of space and kinds of locality descriptions; and second discussing the MVZ's resolution of the problem of locality as both an achievement and a mark of continuing challenge for global biodiversity research.

A Story: Application of "Locality" Records in the History of the MVZ

The Museum of Vertebrate Zoology (MVZ) was established by the patron and entrepreneur Annie Alexander and the scientific director Joseph Grinnell (Stein, 2001). Grinnell noticed the rapid demographic and economic changes in California, was aware of its geologic movements (Grinnell, 1917, 1924), and envisioned his museum as a supplier of facts describing these changes guided by his advice on how best to handle them:[9] "serving as a bureau of information within our general field."[10] More specifically, the museum researchers and students were to conduct a series of rigorous descriptions of species and sub-species distributions in the same localities over time "with application of the 'laboratory method' out of doors as well as in the Museum."[11] Applying the "laboratory method" meant standardization of the work structure and quantification of the resulting data (Kohler, 2002), which in a natural history museum meant introducing "index cards and standardized note-taking procedures [which] were the high-tech cutting edge of museum practice, as they were in industry" (Gerson, 2007b). Grinnell was so keen on implementing new technologies that he defined it as one of the duties of a museum director: "Be alert for improvement of methods in every department."[12]

In line with this duty, a huge effort was devoted by Grinnell and the MVZ staff to build standardized, detailed protocols for almost every aspect of work in the museum down to the kind of ink and paper to use and train students to follow these procedures – the mandatory "Zoology 114" course (see Griesemer, 1990, Sunderland submitted). There was an 8 page written standard for recording information in field notes: "suggestions as to collection and field note taking,"[13] distinguishing between the information about collecting specimens, to be recorded on a specimen tag and

[9] In many respects the MVZ functioned in ways aptly described by Latour's "center for calculation" (Latour, 1999, see also Shavit and Griesemer, 2009).

[10] Grinnell, J., "Analysis of Functions," 2, November 22, 1935, an official document signed by Grinnell, Alexander and Sproul (the University President). MVZ Archive, located at the MVZ main gallery, top left cabinet, file name: Museum Methods – Historical.

[11] Ibid., 1.

[12] Grinnell, J., *Schedule of Curatorial Duties for Staff Members*, 5, August 15, 1929, MVZ Archive, MVZ main gallery, top left cabinet, file name: Museum Methods – Historical.

[13] April 20, 1938. MVZ archive, MVZ main gallery, top left cabinet, file name: Museum Methods – Historical.

other information to be written in field notes, and yet another protocol for transforming the information from the tag into the museum's collection via index cards: "suggestion for handling specimens brought fresh into museum and intended for collections of the museum of vertebrate zoology".[14] This minute procedural decision to distinguish between two kinds of techniques to record information about a species' locality – field notes and tags – is a crucial point in our story, one we shall return to.

Diligent execution and updating of these (and many more) protocols whenever new techniques became relevant, was, and still is, expected of every museum member.[15] The reason is obvious. For an organization containing multiple researchers and students (today around 140 people), shared standards for recording distribution facts greatly eases the production and use of data in pursuit of the MVZ's diverse goals and research questions: basic science on the evolution of speciation and extinction, applied science dedicated to "the promotion of wildlife conservation and management on a biologically sound basis of fact and principle,"[16] and, last but not least, facilitation of Grinnell's and Alexander's ambition "to establish a center of authority on this coast."[17] All in all, the museum's main goal was both ambitious and foresighted, as explicated by Grinnell as early as 1910:

> At this point I wish to emphasize what I believe will ultimately prove to be the greatest value of our museum. This value will not, however, be realized until the lapse of many years, possibly a century, assuming that our material is safely preserved. And this is that the student of the future will have access to the original record of faunal conditions in California and the west wherever we now work (Grinnell, 1910 [1943, 35]).

These words are printed on the museum's walls, posted on its website, and have guided the Grinnell Resurvey project, a major project of the MVZ, for the past 6 years.[18] The founding director also had a very broad perspective on what "the original record of faunal conditions" amounts to. Like any other research museum, the MVZ held a collection of material objects, i.e. specimens, although its curators considered not only traditional animal parts but also nests, eggs, and feces to be specimens, which were similarly tagged and stored in cabinets. These tags, sometimes called specimen labels, are small pieces of paper attached by the collector at the end of the day to organisms obtained and prepared as specimens during that day. The specimen tag was the crucial piece of evidence guiding the handling of the specimen later on, upon its arrival to the museum. A protocol dictates what and how to write on the small tag. One can catch a glimpse of the MVZ's meticulous and socially-intertwined work culture just from reading these words from 1925,

[14] November 13, 1925. MVZ archive, MVZ main gallery, top left cabinet, file name: Museum Methods – Historical.

[15] Interviews with senior staff, April 18, 2006 and May 1, 2006.

[16] Grinnell, J., April 20, 1938, 2, MVZ archive, MVZ main gallery, top left cabinet, file name: Museum Methods – Historical.

[17] Grinnell J. letter to Alexander A., 1907, cited in Griesemer and Gerson (1993, 198).

[18] Interviews with senior staff, March 28, 2006 and May 1, 2006.

recommended by the museum's secretary, Margaret W. Wythe, and endorsed by its director, Joseph Grinnell:

Attach label, immediately, to each specimen, bearing the following information:

(a) Name of collector or collectors (upper right corner of label).
(b) Preparator's field or note-book number (upper left corner of label).
(c) Exact locality, that is, name of a topographic feature and distance from nearest town; elevation; county; state (across middle of label).
(d) Date: month (in writing, not numeral), day and year (Lower right corner of label).[19]

Note the standardized format for recording the "exact locality" by these one or two descriptive sentences at the center of the small label. It should be further noted that "Exact" does not mean the smallest degree of geographical extent but the smallest degree of vagueness in description. Grinnell did not think that reducing the geographical extent of a locality would always enhance its value for current and future research. Rather, a valuable "locality" is sensitive both to one's research question and organism of study. For example, the home range of a deer mouse typically encompasses a whole trap line (a string of traps), so it is a matter of chance in which trap within this trap line it will be caught. In addition, every trap within the trap line should be set in order to maximize the detection of that mouse, since, given Grinnell's research interest, that specific location might only be re-visited a century later, if at all. As a result of this mouse–human interaction, the relevant description of this species locality is the trap line rather than the single trap.

Others who were interested in questions other than Grinnell's sometimes disagreed. In 1914, one of Grinnell's prominent students, Tracy I. Storer, proposed to record the ecological niche of its unique trap on a small tag attached to each specimen, yet Grinnell politely waved his proposal away.[20] Almost a century later, a suggestion to record GPS coordinates for each trap in a trap line was raised by the leading ecologist in the Grinnell resurvey's project and supported by the leading programmer analyst. According to this view the only certain location is that where the animal was actually trapped. The mouse *was* there, and therefore could be demonstrably be there. The same cannot be rigorously said of any other place in the extent of the trap line, because the human configuration of a "trap line" introduces assumptions and biases into the detection of "habitat" and "micro-habitat". Surely the entire contents of the trap line are not suitable as an environment for the mouse, even if every trap in the trap line does occupy such a suitable locality.[21]

[19] Wythe, M., "Suggestion for handling specimens brought fresh into museum and intended for collections of the museum of vertebrate zoology," November 13, 1925. MVZ archive, the MVZ main gallery, top left cabinet, file name: Museum Methods – Historical.

[20] Storer T. to Grinnell J., November 24, 1914; Grinnell J. to Storer, T., December 4, 1914, MVZ Letter Correspondence Archive, MVZ main Gallery. We thank Elihu Gerson for first mentioning this correspondence to us.

[21] We thank John Wieczorek for this comment.

The suggestion to use GPS coordinates was similarly declined by the leading naturalists on the resurvey team. The naturalists argued that the time it would take to record this information in the field would be overkill, both literally, since more small rodents will die while fifty trap locations are being recorded, and metaphorically, since such precise locality data is not relevant for describing species distribution from only one or more individuals from each species, and especially not from species that typically occupy a home range that encompasses an entire trap line. According to this view, the results could be misleading if aggregated from localities too small in geographical extent.[22]

Under the Grinnellian method, when a trap line is set its specific geographical and ecological setting is meticulously recorded in one's field notebook *journal*. Later on, the next morning or afternoon, when an animal is obtained from a trap, its general locality – usually the area around the campsite – is recorded in one's field notebook *catalog* according to the standard presented above. Still later, in the evening, typically after a bit of rest and food in the campsite, and always after long hours spent measuring and preparing each and every animal fit for preservation, one then copied that same "locality" from one's field catalog to the specimen tag attached to the skinned animal. One copies the same general locality description that stands as the page heading in one's journal, and goes on to describe -– and whenever practically possible quantify – properties of the specific localities encountered throughout that day: their landscape, slope, weather, snow level, dominant plants, other plants relevant to animals, soil types, the method and effort of detection (i.e. the kinds of traps used, the number of traps and trap types in each trap line and the length of time each trap line remained open) along with the name, title, and biologically relevant opinion of any local person met that day. This longish record from the journal, written in free text format, has no specific name while the record from the catalog and specimen tag is called the "verbatim locality."

Once the specimens were brought in from the field, their locality records entered the MVZ's collection. Of all the various kinds of data fields one recorded and entered into the main MVZ card catalog (today, the MVZ database), only these few sentences of the verbatim locality[23] will never be changed or corrected by other museum personnel, even if obvious spelling or identification mistakes have been made in the field. The reason is that this small piece of paper connects the specimen to its spatio-temporal context *as originally recorded*, "and so, reversely the student [of today] may quickly trace back again from any particular specimen its history, by referring to the card catalogue and field notebook" (Grinnell, 1910 [1943, 34]). Changing the tag wording might break this chain of reference and thus disconnect a specimen from its recorded environment (Gannett and Griesemer, 2004; Latour, 1999). Given the extensive contextual information that one was required to store in

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[22] Grinnell resurvey meeting, January 23, 2007. For more on the biological rationale for this view see Shavit and Griesemer (2009).

[23] In that sense this "verbatim locality" differs from the "specific locality" which is only written in the museum and entered later into the MVZ database under the category of "locality."

one's field notes, it is not surprising that, for Grinnell, a specimen without a label to trace it to the individual collector's catalog and field notes is, again, considered "lost. It had, perhaps, better not exist" (Grinnell, 1921 [1943, 108]). To add visual context to the descriptions, photographs were taken (of habitats, localities and specimens) and drawings made on TRS maps (topographical and route maps). All these items were stored in the MVZ archives and all are traceable to each individual specimen stored in the collection. Grinnell was farsighted enough to stress that one type of record is not more important than another, since we never know what type of record will be required in the future:

> It will be observed, then, that our efforts are not merely to accumulate as great a mass of animal remains as possible. On the contrary, we are expending even more time than would be required for the collection of the specimens alone, in rendering what we do obtain as permanently valuable as we know how, to the ecologist as well as the systematist. It is quite probable that the facts of distribution, life history, and economic status may finally prove to be of more far-reaching value, than whatever information is obtainable exclusively from the specimens themselves (Grinnell, 1910 [1943, 34–35]).

Although Grinnell stressed the need to use *both* the narrative description in a field notebook journal and the structured description on a small specimen tag, he introduced this distinction to facilitate the widest utility of collected material. Although specimen tags might be sufficient for some taxonomic purposes, the field notes might be of broader significance to ecologists and systematists – specimens merely documenting the presence of a given species in an ecological context (Griesemer, 1990).

Grinnell's sudden death in 1939 brought a brief period of turmoil, which subsided after his former student, Alden H. Miller, was appointed director (Stein, 2001, 255–258). Over the next three decades, Miller carefully nourished Grinnell's legacy. During Miller's administration, Grinnell's distinction between information to be recorded in the "journal" versus the "catalog" was embodied in a clear physical separation as distinct sections within the same notebook and later as separate notebooks. Miller's segregation move made each type of record more homogeneous, further rationalized the workflow, and later made the coding of the specimen tag in a database table much easier to do.[24]

The "verbatim locality" written on the field catalog and the specimen tag prioritized the standardized descriptive locality as the most reliable format to ensure a revisit to any corner of the world where MVZ people work. Information that was not always available during the 1950s and 1960s, such as TRS coordinates, was therefore not included in the standard format of the tag. Another record, the "specific locality," typically comprised of TRS and later lat/long coordinates, was written in one's field notebook catalog and journal. The "specific locality" quickly became

[24] It is much quicker and easier to find the information one needs to copy from the specimen tag/field catalog (the tags and the field catalog hold identical information) to the collection catalog (the index cards at the museum) if there is clear physical separation between "catalog" and "journal" in the field notebooks. For a clear analysis of rationalized coordination see Gerson (2007a).

a necessary part of field notebook catalogs and as part of journal entry headings. However, the *primacy* of an abstract point on a universal grid, referenced by a number with an unequivocal interpretation, was not possible before the MVZ collection was computerized into a database. It was then, for the first time, that a *commitment was made* to a single concept of space – exogenous from the landscape and its inhabitants rather than sensitive to it – to be applied in recording a "locality" In the database.[25] Throughout the late 1970s the MVZ collection records were entered into a computerized database and by 1998 it was, as far as we know, the first collection of modern vertebrates in the world to go online.

Prior to the MVZ's shift to a database collection, one of the forces motivating computerization of records was passage of several environmental laws in the first half of the 1970s. "The National Environmental Policy Act" (NEPA), signed on January 1, 1970 by US President Richard Nixon, required that prior to any major US federal act a statement assessing environmental impact (EIS) on species must be filed. The Endangered Species Act (ESA), signed by Nixon on December 28, 1973, likewise created a need for information about species distributions. Soon thereafter a boom of private companies specializing in assessing environmental impact emerged, and they started arriving at museum collections looking for information. In 1972 the American Society of Mammalogists responded by establishing a committee on Information. That committee, which included an MVZ representative, established a common set of standards for database development, so that data would be compatible across all American museum collections.[26] The NSF recognized the role of natural history museums for society at large and in 1972 built a special program titled: "Biological Research and Resources," to which museums could apply for funding of cabinets, fumigation equipment, etc. to maintain their collections.

However, if the MVZ was to continue its role as a "bureau of information," it not only had to store information but also to supply it quickly and efficiently to the public. But how? By the mid nineteen seventies there were already over four hundred thousand specimens in the MVZ collection and reviewing them all in order to find which species were present in Yosemite National Park was an enormous task.[27] Luckily, the technology to do just that was already spreading in the life sciences: mainframe computers. By the mid 1970s mainframe computers became routinely used in museums, and the NSF responded by expanding its existing funding program to include information technology, now titled: "Biotic Research and System Support." The director of this NSF program, William Sievers, encouraged Jim Patton of the MVZ and Philip Myers of the University of Michigan to jointly propose a grant to computerize the MVZ's and UMMZ's collections and make available a database management system for other museums. In 1978 they received an

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[25] This commitment need not have been explicit. An exogenous or endogenous concept of space could be "chosen" indirectly by favoring certain properties used in descriptions of locality over others, e.g. lat/long over plant cover.

[26] Correspondence, September 3, 2008.

[27] That same request from Yosemite National Park was repeated in 2001, yet this time it marked the beginning of the MVZ's ambitious "Grinnell Resurvey Project."

NSF grant for retrospective capture of information of the Mammalian collection. The grant compelled the museum to decide on the types of information to record in the database. Given that the free-text locality information of the field journal would be hard to code in a systematic way, decisions about what information to record in the database entailed trade-offs in future searchability of information about locality and implied, in turn, commitments to the relative significance of different concepts of locality. Specifically and practically, the question of what locality information to code in the database was whether "locality" information would be extracted from the field journal, the specimen tag or both.

The answer was obvious. The information that the database software (TAXIR: Taxonomic Information Retrieval) could query needed to be highly standardized and organized within a single table ("flat file"), in addition to taking as little space as possible, given the processing power and storage limitations of 1970s mainframe computers. The short, standardized descriptive locality recorded on the specimen tag fitted that technical demand nicely, while the lengthy free-text record in the field journal could only be stored but not searched or queried upon in a flexible manner. Perhaps the main reason to leave aside the field notes, however, was the NSF's explicit interest, and consequently Patton's and Myer's explicit focus in their proposal, in the *specimen* collection, which – by Grinnell's own distinction – was available first and foremost from the specimen tag information.[28] The field journal lacked information considered crucial for a collection – such as museum catalog or accession number – and held vast field ecological information that was time consuming to retrieve.

In 1980 the MVZ's database became operable. That is, a person sending a question by mail about which species were found in Yosemite National Park could receive a written answer – a list of species' locations in counties that spanned the park – within a few days after his query was entered into the mainframe computer and, within hours, a result was printed. As a result, queries about a taxon – e.g. genus, species, sub-species – found at a certain point on a map could be answered quickly while all the environmental, geographical and historical information contained in and distributed among the field journals about that species at that time/space point could not because it was not machine searchable. De facto, this meant, according to anecdotal comments of current MVZ staff members, that queries about the extensive locality records stored in the field note journals were reduced from now on. Not all who queried the MVZ database also showed interest in the information stored in the field journals, however.

"Foregrounding" readily available locality information thus unintentionally "backgrounded" a large source of ecological locality information. This did not raise any complaint from most database users concerned with species distribution questions, which implied that an abstract point locality became not only necessary but also *sufficient* for most of their research questions utilizing the museum collection.

[28] As mentioned, identical locality information appeared on the specimen tag, field catalog, and museum catalog cards, hence coding was done from either format. The original tag was used for entering bird taxa while catalog cards were used for mammal and reptile taxa. We thank James Patton for this comment.

To be sure, some behavioral ecologists and systematist interested in small-scale questions still routinely read field journal information – typically photocopied and mailed to them by an MVZ curator – yet most queries relied on the database alone. As the flexibility and accessibility of the database increased, the expanding uses of the exogenous concept of space as the implied primary, and sometimes only, way to describe species locality data became entrenched.[29]

In 1997 a programmer analyst and his informatics team presented a new, relational data model for the MVZ collection. A new relational database was immediately put online, while its structure has continued to be developed ever since. This database defined not only multiple search attributes for each specimen record – such as who collected it, where and when was it collected – but also defined relations between these attributes, for example linking a specific collector, locality and time to a unique "collecting event" and thus allowing, for example, a search on all the specimens obtained from the event of Grinnell's field trip to Yosemite Valley on October 10, 1914. That kind of search is practical since a relational database allows flexible queries. Now multiple different attributes, hence multiple different queries – of even unanticipated kinds – can be linked to multiple different objects, rather than merely a single set of attributes linked to a single object as in preceding decades. The MVZ's database thus could answer many more kinds of questions. The new relational database was designed to be complete, i.e. contain records of all specimen tags alongside field journal entries, letters, photos, maps and more. Yet, however ambitious and carefully planned, the database's highly structured data model did not provide the tools required to incorporate the free-text records of the field note journals.

In 1998 the programmer analyst visited the University of Alaska Museum to establish a collaborative programming project with local museum members in order to develop a joint database system, open to the public, that would share all the applications to manage the data of the museum collections. "Arctos" was built to interface with collections users who could: (1) search the same live database the museum staff work on (rather than search weekly published updates from the collections database), (2) access online information originally extracted from the specimen tags, and (3) link to additional information stored in other online databases and services (e.g. GenBank, MorphBank, and BerkeleyMapper). At present, Arctos is the largest multi-institutional collaboration of natural history museum in an online database, with data from the museums of Harvard, Alaska, Washington State, New Mexico, and more.

Now that anyone with internet access could quickly and efficiently query the collection, many more did so, yet only queries about locality that assumed a regular grid with standardized meanings for each term, unequivocally (and automatically) assigned to a set of data fields defined by the data model, could be answered by Arctos. The "verbatim locality" records of the specimen tags, along with lat/long coordinates, fitted these requirements while the field journal descriptions did not.

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[29] I.e. viewed as necessary for producing valid data because so many kinds of analysis came to assume it. See Wimsatt (2007) on the concept of entrenchment.

A problem propagated into the database from these verbatim locality records, was that they referred to a relatively large geographic extent. To improve the resolution of these locality records in the database, the programmer analyst also developed a sophisticated georeferencing algorithm and protocol, which allowed one to assign a GIS map point with a maximum error distance (degree of uncertainty) to each historical descriptive "verbatim locality" in the collection (Wieczorek et al., 2004). Descriptive localities written on specimen tags and stored in paper catalog cards seemed finally to be comparable with current and future localities recorded by GPS lat/long methods. It was hoped that whatever uncertainty remained could be reduced by reading the field journals (by now scanned and posted online, but still not searchable), applying auxiliary information to the georeferencing procedure, and thus shrink the error distance around each point locality.

Natural history museums worldwide record localities via the MVZ's georeferencing protocol, for example thirty five museums currently share mammalian data this way (http://manisnet.org/GeorefGuide.html).[30] This widespread adoption of a technology indicates the current overwhelming entrenchment of one concept of space, typically considered a sufficient representation of a species locality in the field: a point on a GIS map, with an implied concept of accuracy such that the smaller its geographical extent, the more accurate/true to nature it is. Problems arose, however, when someone had to actually go back to a locality in the field by following these lat/long coordinates and then mark that point on a GIS map (see Shavit and Griesemer, 2009). This new fieldwork challenge did not, however, turn the concept of locality into a conceptual problem, but only meant more work for those diligent researchers who went the extra mile and interviewed old collectors or read old field notes. What MVZ staff sometimes now call "the problem with locality"[31] did not arise until "going back" became a pressing institutional problem, i.e. until the Grinnell Resurvey project went into the field, returning to Yosemite Valley, in the spring of 2003.

The late 1990s and early 2000s made new computer technologies available in the field. For measuring a locality, GPS receivers had become cheap enough to replace the heavier combination of map, compass, and altimeter. For recording locality information, Palm Pilots and laptops (with spreadsheet software) increasingly replaced handwritten field notebook journals. The new technologies produced mostly numbers and abbreviations instead of narrative free-text descriptions. Since the arrival of the new museum director, Craig Mortiz, in 2000, and the expansion of the Grinnell resurvey project in 2006, these new tools are being extensively used to: represent data in GIS maps, analyze the distribution data using new research methods, and address new research questions, e.g. detection analysis and niche modeling. Consequently, the protocols for recording "locality" in the MVZ collection and in the field are changing in important ways.

[30] Accessed July 18, 2008.

[31] Shavit, A., observation during weekly Grinnell Resurvey meetings between 2006 and 2008.

First, one must record in the field, as part of specific locality, new GPS data fields, e.g. precise longitude and latitude, datum, and device accuracy (http://mvz.berkeley.edu/Locality_Field_Recording_Notebooks.html).[32] A leading naturalist at the MVZ recalls that, in effect: "Lat/long coordinates are largely a recent introduction [to MVZ's practice of locality records] brought about by hand-held GPS."[33] This makes sense: without such GPS data-fields, using GIS mapping systems is unreliable, and without GIS maps computers are limited in power to represent and predict species distribution. However, the MVZ naturalist added: "...if a locality couldn't be located at a geographic scale sufficient to be usable by the scale of the GIS layer [representing the spatial distribution of variables such as temperature or elevation], then the model derived by the combination of those different data would likely be in error, the extent of which would not be known. Georeferenced localities can give a false sense of security, unless they are located at a scale appropriate to the other information with which they are associated."[34]

Second, trapping methods have been standardized so as to be independent of the particular species trapped. One must detect all species in the resurvey using the same number of traps, trap lines, trap-nights and the same trap-types[35] in order to maximize the representativeness of the recorded data of a particular locality and to render present results more easily comparable with Grinnell's and with future results. Finally, the journal must now include a new, standardized format: tables in a spreadsheet to represent the detectability effort rather than free-text, context-specific descriptions of trapping. Locality information that was, for Grinnell, sensitive to a given species in a particular time and place – how, where and when was it obtained or observed – was transformed into a set of tables and data fields, each with a standardized meaning and structure.

Moreover, trapping information previously integrated with species locality information – habitats across the trap line, local weather, and unusual behaviors of relevant species – is now separated from the field notes and must be mined in order to be incorporated into the MVZ database.[36] The result makes the distribution data collected today better fit for verifying a species' absence from a locality (by weighting the probability of species occupancy by the probability of detecting it, given the effort and methods used in the field), yet potentially rendering this data less useful

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[32] Accessed 7 July 2008. "Datum" is a technical term referring to the mathematical basis for delimiting latitude, longitude and elevation relative to a mathematical model of the Earth as an ellipsoid, rather than based on local, ground-based measurements that are affected by local gravity. The shift from a geoid to an ellipsoid model was required when geodesy became based on satellite rather than ground-based measurements and to provide a world standard. "The WGS 84 continues to provide a single, common, accessible 3-dimensional coordinate system for geospatial data collected from a broad spectrum of sources." (Department of Defense 2004, accessed 7 July 2008).

[33] Senior staff, September 3, 2008.

[34] Senior staff, September 3, 2008.

[35] Perrine, John (manuscript). "Data Fields to Capture for Grinnell Resurvey Project." A protocol draft discussed by all senior MVZ researchers and completed on May 5, 2007.

[36] Shavit, A., observation during weekly Grinnell Resurvey meeting, March 17, 2008.

for future queries that might be motivated by different research questions for testing different hypotheses. In order to compare the effort of detecting a species in the same locality across different times (and most likely across various ecological variables as well), each use of a different detection method constitutes a distinct collecting event in the database.[37] For example, if one puts out three traps of different types while walking a single trap line on the same day, one has conducted three different collecting events that day, and this conceptualization might not be very useful to a researcher who wants a species list rather than the responses of species to climate change. Despite problems it might create for the future, the increasing prevalence of data standardized in particular ways in current museum work led some MVZ researchers to record what they regarded as their most important data, if not all of it, in private spreadsheets – the analog of the old field notebook journal.[38] All of these researchers are well aware such data are very likely to become inaccessible after a few years, due not only to new research questions but also to more mundane issues of obsolete software, lack of metadata,[39] or deterioration or loss of media.

The net effect of these technology-induced changes in protocols and practices, which aimed to continue Grinnell's legacy and realize his vision, was that by re-visiting Grinnell's localities, the MVZ "foregrounded" a fundamental gap between two different concepts of space, one exogenous to the research subjects and human interest in them but readily coded in locality descriptions in the museum's database, the other sensitive to both subjects and humans, but hard to code and even less readily interoperable among the increasing number of private databases as well as the museum's. The MVZ's initial response was to eliminate the dualism by deepening and broadening the application of the exogenous concept of space to descriptions of species locality data. That is, most researchers came to believe that recording locality from a calibrated GPS reading, a short descriptive sentence and a standardized detectability table would be enough to allow future replication of their work a century from now.

Instead of Grinnell's comprehensive ideal, enacted by his dictum: "write <u>full</u> notes",[40] the re-survey protocol hesitates: "How observations are to be formally recorded and included in the MVZ database requires more thought. Likewise, their

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37 Shavit, A., observation during Grinnell resurvey meeting with programmers from Alaska and MVZ, April 23, 2008.

38 The primacy of the electronic spreadsheet over the handwritten notebook is one of the reasons why the Grinnell Resurvey project maintained three separate local databases during 2004–2007, none of them interoperable with the main MVZ database.

39 "Metadata" is information about information. That is, information about the nature and structure of the data, for example "author," "title" and "date" for finding a particular book in the library database. "Metadata" is familiar to the average reader from the information in the head section of an html-based web page, preceding the body of the page, between the tags <meta> and </meta>, which is used by search engines to catalog web pages.

40 Grinnell, Joseph, "Suggestions as to Collecting and Field note Taking," 6, April 20, 1938. MVZ archive, MVZ main gallery, top left cabinet, file name: Museum Methods – Historical (underscore in original).

value for the purposes of the Grinnell Resurvey also requires more thought."[41] Given the re-survey's extensive use of tables in private databases and spreadsheets, some MVZ scientists are now actively seeking to "define the notebook of the future."[42] The field journal – with its free-text descriptions focused on organisms' interactions with their environment – is not an integral part of the MVZ database, because of the formalized nature of a computerized database and also because fewer people actually write comprehensive field notes.[43]

This is both an old and a new perspective on Grinnell's vision: old in the sense that it aims to continue Grinnell's legacy of standardizing the work through protocols rigidly adhered to so that the data will be maximally useful in the future, new in the sense that to standardize the work in the computer age and make the work maximally useful in the future, it *juxtaposes* (now via nearby clickable buttons rather than nearby notebook shelves, catalog files, and specimen rooms) rather than *integrates* scanned field notes from each specimen record. From its origin in a series of small labels tied to specimens with unique specimen numbers to link to the field notes, the MVZ collection was built with the aim of standardizing the work in order to allow others easy access to facts from the field journal about a certain species' locality. Comprehensiveness of access has become as important as comprehensiveness of the collections.

Today, "easy access" means an automatic link between data coming from different systems, i.e. data interoperability. However, it is precisely the demand for interoperability between databases that brought researchers to mine data fields from the field journals, rather than simply read them. For someone not initially committed to the value of field notes to invest considerable effort in their utilization, this mining made it seem even less necessary for someone else in the future to go back and again read the original locality description in the field journal. The result of this data-mining process was the production of several local structured databases about the localities in Yosemite in 2003, and Lassen Volcanic National Park in 2005, which, in contrast with implicit initial expectations, were not interoperable with the main MVZ database. Why?

Because history matters: these local databases originated from field notebook descriptions while the data model for the MVZ database originated from small structured tags; each type of record was recorded at different stages of the field work, for different objectives, suggesting different data fields for recording locality data, different part/whole relations between data fields, leading to different, non-interoperable formats. Extracting information from field notes to code and record in local databases thus did *not* bring about interoperability, yet, it *did* further marginalize the concept of space embedded in the field notes since researchers in the wider

41 Perrine, J, "Data Fields to Capture for Grinnell Resurvey Project". A protocol draft commented by senior MVZ researchers and completed on May 5, 2007, 4.

42 Shavit, A., observation during weekly Grinnell Resurvey meeting, April 24, 2007.

43 Interviews or comments made by various MVZ personnel, March 24, 2007; March 8, 2008; May 15, 2008; April 10, 2009.

world of biodiversity research are likely to view the extracted information as "the most relevant", and, given their limited time, are likely to feel less compelled to invest time and effort in these original field notes.

The researchers seemed to be left with the worst of all possible worlds: a standardized, mechanically-produced record of locality that unfortunately is highly inaccurate and uncertain, while the more accurate, judgment-based, natural history description of locality in the field notes was still unreachable from the main database and decreasingly accessible as more researchers become accustomed to getting their answers after 1–2 minutes online. A leading programmer analyst at the MVZ explains that a mismatch in human expectations, rather than the database, should be blamed for this result,[44] yet those very expectations explain the effort of digitizing, databasing and putting online all of the MVZ's collections. Given the history of the MVZ's attempts to record and revisit its localities, the natural-history type of locality record could not easily be computer-coded, nor could it be done away with.[45] Ironically, the harder the MVZ staff tried to apply Grinnell's vision, the faster it seemed in some respects to fade away.[46]

Tension: Two Concepts for One Object

Throughout our story, two different concepts of space implicitly direct the fieldworker's attention while recording locality information. One concept represents the environment as an exogenous background for organisms. It directs scientists to record those geographical and environmental parameters whose nature cannot be affected by an organism or its population (e.g. latitude, longitude, and elevation). The other concept represents the environment as an endogenous, constitutive component of an organism's interactions that structure its locality. It directs scientists to record geographical and environmental parameters that are directly or indirectly affected by an organism or its population (e.g. vegetation associations, soil types, slope, and landscape). The two concepts are not mutually exclusive and both are applied in the MVZ's fieldwork to a single kind of object around which the entire MVZ database revolves: the individual specimen, whether killed, observed, or caught and released (with or without leaving behind a tissue or blood sample). The specimens and the information recorded about them – whether in narrative field

44 April 10, 2009, written comment on the manuscript.

45 Personal observation in MVZ field trips, during August and September 2007 and May 2008.

46 It is important to mention that not all MVZ personnel agree with our interpretation. A leading programmer analyst: "I definitely disagree with this statement. I'd like to think I have maintained Grinnell's vision in all that I've done, even if I might record data in ways different from what he did". April 10, 2009. What precisely it means to maintain Grinnell's vision is, of course, one important aspect of what our work aims to understand. To some extent, disagreements within the MVZ about what it means to maintain Grinnell's vision probably reflect generational differences among museum staff; changing disciplinary, institutional, and organizational structures and pressures; and differing exposure to technology and technology-induced conceptual change.

notes, specimen tags, or local or global computer databases – are the focal point of research, but also of tension regarding descriptions of locality because both concepts of space appear central to the MVZ's practices, yet they place conflicting demands on data collection, recording and accessing.

To take just one example, the "locality" table in the MVZ's Arctos database schema contains a data field for ID number, three data fields dedicated to elevation measurement, six data fields for TRS map data, a data field for "specific locality," an additional table of latitude and longitude coordinates with thirty five (!) data fields devoted to incorporating all the many different grid systems available for recording lat/longs from GPS or topographical maps, and a single data field named "locality remarks" where information can be entered as free-text descriptions not searchable in Arctos. Although a data field for "habitat" appears in Arctos – under "collecting event" rather than "locality" – this type of information is not searchable since there are multiple different standards for naming habitats – not all of them translatable – and most MVZ collectors do not use these standards for their journals but rather their own narrative descriptions. Some researchers list dominant plants, others use a single name such as "mixed canopy" or "oak forest" and others only "campsite." It is clear that the Arctos database schema reinforces the dominance of the exogenous concept of space via its specification of data fields that are coded and those that are not. In addition, the data recorded in "locality remarks" and "habitat" are not delimited by the questions defining the Grinnell resurvey, but are rather derived from traditional standards and protocols for fieldwork conducted by the MVZ.[47]

It seems Grinnell's initial distinction between two kinds of "locality" records (field note and specimen tag) has widened in the resurvey effort, as explicated in the current dual guidelines for recording "locality" in database data-entry versus in field notes. First, in the database guideline:

> The locality is the specific place associated with a specimen, document, or image. Localities refer to, but do not contain, higher geography (cf.) information. *A locality can be uniquely defined by geographic coordinates (latitude, longitude, and datum) with or without a descriptive specific locality* (http://mvz.berkeley.edu/Locality_Guidelines_Locality.html).[48]

Second, the protocol for writing locality in one's field note journal and catalog:

> Locality: Provide a descriptive locality, even if you have geographic coordinates. Write the description from specific to general, including a specific locality, offset(s) from a reference point, and administrative units such as county, state, and country. The locality should be as specific, succinct, unambiguous, complete, and accurate as possible, leaving no room for uncertainty in interpretation. Hint: The most specific localities are those

[47] Moritz, C. et al., "The Grinnell Project: Using a Unique Historical Record to Document Responses of Mammals and Birds to 100 years of Climate Change.," Grant number 0640859, submitted to NSF program PD-041128, on July 9, 2006.

[48] Accessed 7 July 7, 2008. "Higher geography" refers to features such as: "continent, ocean, country, state, province, county . . ."

described by a) a distance and heading along a path from a nearby and well-defined intersection, or b) two cardinal offset distances from a single nearby feature of small extent (http://mvz.berkeley.edu/Locality_Field_Recording.html).[49]

The resemblance to Grinnell's guidelines is clear in the latter and somewhat lacking in the former. Yet it seems that calling this a "conceptual gap" would be an exaggeration. If our argument about the contrast and difference between two concepts of space is not more than the above inventory of minute practical coding differences, it seems that our concerns may be "much ado about nothing."

However, if one agrees with Wittgenstein that "'to give a new concept' can only mean to introduce a new deployment of a concept, a new practice" (Wittgenstein, 1978, 432). then our claim for a basic conceptual gap between two concepts of space driving two kinds of practice and protocol for describing "locality," both today and in the past, is clearly supported. This conceptual gap emerged from a duality of practice – interactive, comprehensive and open-ended narration versus representative, fixed, and structured data-entry – that began in Grinnell's time, deepened upon computerizing the museum collection during the 1970s and further diverged via online databases and GPS technology in the last two decades. The expectation today that updating and uploading locality records will be nearly instantaneous (e.g. polished off on a Saturday morning),[50] creates demands for data recording, storage, and retrieval at odds with the comparatively leisurely pace that curators traditionally experienced. Seemingly small "technical" differences on how to record "locality" in a single recording event can stop the work altogether when implemented on a museum scale, the scale relevant for Grinnell (Griesemer and Gerson, 1993; Star and Griesemer, 1989).

Replicating Grinnell's localities in a rigorous manner *and* on a relatively small spatial scale, which took years of extensive effort, could not have been done without deploying both concepts of space and associated protocols for locality descriptions. Nonetheless, it was done well enough. After all the trapping and recording work of a successful re-visit has been done, a corrected, more precise, lat/long record was reached that could be entered into the database. It is at this final, seemingly "trivial" stage where the "small" and "technical" differences between exogenous and interactive concepts of space, could, and have, stopped the work.

For example, a senior naturalist planned to return to upper Lyell Canyon, which was originally worked primarily by Camp, Storer, Ferris, and Holliger in late July 1915. Their camp, near where the "footbridge" crosses Lyell Fork on the John Muir/Pacific Coast trails, was identified on modern topographic maps. The elevation at that point is about 9,700 ft. The slope, however, is very steep: a radius of 0.5–1 km from this site one would extend from 9,000 to 10,500 ft elevations, and from lodgepole pine-western hemlock forest to whitebark pine at tree line and

[49] Accessed July 7, 2008.

[50] Interview with MVZ curator, March 23, 2006. It is not the process of curating that is expected to be instantaneous but the upload of corrected data into the database, after a long process of correcting the georeferenced records has been completed.

above. The researchers who worked here during the original survey had trap lines extending from their camp at 9,700 ft up to Mt. Lyell and Donahue Pass at about 11,000 ft. When the survey was first georeferenced, all of these separate trap lines were recorded as "elevation 9,700–11,000 ft," an immense error radius. After returning from the resurvey fieldwork, and before conducting the data entry, a senior staff member read through the field notes and corrected all specimen localities along this gradient (to match the trap lines mapped in Camp's field notes, for example). As a consequence of this work, the error radius around each specimen and around the locality where it was obtained was reduced substantially. The new error radius calculated for the 9,700 ft point of their campsite was 162 m.[51] However, this new lat/long record was stored in the Yosemite *local* database, which was built in 2003 from reading the field journals, while the MVZ database was built in the 1970s from reading the museum catalog cards. Each database was built from different sources and was thus structured somewhat differently. As a result, none of the improved locality data stored in the Yosemite local database could be updated into the main MVZ database without *a lot* of additional technical work, most of it manual, similar to the curator's correction work based on the field notes. It took months to transfer the corrected locality of Upper Lyell Canyon to the MVZ database. The senior naturalist stopped his correction work for a while, and only after several discussions and manpower added to the GIS lab was this task finally completed.

Given the many thousands of locality records in need of similar improvements at the MVZ, this lack of interoperability between locality data emerging from incompatible data formats and locality descriptions driven by different concepts of space might become an intolerable problem.[52] Given the millions of NSF dollars already invested in the MVZ's database and the rigorous information it can (and does) deliver, given the prominent role of this particular research museum in setting the recording standard for others, and given the billions of dollars and euros invested – and planned for future investment – in similar online databases of biodiversity world-wide, this lack of interoperability presents a major challenge (Bowker, 2005; National Research Council, 1995).

Resolution: Workable Alternation Rather than Universal Interoperability

In the last two decades, complaints of lack of data interoperability became an everyday part of almost every biodiversity survey involving replication in space and/or time (National Research Council, 1995). We have argued so far that the history of the MVZ's application of two concepts of space to locality descriptions can explain, in part, how and why this lack of interoperability emerged. This is one reason why history can be useful for biologists: small historical contingencies brought about

[51] Interview with senior staff, June 9, 2008.

[52] Shavit, A., observation during weekly Grinnell Resurvey meeting, March 6, 2007.

this conceptual gap, and it was the biologists themselves who uncovered the tension between concepts and differences between fieldwork and database practices through careful study and reflection on their own historical records and documents. In light of the MVZ scientists' attention to history, which "foregrounded" the problem of locality, in this section we discuss how their continued attention to history is resolving the problem created by the conceptual gap that their practices opened up. We argue that resolution involves noticing and "minding" the gap and thus "bridging" it rather than closing it, by a practice of "workable alternation."[53]

An institutional response to the locality-interoperability challenge surfaced in the MVZ resurvey project around 2006. At that time, the MVZ director, the curator of birds (and lead PI for the digitizing MVZ grant), the bioinformatics programmers, and the georeferencing manager all came to see that the way to "connect" the different locality records and make them less vague would *not* be to rewrite them all as various kinds of database records with GPS measurements of ever-smaller spatial extent. On the contrary, amplification of this type of locality description only made the interoperability problem worse and, for most species studied, only increased the confusion over what locality records are records *of*. Instead of unifying all locality records under a single concept of space – which might be endorsed as consistent with the goal of universality – the MVZ resurvey team returned to Grinnell's vision and determined to implement it even more forcefully: "These field notes and photographs are filed so as to be as readily accessible to the student in the museum as are the specimens themselves (Grinnell, 1910 [1943, 34])."[54]

Since 2003, a large portion of the field notes and photographs have been digitized and posted online, yet posting did not make this information "readily accessible" in the sense one expects of queries to relational databases because the posted notes were not linked with particular specimens. For Grinnell, it was the collector's field notebook number written in the upper left corner of the specimen tag that allowed any other researcher to trace back a specific specimen to its place in the field notes. The GReF (Graphical Referencing Framework) project is currently devoting an immense effort to link every specimen in the collection with the journal field note page(s) on which it is described.[55] Trained undergraduate students carefully read the online field notes and whenever they come upon a specimen number, a date or a

[53] We use the metaphor of "bridging" to indicate the gap is not closed, but these may be fragile bridges. We base our notion of workable alternation on the important thesis of Kiester and White (in preparation) that "... a structured relationship between two alternative concepts of space provides the most comprehensive and accurate assessment of the distribution of biodiversity and its geographical patterns of policy requirements." Kiester and White invoke Wilhelm Windelband's nineteenth century distinction of "nomothetic" and "idiographic" purposes of scientific investigation in order to contrast different concepts and modes of representation of space that must alternate at different spatial scales in empirically sufficient ecological and geographic studies of biodiversity.

[54] In this sense, we agree with the programmer analyst (see footnote 46), that the technical database work of the resurvey has been conducted in keeping with Grinnell's vision.

[55] For a description of the Digital MVZ projects, see: http://mvz.berkeley.edu/Digital_MVZ_Project.html (accessed 7 July 2008). On GReF, see http://code.google.com/p/gref-mvz/wiki/UserGuide (accessed 7 July 2008).

location, they tag it electronically. Later, a link is made to every place in the database where this number, date or locality is mentioned. The result is not full interoperability, since one does not receive a machine-produced answer to one's query. However workable alternation is achieved between the systems in the sense that one can click on a link from a single specimen page and reach a page in the journal narrating how it was collected. The researcher can thus work quickly back and forth – alternate – between the two kinds of information, posing structured queries in one and reading free-text descriptions for answers to questions "the old fashioned way" in another.

Since both types of locality description are necessary for completing the resurvey and their conceptual differences are expressed through differing protocols and methods for data recording and reading, one can and must alternate between them while recording and using specimen information. The insight of 2006 did not invent workable alternation – Grinnell and his colleagues had been doing it since beginning their collections – but it did exploit computer technology to speed it up and make it widely accessible online. And since a fast alternation procedure is now at hand and working, a satisfactory resolution has been found. The "basic problem with the concept of locality" was resolved well enough for now.

This resolution has *not* meant, however, that traditional epistemic problems of theoretical representation aimed at explaining nature, in contrast to controlling it or ameliorating its effects, have been pushed into the background. While our case study supported the claim that "science has undergone a profound methodological and institutional transformation during the past decades (http://www.uni-bielefeld.de/(en)/ZIF/FG/2006Application/index2.html, accessed June 23, 2008)," we argue that scientists in our case study are engaged in alternating between different kinds of locality recording practices in order to accomplish *articulated* goals of theoretical explanation and application. Our case study supported the claim that the on-going transformation of science is at least partly technological, but also the claim that technology has brought conceptual, epistemic concerns to the *fore* because of the particular way technology and historical concerns interact. Thus, we locate fundamental change *within* the context of our single, albeit historically extended, case study rather than place our case study on one side or the other of a philosophical divide. We argue that shifting-back-and-forth or alternation of applicable concepts of space and protocols for locality description is a practice that emerged to *resolve* challenges arising in the work. We do not see a secular shift over the twentieth century from theoretical projects and programs to technology-driven applied research, but rather theoretical research that moves into a world transformed by GPS-technology and thus science taking place within a changed context of application.

In general terms, we track a phenomenon which is the reverse of one well-documented by Hans-Jörg Rheinberger (1997). Instead of a reconfiguration of scientific or "epistemic" things as "technical" things in the course of laboratory investigation, we track a movement from technical things back to problematic epistemic things – the problematizing of established technical categories as distinctive new scientific problems. In our case study, the technical category of specimen locality, long-established in field and museum protocols instituted by Grinnell and reinforced by subsequent directors and curators, became problematic in the face of theoretical and technological change as well as changing public priorities for science.

The story we tell is *not* a simple transformation from the theory-driven goal of understanding species distributions to an application-driven goal of utility and control in the face of climate change and growing human population. Grinnell and his successors shared a vision of universally useful information contained in a natural history museum and in that sense, the successors have held true to Grinnell's remarkable institutional legacy and initiated the Yosemite resurvey as part and parcel of that legacy. But the resurvey participants also brought new perspectives to bear, due in part to the transformations of ecological science in the intervening years, in part to changing technologies – especially the introduction of digital computers, relational databases, global positioning satellites and receivers, and GIS maps, and in part to changing social and political interests and pressures that placed a premium on rapid access to data on species distributions. These scientific, technological, and political changes led to tensions when new methods and protocols were brought to bear on ostensibly Grinnellian projects, which we explored here through the lens of different descriptions of specimen locality. These changes however did not lead to a replacement of one set of practices by another, but rather to a more complex articulation of Grinnell's concepts and practices with new ones derived from the transformation of ecology, technology and society.

Conclusion

The gaps between two concepts of space and kinds of "locality" descriptions seem at first to prevent one from revisiting a locality in the field based on one's locality records, but we conclude that the MVZ scientists have done just that while acknowledging the basic ambiguity at the heart of their term "locality." The Yosemite resurvey worked in a practical sense, and, following Wittgenstein, this is all that it means to step into the same river twice:

> What we do is to bring words back from their metaphysical to their correct use in language. (The man who said that one cannot step into the same river twice said something wrong; once *can* step into the same river twice.) And this is what the solution to all philosophical difficulties looks like. Our answers, if they are correct, must be homespun and ordinary (Wittgenstein, 1993, 167, italics in original).[56]

We hope our discussion of "locality" (and thus "survey replication"), while exposing its complexities via its different and changing contexts of application, also brought this word back to the ordinary. A concept that shapes so many aspects of a practice, as "locality" does for the study of species distribution (and biodiversity at

[56] We have greatly benefited from a clear discussion on this remark by Wittgenstein in Ben Menahem (2006).

large), *cannot* present "deep" or "underlying" problems while the researchers compelled by these practices somehow view "the problem" from the outside and correct it. It simply makes no sense to describe the work at the MVZ this way. The fact that "locality" descriptions assume a variety of meanings resting on different concepts of space does not necessarily mean they present a deep conceptual problem, since each meaning can be uniquely applied in a specific, well understood context, and one can, in the right circumstances, with suitable goals, freely alternate between these different contexts. In the field, researchers alternate their collecting effort across the "trap" and "trap line" scales while behind their desks they alternate between "data" and "narrative" (see Shavit and Griesemer, 2009). It is because "locality" is such a basic or constitutive concept in biodiversity research and its multiplicity of meanings was so entrenched in the MVZ tradition, that no one seems to have found the gap compelling until computerized databases, GPS machinery and GIS maps all required a single "locality" on the same small scale, thus making the multiplicity of "locality" problematic. After the problem was embraced, the MVZ scientists explicitly addressed it through conceptual and historical analysis.[57]

We have argued that minute technicalities arising in the history of empirical practice in the MVZ entrenched a conceptual gap in the meaning of "locality," inducing a practical problem for analyzing contemporary biodiversity data. A methodology of alternating between the two concepts of space and thus of kinds of description of locality in investigations by scientists brought these theoretical tensions to light, and led to their practical resolution – though not a general solution – that satisfied the biologists and their community (Moritz et al., 2008). In this case at least, historical analysis served science as a practical tool for those who would tackle theoretical challenges in technically driven, historically rich research fields.

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[57] On the concept of entrenchment see Wimsatt (2007, Chapter 7).

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Part III
Changing Conditions of Scientific Research: Institutional Changes in Applied Research

Protected Spaces of Science: Their Emergence and Further Evolution in a Changing World

Arie Rip

Introduction

Most often, discussions of ongoing changes in science in society are framed, by actors as well as analysts, in terms of science-as-we-know-it. In fact, the reference is often to science as-we-*knew*-it, to a Golden Age when things were better. Indicative is how US President Obama's phrase, in his inaugural address in January 2009, about "restoring science to its rightful place", was taken up by scientific establishments. The phrase was meant to contrast with the Bush Administration's politicization of science,[1] but spokespersons for science picked it up and interpreted it as "more money, more freedom for science". This shows the deeply engrained "entitlement" attitude of scientists, where the structural dependence of science on sponsors is backgrounded, and turned into a "right."

The origin of this "entitlement" attitude can be traced back to the 1870s, with the various "endowment of science" movements in the UK, France and Germany. In other words, it is historically contingent and its force derives from the eventual institutionalization of certain sponsorship constellations, not from characteristics of science as such. Having seen this, one starts to wonder whether there can be something like "science as such", somehow given, independent of history. There are enduring achievements, but science, as we know it now, is also the convergence (i.e. inclusion and exclusion) over time of different activities, their institutionalization at particular times and places, and their further co-evolution.

This is not a message of relativism. What has (co-)evolved over time has value, and there are important issues at stake in the present discussions. What I want to problematize is the simplistic reification of science as something given, somehow, which can then also be referred to as a standard, as what is "proper"

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[1]Cf. the March 9, 2009, Memorandum to Heads of Agencies, on scientific integrity. http://www.whitehouse.gov/the_press_office/Memorandum-for-the-Heads-of-Executive-Departments-and-Agencies-3-9-09/ accessed 17 March 2009.

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science. Of course, achievements must be recognized and cherished, and when threatened, defended and hopefully "restored." But one has to consider possible further evolutions, and their value, also if this does not conform to what is now considered "proper" by scientific establishments. Standards for evaluation cannot be specified beforehand, but co-evolve with practices and institutionalizations. Still, there is a continuing thread, the goal and practices of robust knowledge production (in context). I will come back to this in the next section, and build on it to offer my diagnosis.

To do so, I have to clear away pre-conceptions about science and its dynamics. Science is not just a way (perhaps the main way) of producing robust knowledge, it is also part of a master narrative of progress, and has become an icon of modernity. And it has become linked to nation states, which sponsor scientific research, and shape its "rightful place". This is what the Bush Administration did (even if one may not be happy with it) and what the present Obama Administration does. What is done at the laboratory bench (and increasingly, in the computer), is not independent of these larger developments, even if the scientists, in their protected spaces in the lab, do not feel the impacts directly. I will develop this point by showing the importance of protected spaces, not just at the micro-level of the laboratory, but also at the macro-level of a "rightful place" for science in society, and at the meso-level of scientific communities and institutions of the science system.

The epistemic and institutional aspects are entangled, at the micro-, meso- and macro-levels. This is already visible in how Kuhn (1970), in his Postscript, emphasizes that the (epistemic) paradigm and the relevant scientific community are two sides of the same coin. A further point was introduced by Campbell (1979): in such scientific communities there are "tribal norms" (like struggle for visibility) which may not have an immediate epistemic value, but support the life of the community, and are thus important for knowledge production, and shape it. One can see the epistemic and the institutional as two different dynamics which impinge on each other, and may, or may not, support each other. In fact, they are integral to each other.

This perspective implies a criticism of much of philosophy of science: while the importance of social and institutional aspects is increasingly recognized, it is taken as a context, and thus external to the core, epistemic business of science, rather than an integral part of epistemic practices. The sociology of science should be criticized as well, however, for its neglect, or at least black-boxing, of the epistemic business of science. There appears to be a division of intellectual labour here. Philosophy of science looks at what is happening within the protected places at the micro-level and at the meso-level of disciplines, and forgets to ask about the nature and effects of the protection. The Mertonian sociology of science (Merton, 1973) stays outside, while laboratory studies immerse themselves within it and forget about the outside (as in Latour and Woolgar, 1979, where the specifics of biomedical science in the USA in the 1970s are not discussed).

This is a bit of a caricature, because there is lots of interesting work done that transcends these strong reductions of complexity (and I can build on such work for my analysis). But the caricature does indicate that I have to battle on two fronts: integrate the institutional in the epistemic focus of philosophers, and integrate the epistemic in the institutional focus of sociologists.

Many of the current diagnoses of changes in science and its interactions with society focus on institutional aspects, as in the idea of university, government and industry overlapping and co-evolving as in a Triple Helix (Etzkowitz and Leydesdorff, 2000).[2] Closer to my call for an integrated socio-epistemic approach is the diagnosis of wide-ranging changes in modes of knowledge production put forward by Gibbons et al. (1994) and Nowotny et al. (2001).

Gibbons et al. (1994) contrast an earlier Mode 1 (university-based and disciplinary oriented) with a presently emerging Mode 2, which is transdisciplinary, fluid, has a variety of sites of knowledge production including "discovery in the context of application" (e.g. in industry) and new forms of quality control. The separate features they describe are clearly visible, but one might want to question their overall thesis that these add up to a new mode of knowledge production, comparable in its internal and external alignments and eventual stabilization to Mode 1 (Rip, 2000a).

More important for my analysis and eventual diagnosis is the recognition that their Mode 1 is historically located. Its building blocks emerged during the nineteenth century, and these became aligned, and locked-in after 1870 (as I will discuss later). However, there was science, or at least robust knowledge production, before the nineteenth century. If one wants to specify encompassing modes of knowledge production, one could say there must have been a Mode 0 of knowledge production. There might not have been a specific mode of knowledge production, though, rather overlapping varieties of knowledge production, as in the "melting pot" of the Renaissance in Europe.

I will address these issues in the next sections in terms of identifiable contextual transformations which are followed by stretches of more or less incremental development. A basic question, important for the diagnosis of our present situation, is visible already. How could a Mode 1 emerge at all and get a hold on the variety of knowledge production and institutions? The key "mechanism" I propose is a lock-in of dynamics at three levels: ongoing search practices and knowledge production "on location", more cosmopolitan interactions of scientists (and practitioners more generally) and the institutional infrastructures to do so, and legitimation of science and its role in society. Such a lock-in creates nested protected spaces for doing science, and in a particular way – in the case of Mode 1, the combination of relative autonomy and disciplinary authority –, at the price of accepting the constraints that go with such protection. One such constraint is the hold disciplines have obtained on the production of scientific knowledge. Another constraint derives from the norms and values dominant in the regime of Science, The Endless Frontier, visible already from the late nineteenth century onwards, but coming into its own after the second world war (Bush, 1945). The entitlement attitude identified in the opening paragraph is part of this regime.

Clearly, we need a long-term perspective to offer an adequate diagnosis of ongoing changes in science in society.

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[2] See Hessels and Van Lente (2008) for an overview and for a discussion of the reception of the Gibbons et al. (1994) claim about a new mode of knowledge production.

Long-Term Dynamics of Institutionalized Knowledge Production

In a long-term view, one must be careful in speaking of "science" because it is only from the early nineteenth century onward that an easy reference to science is possible. True, the word "science" was used before, but it was only one of a range of terms, including "natural philosophy".[3] Still, one needs some guideline as to what to include in the analysis of developments. To indicate continuities, or at least lineage, one might still speak of "scientific" knowledge production, but using quotes as a reminder that the term science refers to eventual institutionalizations, and may not have been used at the time.

To cover the variety of modes of knowledge production, a broad description is necessary. I will just state the key elements, but they can be argued in more detail (cf. Rip, 2002b). Given the dominance of science as presently institutionalized, some of my formulations could be read as polemics with the strong claims about science as the exclusive road to valid knowledge.

Knowledge that claims some validity, scientific or otherwise, is a precarious outcome of efforts to make knowledge applicable at other places and other times – so that one can learn from one place and time to another, and act on that knowledge with some confidence. When knowledge production becomes professionalized, such "acting" includes its use in further knowledge production.

The transformation of local experiences to findings with a cosmopolitan status is an essential ingredient of the "scientific" mode of knowledge production: it is the (precarious) basis of scientific claims of universal validity. Such transformations are not limited to the specific mode of knowledge production of modern western science, however. Professional knowledges are one example, and craft knowledge and folk knowledge can also work towards cosmopolitan status.

The claim of the applicability elsewhere and elsewhen of the knowledge produced raises two general questions. One is how robust results are produced on location. To get nature to work for us, and on our terms, whether in scientific experiments, industrial production, or agricultural and health practices, we have to shape it, and use whatever comes to hand. Already in the creation of a laboratory and in the set-up of experiments, local and craft knowledge are important, and thus form an integral, albeit neglected, part of scientific knowledge production.

The other is how cosmopolitan knowledge can be translated back to concrete situations (and how to operationalize the notion of validity). The movement for evidence-based medicine offers an interesting case, showing the ambivalences, because it transcended and improved upon local, experience-based knowledge, but has now "overshot the mark" and "excludes too much of the knowledge and practice that can be harvested from experience (. . .) reflected upon"(Berwick, 2005).

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[3] Indicative is that the word "scientist" was coined by Whewell in 1833 "to designate collectively those who studied material nature." Morrell and Thackray (1981, 20) locate this as a response to Coleridge's challenge to the 1833 Cambridge Meeting of the British Association, that the members should not call themselves philosophers. Ross (1962) gives the story of the word.

Institutionalization of knowledge production implies the emergence of tried and trusted ways of producing knowledge that can claim to be valid. A phrase like "disciplined enquiry" captures this (Kogan, 2005, 19), but also indicates the ambivalence involved, when institutionalized disciplines start to discipline ongoing practices of knowledge production. Already within science as-we-know-it there is variety, in particular between more experimental approaches and more "natural history" approaches. The unity of science is primarily institutional.

This outline of a philosophy and sociology of knowledge that claims validity is the backbone of my analysis of developments in knowledge production and its institutionalizations. Taking a bird's eye view, one can identify major changes as well as periods of relative continuity. Mendelsohn's diagnosis of three main transformations remains relevant (Mendelsohn, 1969), and I will follow his lead but speak of contextual transformations to do justice to the entanglement of the epistemic and the institutional. Later research has corroborated the diagnosis of a "positive" transformation in the second half of seventeenth century (see especially Van den Daele, 1977a and 1977b) and a "professional" transformation in the course of the nineteenth century (which leads to the lock-in of Mode 1). In the late twentieth century, the earlier regime opens up. New closures that emerge might add up to a third transformation, but it is unclear what it might consists of.[4]

The diagram below (Fig. 1) offers a (selective) overview of long-term socio-epistemic developments. In the diagram, I use the notion of a "social contract" between science and society to identify a key element in the transformations and their stabilization, even if it is not a formal contract, and the partners of the contract are ill-defined.[5]

In the next three sections I will zoom in on some parts of the socio-epistemic history which are relevant to my search for a diagnosis that includes a long-term perspective. Here, I note five features of the overall history which are always relevant, even when not foregrounded.

First, the ever-present messiness and heterogeneity (socially and epistemically), which is more visible in natural history than in laboratory-experimental approaches.

Second, the movement from local to cosmopolitan, and back again, where social/institutional and epistemic features are two sides of the same coin. This was visible already in my broad description, above, and can be developed further.[6]

[4]At the time (Rip, 1988), I spoke of a political transformation, but that was programmatic. By now, there are some indications, if one takes "political" to mean the increased and explicit interaction of society with science.

[5]The notion of a social contract between science and society has been used before, particularly in the USA, and offers a way to diagnose what is happening now as the breakdown of an earlier social contract, and then identify elements of a new social contract (Guston and Kenniston, 1994).

[6]As I have shown (Rip, 1982, 1997), going from the local to the cosmopolitan is an epistemic and a social (institutional/political) movement. It involves circulation (among localities, possibly guided by cosmopolitan rules), aggregation (forums, intermediary actors), and an infrastructure for circulation and aggregation. Generally valid knowledge can only be achieved when there is a functioning cosmopolitan level. The nature of the knowledge produced will then be shaped by the affordances present at the cosmopolitan level (cf. Campbell's (1979) point about tribal norms).

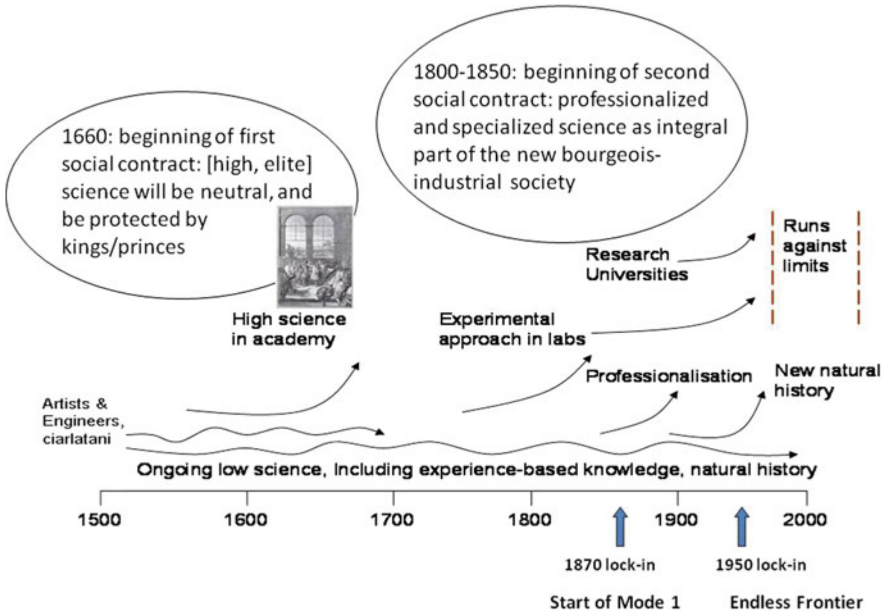

Fig. 1 Long-term developments of "scientific" knowledge production

Third, the key role of sponsors in enabling knowledge production and shaping its institutionalization. Different forms of patronage occur over the centuries, and include present science policy and university-industry interactions.

Fourth, the emergence of partial and sometimes hegemonic regimes, where macro and micro are aligned. Often, through intermediate (or meso) level organizarions or institutions, from the scientific societies in the seventeenth and eighteenth century to the research funding organizations of the twentieth century.

Fifth, the creation (and increasing importance) of protected spaces. At the macro-level, protection by kings (as in the 1660s in Britain and France) and later by nation states. At the micro-level, laboratories and controlled experiments. At the meso-level, intermediary structures like the funding-agency world after the second world war.

Nested protected spaces are the distinguishing characteristic of knowledge production in science-as-we-know-it. Protected spaces have material, socio-cultural, and institutional aspects. This is clear in the notion of a laboratory as a place where experiments can be conducted under restricted conditions: these conditions include the disciplining of its inhabitants and the exclusion of unwanted visitors. Field sciences have more difficulties in creating the desired protection, but attempt to create their boundaries as well, especially when aspiring to be part of high science.

The effect of protected spaces is the reduction of interference and of variety. In other words, productivity of scientific knowledge production is based on exclusion. This holds for laboratories (and their equivalents) and for disciplinary scientific communities which guard their status by excluding those who are not qualified. And for professionalized, authoritative science (since late nineteenth century), which excludes other loci and modes of knowledge production as non-scientific.

Thus, there is an essential tension: the productivity of scientific knowledge production is based on exclusion, and this may reduce unruliness and innovation. In an earlier attempt to address and diagnose socio-epistemic changes, I noted:

> (. . .) the recurrent and unavoidable dilemma between – on the one hand – the need for some order, and the reduction of variety that goes with it to be productive in what one does (here, search for knowledge), and – on the other hand – the need to go against that same order to innovate, or just to respond to changing circumstances. For science, and its institutionalized interest in producing novelties (up to priority races and conflicts), the dilemma is an essential tension. [As Kuhn (1977) phrased it and Polanyi (1963) experienced it.] (Rip, 2002b , 101).[7]

The dilemma cannot be resolved, but it is made tractable in practice. Protected spaces which enable as well as constrain make it tractable. Their existence has become a functional requirement for doing science, but the specific ways in which these spaces enable and constrain can have limitations, or may even be counterproductive. Also, there are pressures from without as well as from within on existing protected spaces: to change, to become porous, perhaps to be abolished. A diagnosis should be based on an analysis of long-term developments, so as to understand the nature and functionality of the protected spaces. This part of the diagnosis then leads to further questions: are present protected spaces opening up? are new kinds of protected spaces emerging? I will address these further questions (albeit selectively) in the last sections of this chapter.

The Melting Pot of the Renaissance and Partial Closures

For a birthplace of Western science as-we-know-it, fourteenth–sixteenth century Renaissance Europe looked messy, unruly, and without clear boundaries between various knowledges. There were the (medieval) universities. There were travelling humanists, artists and engineers. There were also almanac makers, astrologers, mountebanks and *ciarlatani* performing tricks at the fairs. Princes and wealthy persons were sought as sponsors. The scholarly and craftwork to be done was defined in terms of the wishes and aspirations of sponsors, as well as for the market place.[8]

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[7] I also offered a diagnosis: "Science, in its interest in searching for knowledge and trying to make its products robust, can be contrasted with science as an authority, which often relies on traditional ways of knowledge production and disciplinary controls of quality. If authority as such, disciplinary or otherwise, rules, science becomes its own worst enemy."

[8] One intriguing variety of knowledge production was through so-called "professors of secrets". They collected recipes from different crafts and some of their own experience, and sold them on the fairs or to sponsors. The ambivalence in their position is curiously similar to that of biotechnologists and other scientists in commercially important areas. They had to advertise themselves and their knowledge in order to create some visibility. However, at the same time they had to keep

The variety of knowledge production visible in the Renaissance became partially contained. Micro-protected spaces for experiments were introduced by Boyle and others: somewhat controlled conditions, and oriented towards demonstration. This was combined with macro-protected spaces (privileged by a King) where “deviant” approaches were excluded. At the same time, natural-history approaches to knowledge production continued – valued by sponsors because it allowed exploitation of what is “out there”.

The so-called scientific revolution of the seventeenth century replaced unruliness with proper procedure (in scientific academies) and started to create boundaries between mechanical philosophy and the crafts (Van den Daele, 1977a, 1977b). While this was just one part of the developments, the distinction between “high science” and “low science” (as I have called it, referring to a similar distinction between Anglican high church and low church) would continue, up to the eventual dominance of physics in the pecking-order of disciplines. Whether one considers this development as an achievement or as de-humanisation (Toulmin, 1990), the rationalistic mode of knowledge production which eventually emerged had grown out of the fertile soil of the Renaissance. The richness, variety and openness of knowledge production at the time were important for the scientific revolution. And I add, it remained, and remains, important as a backdrop to high science, and as a source of renewal.

Within this overall shift, sponsors in interaction with scholars and artists played an important role, and this is also how a key institution of modern science, peer review, emerged. In Renaissance Europe, immediate and bilateral patron-client relationships developed into triangular relationships, in which the patron needed advice about his sponsorship of a painting, a sculpture, or an engineering work, from a knowledgeable third party – in particular, humanist and other Renaissance scholars, who might on other occasions profit from patronage themselves. This circulation enabled the emergence of a community of what we now call “peers”, and the practice of “peer review” – which remains, essentially, advice to a sponsor, i.e. a journal editor/publisher or a research funding agency (Rip, 1985).

In the case of Galileo at the court of the Medici in Florence, this is visible, and further patterns can be seen emerging that eventually became a fact of “scientific” life. As Biagioli (1993) showed in detail, Galileo was first of all a courtier who offered his work to his patron, and looked carefully after his “local net”, but he was also active in building a “cosmopolitan net” with his competing colleagues at other courts – the competition focused on who could offer the more interesting things to their respective patrons –, and distancing himself from other, low-brow clients of his patron.

Cosmopolitan interactions, while deriving from, or at least coupled to, local contexts and interests, stimulated the emergence of virtual communities, linked through circulating texts and their contents. The influence of patronage games continued in

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their secrets in order to maintain a competitive advantage over other such “professors” operating on the same market or for the same sponsors (Eamon, 1985).

a more global way, as when institutional etiquette was enforced. The need to appear courteous pushed the struggles among practitioners below the surface that was presented to the outside (cf. Shapin, 1994). To coin a phrase: Scientists are tradesmen rather than gentlemen, but need to behave, and seen to behave, nicely to keep up legitimation.[9]

From the late seventeenth century onwards, the emergence of scholarly journals in the Republic of Letters helped to support "cosmopolitan nets". The scientific societies of the eighteenth century could publish reports of research, and might channel support from patrons to their members. The Enlightenment movement (in its various instantiations in different countries) allowed for overall legitimation of scientific knowledge production, independent of the support by specific patrons. At the same time, specific practices, e.g. of mining and metallurgy, or medical preparations, or meteorological data collection, were developing general insights, and thus added a cosmopolitan level as well. This could link up with general theorizing, as in the case of chemistry, and thus create proto-disciplinary communities (Hufbauer, 1982).

Professionalisation of Science in Bourgeois-Industrial Society

While the history of the emergence of disciplines and specialties starts in the late eighteenth century they become a serious business with professionalization of science and the revitalisation of higher education in the second half of the nineteenth century. By the late nineteenth century, disciplines were becoming dominant institutional categories, sedimented and codified in university departments and library categories. This is the institutional infrastructure for recognized specialties to emerge, with their own paradigm, cognitive style, and ideals of explanation.

Part of the work in research practices then becomes to transform the local production of knowledge items into more cosmopolitan knowledge claims – as in a scientific paper. Such claims are addressed to non-local audiences as constituted by a research area. These audiences and areas can be hybrid, as was (and is) the case in many sub-areas of chemistry (Rip, 1997). Research areas, specialties and disciplines offer spaces for cosmopolitan scientific work – a protected space at the meso-level.

Scientific work became sufficiently independent to relate to, and profit from, distributed sponsorship: from scholarly societies, various patrons, the state (in particular in France and in the German states) and professional practices (as in the UK). The 1870s mark a further change. Spokespersons for "science" felt sufficiently secure to claim that "science" should be "endowed" by the nation state (MacLeod, 1972). The state responded and became a general sponsor. In parallel, universities started taking up research and scholarship in earnest.

The increased role of the nation state strengthened the idea of a national community of scientists, located primarily at universities. While there had been self-styled

[9] This difference between public presentation of science and actual interactions inside the world of science continues, cf. Gilbert and Mulkay (1984) on contingent and rational repertoires.

spokespersons for science before, there now emerged a scientific establishment with institutionalized channels for lobbying and advice. This partial lock-in became complete when government funding agencies for science took off after the second world war: the agencies were captured by the national scientific communities, legitimated by the ideology of "Science, The Endless Frontier", which could now dispense resources (Rip, 1994). In a phrase: scientists divided the spoils (while voicing concerns about insufficient funding). Funding agencies became the bastion of disciplines, although with occasional, and now increasing, guilt feelings about multi- and interdisciplinary work, and attempts to respond to new developments. The authority of disciplines thus derives from the combination of their ordering of knowledge production, and their role as sponsoring categories in national research systems.

Sponsors and Spaces

This history of the emergence of Mode 1 shows how sponsorship of science is an integral element. A closer look at the variety of sponsorship relations actually indicates that there was always more to science than the regime of Mode 1. This allows me to introduce a further aspect of the dynamics of the development of science, which became important in the late twentieth century.

Since the late nineteenth century, local and state governments and industrial firms have used research and researchers for particular services, employing them or contracting them. An element of sponsorship was added because of the expectation of general value of the findings (so no detailed specifications of the work) and because the researchers were allowed to further their own reputation and career. This worked out differently in different scientific fields. In chemistry, from the late nineteenth century onward, a productive practice developed of interactions with industry and other sponsors, including a workable etiquette, particularly since the interbellum.[10] In fact, this allowed chemists to accommodate the new challenge of biotechnology in the 1980s and 1990s.[11]

The big charitable foundations, first established in the early twentieth century, are the nearest equivalent to the earlier patrons of science who could, and would, act according to their own discretion. The Rockefeller Foundation, based in the USA, had a generalized interest in natural and social science, linked to its concern about

[10]The wishes of customers and sponsors were internalized in the field, that is, need not be present as such to have an influence. The functionalities the sponsors were interested in would be realized through the heuristics that made up the paradigm or the regime (Slack, 1972; Van den Belt and Rip, 1987). This way of formulating the point resembles the finalisation and functionalization thesis of the Starnberg group (Schäfer, 1983), but does not depend on their overall (and physicalist) diagnosis of the development of Western science.

[11]Biologists, on the other hand, had no such history of interaction with industry (their practical relations with sponsors were in medical and agricultural sectors), so the advent of biotechnology created transitional problems, with conflicting etiquettes and complaints of naiveté (Rip and van Steijn, 1985).

the future of urban-industrial society. It has stimulated new developments in biology (including work that paved the way for molecular biology), anthropology and social science from the 1930s until at least the 1960s. Being funded by the Rockefeller Foundation added to the reputation of the researcher and the research institution.

In addition to such concrete sponsors, one can see the emergence of abstract sponsors, starting with the idea (or ideograph, cf. Rip, 1997) of SCIENCE as progress through the advancement of knowledge. Reference to this abstract sponsor supports concrete resource mobilisation efforts, especially with the state and with science funding agencies, and is thus an indirect source of resources. The nation state, a concrete sponsor from the 1870s onwards, also became an abstract sponsor when scientists started to refer to their duty to the NATION, which would shape directions of their research (in return for support).[12]

The emergence of a further ideograph, INDUSTRY, is very visible in chemistry. In addition to Bayer, Hoechst, ICI or Dupont contracting for specific types of research, it was toward the chemical sector in general (and also the medical and pharmaceutical sectors) that chemical research and researchers would be oriented, explicitly or implicitly. Since reference to the importance of industry helped to mobilize resources, the ideograph INDUSTRY became an abstract sponsor. Reference to INDUSTRY was increasingly important for science in the late twentieth century. Spokespersons for industry (that is, INDUSTRY) were expected to sit in committees, and chairmen of science funding agencies are often required to have some experience in industry, or at least experience in the private sector.[13]

There are other such combinations of concrete and abstract sponsors, the MILITARY being a prime example in the post World War II situation – even if the link of science to the MILITARY is now also contested. The ideograph SUSTAINABILITY has become powerful in recent years. NGOs (non-governmental organisations) ranging from Greenpeace to the International Council of Scientific Unions present themselves as spokespersons, and are involved in agenda-building for science. Individual scientists and groups develop new approaches (including holistic ones) to link up with SUSTAINABILITY. Being able to invoke SUSTAINABILITY mobilizes symbolic and financial resources. Even if it also involves one in the debates and controversies about the environment, global climate change, and issues of expertise and decision making generally.

Abstract sponsors create a space, and to some extent a protected space, for scientific research, and are thus part of the evolving social contract between science and society. They also play havoc with existing disciplinary distinctions. Just as academic disciplines could emerge and stabilize through the backgrounding of sponsors, the "return" of the sponsors (concrete and now also abstract) introduces dynamics leading to hybrid scientific communities and hybrid forums

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[12] A similar phenomenon is the recent, and reluctant, acceptance by scientists of accountability, because "we're spending the taxpayers' money."

[13] By now, USERS have become important as an ideographic category as well (Shove and Rip, 2000).

carrying new or at least modified ways of knowledge production. Patient associations in medical and health research would be one, and striking, example (Callon and Rabeharisoa, 2003). Research supported by new sponsors like the Bill and Melissa Gates Foundation, or idiosyncratic "upstart philanthropists" like Fred Kavli,[14] is not bound to existing categories.

The Existing Regime Is Opening Up

The whole constellation of spaces and sponsors and modes of knowledge production which appeared to stabilize, and even lock-in, after the Second World War as the regime of "Science, The Endless Frontier" now appears to open up. If Gibbons et al. (1994) are right, a new regime – their Mode 2 – is upon us. Such a programmatic claim is premature, but they (especially in Nowotny et al. 2001) do offer evidence that the existing regime is evolving, and opening up to more interactions with society.[15] So the first step is to map ongoing changes, and that is where the perspective I outlined is useful.

Part of the dynamics derives from overall changes in our societies, which have been diagnosed as "reflexive modernization" (Beck et al. 1994). A key element of this diagnosis is that institutions of modernity, including science, cannot continue as they were used to. The heterogeneity that is encountered at the moment can be deplored (by scientific establishments) as threatening science-as-we-know-it.[16] But it can also be seen as an opportunity, because openness and variety allows renewal similar to what happened in the melting pot of the European Renaissance. As Beck and Lau phrased it: "what appears as "decay" and de-structuration in the unquestioningly accepted frame of reference of first modernity (and in this respect is bracketed off and marginalized), is conceptualized and analysed as a moment of potential re-structuration and re-conceptualization in the theoretical perspective of reflexive modernisation" (Beck and Lau, 2005, 552).

The dynamics are not just institutional. There is "new" natural history, i.e. pattern recognition modes of knowledge production supported by ICT tools, GIS (Geographical Information Systems) being one example (Rip, 2002b), there is the advent of technosciences, and in general, the renewed importance of tinkering in

[14] Fred Kavli has started the Kavli Foundation which creates Kavli Institutes in basic areas of astrophysics, nanoscience and neuroscience, the fields that Kavli is interested in. The phrase "upstart philanthropist" to characterize Kavli was used in a news article by Michael D. Lemonick about Kavli, in *Time*, August 13, 2007, p. 44.

[15] Others come up with similar diagnoses of opening up of what was closed/protected before, cf. "porous university" (De Boer et al. 2002).

[16] Cf. Asher et al., 1995: In October 1994, "the world science leaders" met in Jerusalem. They were defensive, but prepared to defend the bastion of science. And: "if we do not measure ourselves, somebody else will" – "upper management," the government, funding agencies, whoever – and they will probably do an even worse job of it."

the lab and in the field. This merges into making things – up to plants and animals – that (might) work, and having experimental infrastructures ("platforms") for research purposes, which can also be exploited for product development. This is very visible in the newly fashionable and "theory-poor" field of nanoscience and nanotechnology (Nordmann, 2008).

There are meso-level developments as well, starting as responses to ongoing changes but then contributing to them in their own right. A key development is the increasing importance of the (new) category of "strategic research", epistemically as well as institutionally. In the formulation of Irvine and Martin (1984) it clearly reflects a new division of labour between the quest for excellence and for relevance – and it may actually combine them.[17]

- Basic research carried out with the expectation
- that it will produce a broad base of knowledge
- likely to form the background
- to the solution of recognized current or future practical problems

This creates a new protected, but not necessarily closed, space.[18] Drawing on this, Centres of Excellence and Relevance are becoming a new and important institutional form, within universities and outside them. Their viability relates to the emergence of markets of strategic research (Rip, 2002a , 2007).

Strategic research has now become pervasive, and science institutions adapt and evolve (Rip, 2000b ; Rip, 2002a). A regime of Strategic Science might emerge, replacing – or grafted on – the regime of Science, The Endless Frontier, with master narratives of technoscientific promise (Felt et al. 2007, 21–29) and relevant expertise. It is carried by an alliance between politicians and science policy makers on the one hand, and a new elite of scientists promising to contribute to wealth creation and sustainability, on the other hand.

Part of the evolving regime, but more difficult to handle, is public scrutiny of science, ranging from accountability to involvement with publics. This is linked to increasing recognition of the value of experience-based knowledge, and further shifts in the notion of expertise (Callon et al. 2001 and 2009). At the same time, there are attempts at epistemic authority by those who used to be seen as outsiders, up to the US Congress pronouncing on what is "sound science" (i.e. direct observation

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[17]Stokes (1997) showed the possibility of such a combination ("Pasteur's Quadrant"). His analysis, however, is a typology rather than diagnosis of dynamics. Cf. his use of historical figures (Pasteur, Bohr and Edison) to typify the three main quadrants, and his neglect of the fourth quadrant.

[18]There is also the new category of "translational research", now very visible in biomedical and pharmaceutical research, up to use in the Roadmap of the US National Institutes of Health (Atkinson-Grosjean, 2006, 171). The category is more broadly applicable, e.g. to engineering sciences and to environmental sciences (where the "translation" is towards decision making). The fact that there is such a category tells us something about changes and how these are captured with a label – under which a new protected space can function.

rather than models and "theory" – which might be used to support environmental and other regulation). Following Brown (1996), one can call this conservative epistemic politics.

To turn this mapping of ongoing changes into a diagnosis, I return to my analysis of protected spaces as a functional requirement for science, at micro-, meso- and macro-levels. Protected spaces are essentially ambivalent: protection enables and nurtures, but also constrains, and might imprison. While the earlier regime is opening up, there is also closing down towards new institutionalizations, including new protected spaces.[19] Functional equivalents of the disciplines under Mode 1 might emerge. With this ambivalence in mind I will discuss, in the next two sections, changes (challenges) in knowledge production, and responses of scientific institutions to the opening up of the earlier regime.

Ambivalences of Opening Up Institutionalized Knowledge Production

The opening-up of the earlier regime occurs in a variety of ways, but a key dynamic is the recognition of non-institutionalized knowledge production and use, which had been excluded, over the centuries, from the core business of science. This "core" business is now getting further "recontextualized", building on earlier layers of recontextualisation of science in society (e.g. strategic research programmes from the 1970s onward). This is visualized in the diagram below (Fig. 2), together with examples of new boundary interactions.

For my overall argument, there is no need to discuss the details of the diagram (see Rip, 2007). I note that all the new developments have a socio-epistemic character. In Nowotny et al. (2001), some of these are discussed as well, and linked to a notion of recontextualisation that is similar to the one I use here. Nowotny et al. (2001) also introduce the notion of socially robust knowledge production, adding an additional societal layer to ongoing scientifically robust knowledge production. They see this as the way forward for science in society, but tend to argue the value of societal participation per se, i.e. a political consideration. There should be epistemic considerations as well. To introduce these, I will articulate the pursuit of robust knowledge as the central characteristic of "scientific" knowledge production. Then, ongoing changes can be discussed as changes in the division of labour in the production of robust knowledge.

There are ambivalences involved in the production of robust knowledge, and not only because society enters the picture. Epistemically, robustness of knowledge has to do with how it will work again, at other times and in other places: it must be able to withstand variety and interference. To have robust outcomes,

[19] The terminology of "opening up" and "closing down" is inspired by Stirling (2008).

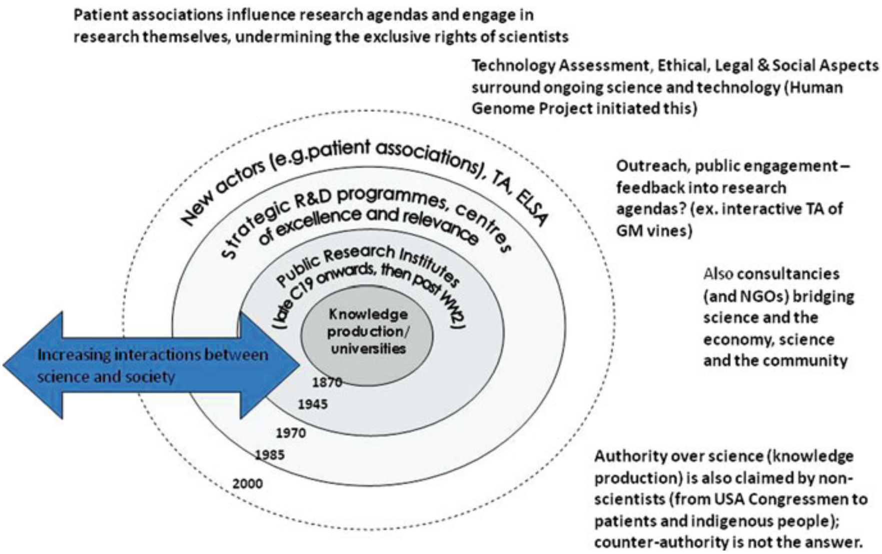

Fig. 2 Opening-up and recontextualisation of science in society

interactions and struggles are important, some of them (like peer review and struggle for visibility) focusing on traditional scientific robustness, while others will be linked to difference in values or interest strategies. The quality of the knowledge that is produced will improve through such agonistic (and sometimes antagonistic) interactions. These force actors to articulate the merits of their position, to search for arguments and counter-arguments. But they can also lead to impasses, or to repression of innovation.

This perspective on robust knowledge production is broader than the Popperian-Mertonian emphasis on fallibilism and organized scepticism. The latter now appears as a special case located within the protected space of an academic scientific community, and abstracted from many of the vicissitudes of the real world. Of course, agonistic struggles in unprotected spaces have their problems. They can lead to impasses, when parties limit themselves to mutual labeling of the other as contemptibly wrong, as has happened in the debate on nuclear energy (and happens to some extent in the biotechnology debate, although third parties e.g. supermarkets now intervene and help to overcome the impasse). But such processes occur in academic science as well, when insider-outsider or regular-deviant labeling hinders productive interaction. On this count, there is no reason to hark back to the protection afforded by academic scientific communities.

The production of robust knowledge as well as the assessment of its robustness need not be the exclusive domain of relevant scientific and technological communities. Epistemic quality should continue to be the goal, but it is not a matter of following methodological recipes. A key point, visible already in Kuhn (1977), is that knowledge production requires some closure of epistemic debate.[20] Such closure reduces complexity, but at a cost: alternatives will be backgrounded. Thus, there is a *prima facie* argument to entertain variety. But variety has costs as well: continued exploring need not lead to usable outcomes.

The ambivalence of entertaining variety is exacerbated when there are different cultural backgrounds, up to different "cosmovisions." An important domain where such struggles occur and have epistemic import is indigenous knowledge. By now, it is politically correct to accept claims from other cultural backgrounds to deserve a place under the epistemic sun,[21] but this creates tensions for Western science.[22] It is now also practically correct to appreciate indigenous knowledge, as an as yet insufficiently tapped resource for development. Is it also epistemically correct? When cultural communities take over the quality control that used to be done by disciplinary communities there may be a problem of creating unproductive, because closed, protected spaces. The same exclusionary tactics will be involved ("we are the only ones who can judge") as Western scientists can apply, but these tactics will now foreground cultural heritage rather than new knowledge production.

A case in point is New Zealand's funding of Maori Knowledge and Development research. It is but one example of the overall growing interest in indigenous knowledge, practically and politically, and the increasing voice and power of indigenous communities and non-Western approaches to knowledge (Battiste, 2000, Smith, 2000). In this case, the creation of a closed shop was encouraged by science funding actors emphasizing that Maori development research must be by Maori, for Maori, and follow a Maori world-view and approach to knowledge.[23] In other words,

[20]The importance of provisional closure through reference to the status and productive use of expertise is also visible in regulatory science and now leads to attempts to create (even if precariously) new and authoritative forums.

[21]Compare the advent of an "indigenous Renaissance" world-wide (Battiste, 2000), and science funding agencies in countries like South-Africa and New Zealand supporting indigenous knowledge production.

[22]Cf. how the International Council of Scientific Unions wrestled with the need to accommodate indigenous knowledge, also for political reasons, and wanted to continue to condemn pseudo-science. After some epistemological considerations, they offer an institutional-political answer: "Traditional knowledge is therefore neither intended to be in competition with science, nor is such a competition the necessary result of their interaction. On the contrary, as we have seen earlier, traditional knowledge has informed science from its very beginnings and it continues to do so until today. If a competition between science and traditional knowledge arises at all, then the initiative typically comes from people who want science to replace these other forms of knowledge. Pseudo-science, on the other hand, tries at least partly to delegitimize existing bodies of scientific knowledge by gaining equal epistemological status. The existence of pseudo-science as an enterprise fighting science is thus invariably bound to the existence of science whereas traditional knowledge stands on its own feet." (ICSU, 2002, 11)

[23]The phrase is from the (2001 and later) government science budgets, "output class" Maori Knowledge and Development. The sentiment is carried broadly, as was clear in a June 2001 meeting of science officials and Maori representatives (http://www.morst.govt.nz/creating/maori/huiprogramme.html, accessed 10 October 2001). Pete Hodgson, Minister of Research, Science and Technology emphasized: "(. . .) in last year's Budget we created a new

positive discrimination rather than exposing knowledge production to challenges so as to make it more robust. Of course, positive discrimination may be necessary for some time, to nurture what still has to grow. The ambivalence returns with the question how long the nurturing should continue, and what its form should be.

The reference to "what still has to grow" may be found condescending, and the closed-circuit message of "by Maori, for Maori and with a Maori worldview" may be applicable to some peer review circuits in Western academic science as well. Still, it is important to keep the question of epistemic quality alive, also for indigenous knowledge, without it becoming an excuse to reject indigenous knowledge approaches out of hand. The key entrance point is the creation of spaces and how they function. The science policy initiative in New Zealand created a protected space at the meso-level, and could structure it only in terms of the way present funding agencies do their business. Thus, responsibility for the emergence of the closed shop will rest with them just as much as with emancipatory movements for indigenous knowledge.

Institutional Responses of Funding Agencies and Universities

The New Zealand science policy initiative is one example of science institutions being on the move, half-heartedly or actively engaging with the new challenges. In a sense, they are forced to move because their context is changing, and because they experience internal changes, e.g. new ways of knowledge production. I am talking here about the traditional institutions of science which are set in their ways, not about new types of institutions for whom opening up of the regime offers opportunities. However, for the question of overall change (and its diagnosis) the traditional institutions are a key entrance point because they cover a large part of the system of science. Thus, I will focus on traditional institutions, and offer an assessment of how funding agencies and universities are changing, and may change further.

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funding stream for research specifically by Maori for Maori. (...) The type of research supported by this stream embraces Maori customs and knowledge, using this base to research and develop tools and mechanisms to improve Maori health, social and economic well-being. In the same meeting, James Buwalda, CEO of the Ministry of Research, Science and Technology (MoRST), emphasized that indigenous knowledge systems have a valid role in economic, environmental and social development. And he adds: "Maori world-views have equal status alongside Western science." Similarly, Minister Hodgson was willing to say: "I think Maori think differently. (...) different ways to approach a problem, explore it and solve it. (...) good for us as a nation."

Their embracing the Maori perspective marks a shift in policy at the top. In the meeting, called a *hui* to emphasize its link to Maori culture, Michael Walker (himself Maori) referred, somewhat cynically, to "the hymn sheet of the science/research agencies" about the importance of Maori knowledge. Hymn sheets may well have effects.

Funding Agencies

From the 1950s onward, national-level science funding agencies have become a keystone institution for the modern social contract between science and society, as they were integrated in the inward-looking world of the Republic of Science (Polanyi, 1962; Rip, 1994). They have a strong institutional identity which continues to be reproduced even while circumstances are changing. As government agencies, they need to be accountable, so will not find it easy to respond flexibly to changing circumstances. In a sense, they are prisoners of their own achievement in doing a good job (i.e. funding research and assessing proposals).

Even so, the external pressures for relevance of science from the 1970s onwards had to be responded to, somehow. The funding agencies adapted, some more than others,[24] and shifted their practices by including relevance or merit criteria in addition to scientific quality of proposals. At first the additional criterion of relevance was not taken very seriously, and most often judged by scientists, and thus reabsorbed into the Republic of Science. Subsequently, the category of strategic research covered more and more of the research activities sponsored by funding agencies, and definitely featured in annual reports and strategic plans.

After the 1980s, links with market actors had become unavoidable, and were integrated (to variable extent) in their workings, e.g. in their consultations and in the composition of boards and panels. The need for broader consultation about strategies (and the need to articulate strategies at all) was visible already in the 1990s, and became generally accepted and widely practised in the 2000s. There is some opening up to plural stakeholders (not just market actors). By the late 2000s, overall changes in the science system were taking hold, including the more active role of patient associations and environmental groups, and the reference to "responsible development of science", in particular of newly emerging science and technology like nanotechnology (Kearnes and Rip, 2009).

It is not clear whether these new developments will be temporary exercises, and be reabsorbed into the main thrust of the enlightened modernist response.[25] Given the strong mission of national-level funding agencies, and their need to be accountable, they cannot shift very much. If the ecology of the science system would change, however, for example because of the increasing importance of private funding bodies, especially charitable foundations, they could, and would have to move.[26] In the UK, where the Wellcome Trust funds more medical research than the

[24] For each national level funding agency (or agencies), the institutional path works out differently, with some resisting the pressure to include relevance (e.g., Germany), and others embracing it, at least in public declarations (e.g., UK).The German Deutsche Forschungsgemeinschaft justifies its reluctance by referring to the freedom of research, as laid down in the Constitution.

[25] This terminology derives from an analysis of responses of science institutions to reflexive modernization (Delvenne and Rip, 2010). One possibility then is that modernist approaches continue, but in an enlightened way.

[26] I have drawn up scenarios of how funding agencies might develop, maintaining their core assets, but moving more freely in the ecology of national research systems (Rip, 2000b).

government funding body (Medical Research Council), the move has started, there are joint programs and coordination.

Universities

The Wittrock and Elzinga (1985) volume on universities as a “home” (i.e. protected space) for the scientist, marks the beginning of an ever-expanding set of studies and comments on how universities are endangered by bureaucracy and “epistemic drift”. Focal points are “new public management” as imposed on the universities from above (but often embraced by boards and administration as a way of strengthening their role as a “steering core” (cf. Clark, 1998)), and the notion of a “third mission”, towards society, which is felt as an imposition, even if some entrepreneurial universities take it as part of their profile (together with excellence).

This literature begs the question whether scientists should have a “home” (a protected space) “of their own”, and whether that should be the university. Many universities, and most other institutions of higher education, are not dedicated to research. Also, there is proliferation of higher education institutions globally, up to claims to create research universities. Indian tycoon Anil Agarawal is building a university town, and was quoted in *Financial Times* (July 22, 2006) as saying: “Vedanta University will be modelled on the likes of Harvard, Oxford and Stanford, catering for 100,000 students. What is money for if not to be made and given back to society?”

For my question about responses of universities, there are two interesting developments. First, attempts to create conglomerates. In the Netherlands, Wageningen University and Research Centre is a (precarious) combination of an agricultural university and dedicated agricultural research institutes. In France, there are collaborations between universities, Centre National des Recherches Scientifiques, and some of the big public research institutes. In South Africa, the alliance between the University of Pretoria and the Council for Scientific and Industrial Research has drawn attention. In Germany, Göttingen University has created an alliance with five Max-Planck-Institutes and other research institutions in the area. The establishment of Karlsruhe Institute of Technology, a merger between the university and the big public research centre in Karlsruhe, is a recent and very visible example. The message is that the traditional mission and boundary of the research university is not sacrosanct.

Second, the emergence of a new kind of entity, Centres of Excellence and Relevance embodying and pursuing strategic research. This started in the 1980s with the USA Engineering Research Centers, the UK Interdisciplinary Research Centres, and the Australian Collaborative Research Centres. By now, Centers of Excellence and Relevance emerge everywhere, and they are not limited to the context of research universities. In fact, they are a new species in the “ecology” of present research and innovation systems.

Such Centres can thrive because there is, by now, a “market” for strategic research, as well as direct support of excellence by funding agencies and

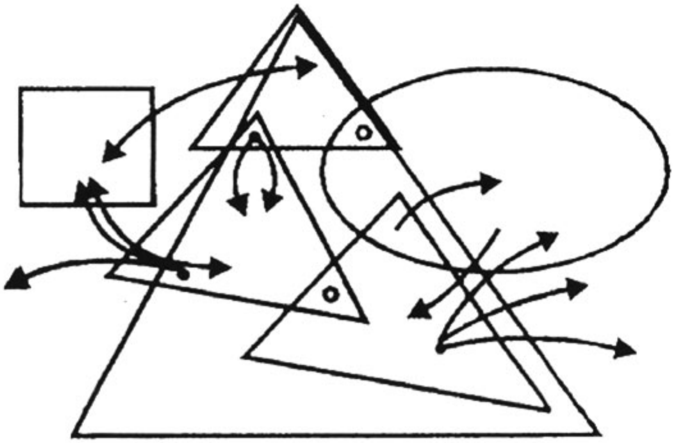

Fig. 3 The university "complex" of the future

independent sponsors. When such Centres are part of a university, they are somewhat independent in terms of resource mobilization, and they can throw their weight around because they are important for the profile and competitive position of the university. In Rip (2002a), I have used my own university and its MESA+ Institute for Nanotechnology as a case study. Subsequent developments show the mutual dependency of the university and this Centre for Excellence and Relevance. To put it bluntly: the university is bursting at its seams because it houses such Centres. It has to re-invent itself – or give up being a research university.

The net effect is reinforcement of the pressure on research universities to transform themselves into the equivalent of a holding company, as is visualized in the diagram below (Fig. 3). As soon as this happens, there will be openings for further developments, including the emergence (and design) of new kinds of protected spaces.

In Conclusion

The constellation of partially nested protected spaces of the regime of Science, The Endless Frontier, is opening up, and at all levels. Some such spaces, like funding agencies, modify themselves but essentially continue their path. Universities have more activities and concerns than protecting scientific research, but if they are research universities excellence and relevance of their research is an important part of their profile. Rather than continuing their traditional autonomy, they now enter into symbiotic arrangements. Centres of Excellence and Relevance are already somewhat independent of the university, and are becoming protected spaces in their own right. To coin a phrase, they could be the "home" of Mode 2 knowledge production.

The other main trend is the recognition of the value of experience-based knowledge. This has created openings, and there are experiments, but there are no institutionalized protected spaces yet. In the case of indigenous knowledge in countries like New Zealand where the issue is politically sensitive, incipient institutionalization showed the ambivalences of protected spaces (epistemic and institutional). There are other interesting developments as well, like the recognition of consultancies and environmental organizations as carriers of knowledge production.[27]

My socio-epistemic diagnosis (at the micro-level) of the need for protected spaces, even if their productivity is based on exclusion, is relevant at meso- and macro-levels as well. Again, there is the essential tension between entertaining variety (to ensure innovativeness) and maintaining some closure (to be productive). The risk is that these tensions will be short-circuited through institutionalization focusing on short-term productivity. Thus, in general, one should maintain (and even cherish) some heterogeneity, so as to avoid reducing complexity too much. To postpone a lock-in, one has to be prepared to live in (partially) unprotected spaces.

There is a governance aspect as well. This is brought out well in the MASIS Report (Markus et al. 2009), where US President Obama's phrase about "restoring science to its rightful place" is rephrased as a question about an *adequate* place of science in society, taking ongoing changes and contestations into account.

> [There is a] patchwork of transformations and tensions [which] does not result in a clear picture of an 'adequate' place of science in society. In fact, the open debate about the place of science in society should continue, and experiments to address tensions and other challenges should be welcomed.

The challenge is to support ongoing dynamics, rather than containing them, so dynamic governance is called for (Markus et al. 2009, 4–5).[28]

Given the uncertain future of unprotected spaces and dynamic governance, the immediate challenge is to avoid reducing uncertainty by reifying an earlier epistemic pattern ("this is what science is, interference will not be tolerated"), and/or an earlier institutional pattern ("let's continue with what we have been doing and organizing

[27] Claudia Neubauer has called this the emergence of a "third sector" of knowledge production, in a paper for the preparation of the Expert Group Report *Taking the European Knowledge Society Seriously* (Felt et al. 2007).

[28] These quotes are part of a more specific diagnosis: "The revival of excellence of science as a goal, reinforced by the establishment of the European Research Council, provides an occasion for international competition, and for performance indicators based exclusively on publications in ISI-indexed journals. At the same, there are calls for increased democratization of science, concretely, the involvement of more stakeholders. More stakeholders, and existing stakeholders in new roles, are involved." "There are also developments in the governance of science in society. The governance of scientific institutions is under pressure, not least because of different contexts of governance, simultaneously pushing innovation, democratization and scientific integrity. New forms of governance are emerging: the discourse on responsible development, including attention to ethics and codes of conduct; interactive forms of technology assessment; and experiments with public engagement. Again, these are not without tensions, but they indicate that we do not have to fall back on traditional forms of governance." (Markus et al. 2009, 4–5)

all along, perhaps modify it a little"). Of course, there are epistemic and institutional achievements that should be cherished, but even then, one has to understand how they came about, and whether they can continue to be productive under the new circumstances, or should be modified, even be replaced. This is how we can arrive at a dynamic diagnosis, profiting from a long-term perspective and considering the evolving ecology of science systems in context.

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The Cognitive, Instrumental and Institutional Origins of Nanoscale Research: The Place of Biology

Anne Marcovich and Terry Shinn

This chapter explores four features of nanoscale research. One is frequently confronted with the claim that nanoscale research (NSR) constitutes nothing new in science – that it is simply old science pursued under a new name in order to benefit from changes in funding policy. In effect, NSR is old wine in new bottles. This assertion is in part connected to the fact that certain areas of NSR are deeply rooted in semi-conductor physics and technology and in solid state physics generally. However, much nanoscale research is unrelated to solid-state physics. We will document the existence and importance of numerous other domains in NSR linked to the birth of materials by design. Moreover, it will be shown that NSR is the product of an instrument revolution. Contrary to affirmations that NSR is continuity under a different name, we will argue that the substance of the field arose suddenly and completely unexpectedly in the course of a single decade. A variety of science emerged that is in some respects novel, being grounded in the combinatorial of new instruments, new materials and a new logic regarding the formulation of research questions.

Unlike all other extant domains of science, NSR cannot be identified by its focus on a class of materials or a category of forces. In the case of NSR it is not possible to point to a defining object or dynamic. Nanoscale research refers solely and uniquely to a physical dimension – objects of any and every category whose dimensions range between one and, say, thirty nanometers. Thus NSR encompasses chemical, physical and biological substances. Our second objective in this chapter is to identify a common denominator that permeates the entire field of NSR. We suggest that the two related themes of detection and control permeate the whole of NSR. Third, we see in biology-related NSR many emblematic instances of our assertion regarding control and detection. Although they are ubiquitous in NSR,

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the themes of manipulation and identification prove to be particularly prevalent and decisive in areas associated with biology. Our attention is drawn to biology, not least because in recent years it appears to have acquired increasing importance, with biological materials being used as resources in non-biological research, constituting an object of research in themselves, and providing a bridge between the microscopic and macroscopic scales.

Finally, the complex link between science policy and the emergence of a new field will be discussed. The material and instrumental foundations of NSR were firmly laid during the 1980s, and NSR expanded swiftly during the 1990s. However, it is only during the first decade of the twenty-first century that NSR began to grow exponentially and to constitute a veritable "movement". While the instrument revolution and the birth of materials by design, along with the legitimization of certain new concepts, provided adequate grounds for constituting NSR, the community remained merely embryonic. Although the pre-conditions for a community existed, community enfranchisement required an additional factor, namely, the institutionalization of the emergent movement through the investment of social resources in the guise of the 2000 US National Nanotechnology Initiative. The number of NSR publications skyrocketed once appropriate institutions, symbols and national resources had been put in place. Knowledge alone proved to be not enough.

Much of the data for this article is drawn from our research programme on the Feynman Nanotechnology Prize which we began in 2007. We regard this source as strategically useful because the annual prize is designed to identify the most promising work in NSR. The prize was initiated in 1993, and since 1997 two laureates have been named every year, one for theory and one for experimentation. Some information acquired in related interviews is also included in this chapter.

Early History

To our knowledge the term "nanoscale", along with systematic research on nanometric objects, was first introduced during the early years of the twentieth century in the course of investigations conducted by Richard Zsygmondy (1865–1929) on the composition of colloids (Zsygmondy, 1898, 1966). A debate raged at the time over whether colloids consist of crystals or of particles, a long-disputed matter which the Viennese scientist strove to settle. In order to do so, he sought to produce a fine powder out of gold. After numerous attempts to reduce gold to the smallest possible division, Zsygmondy came by chance across an article by Michael Faraday (Faraday, 1857) in which he noted that, when exposed to light, gold in a finely divided form produces light of quite a different colour – red in tint – from gold in its solid state or when it is composed of larger particles. Using a technique for obtaining very fine divisions of gold, first developed by Faraday half a century earlier, and employing the ultramicroscope that he himself had invented during the 1890s, Zsygmondy studied the gold divisions in a liquid. He immediately determined that colloids consist of fine particles and not crystals, which are much larger in size.

This determination and characterization of the particles was made possible by combining his ultramicroscope with a refinement consisting of the Tyndall Effect.[1] Zsygmondy was thus able to see particles measuring only ten nanometers in diameter which he could count for a unit of volume. He also gleaned some information about their form, noticing that the form and size of matter strongly affect certain properties, notably optical properties. Hence gold in the form of nanoparticles produces red light, while larger particles of gold yield the yellow light to which we are accustomed. This study of colloids led to the introduction of nanoscale terminology; more specifically, the scale and form of matter were interpreted as lying at the heart of physical characteristics and material behaviour. Zsygmondy was awarded the Nobel Prize for his work on colloids and the invention of the ultramicroscope in 1926, the first of three such prizes to be awarded for nanoscale-related research. During this period of nanoscale investigation, the distinction was established between the deterministic and the stochastic behaviour of matter at this scale. This distinction played a key role in much subsequent nanoscale research, as it suggests the possibility of control of nanoscale objects.

An event occurred in 1959 which, seen in retrospect, presaged the advent of NSR. In that year US physicist Richard Feynman (1918–1988, Nobel Prize in physics 1965) gave an address to the American Physical Society at Caltech entitled "There is Plenty of Room at the Bottom." In his talk Feynman advanced three key ideas. First, he claimed that it was possible to construct objects by assembling atoms one by one – the so-called bottom-up process. Second, engineers had long managed to build devices without possessing any thorough scientific understanding of their materials. This implies a refutation of the argument that because we do not possess full knowledge of the world of atoms we cannot build useful devices. Third, implicit in Feynman's message is the idea that the world of molecules and atoms is deterministic and not unmanageably stochastic. Up to that point scientists had viewed the behaviour of atoms as stochastic and as uniquely governed by the Heisenberg Principle. The suggestion that atoms and molecules might behave as deterministically governed particles ran contrary to the orthodoxy of the day. Finally, Feynman formulated what appeared to represent an inconceivable challenge: he proposed a prize of one thousand dollars for the first person who could build a machine in the space of a few microns, and a prize for the person who could encode the quantity of data corresponding to the Encyclopaedia Britannica within a space equivalent to the head of a pin. To the astonishment of all, the first challenge was met successfully within little more than a year and the second in 1985. Based on an arsenal of new materials, instruments and learning, within fewer than 20 years this vision of controlling matter at the atomic and molecular level became standard practice.

[1] The Tyndall Effect is the scattering of light by particles in suspension in a colloidal solution.

Instruments and Materials

The 1980s gave rise to a spate of new materials and to several key instruments relevant to NSR. Many of the materials crucial to nanoscale research were introduced in a little over 10 years. These were not materials born of nature but rather substances produced by humans, arising directly from the laboratory. The rise of nanoscale research is synonymous with the rise of materials by design.

The same decade produced many of NSR's most frequently used instruments as well. Two instruments in particular have proven essential: the scanning tunnelling microscope (STM), invented in 1981, and the atomic force microscope (AFM), introduced in 1986. Shortly after their introduction, each of these devices spawned related apparatus. Both the STM and AFM perform two functions essential to NSR, in that they are both detection and control devices: they identify and they also intervene directly. They enable the detection as well as the manipulation of single as well as of aggregated atoms. Furthermore, in some cases the extraordinary precision of the two instruments permits study of the form of molecules, providing information about their surface and internal structure. Since form is connected to structure and structure is in turn connected to function, the AFM and STM enhance control by means of significant second-order information concerning the dynamics of nanometric substances.

Materials by Design

Self-assembling molecules – "SAMs": Self-assembling molecules, better known as SAMs, are self-assembling and self-organizing molecules with no outside intervention. The study of SAMs began in 1983. Several families of SAMs exist. The most common are alkanetheols. SAM molecules possess a "head" and a "terminal" group; the head attaches to a substrate, most frequently gold or silver, which ensures the anchoring of the molecule. The terminal group is "functionalized" by the configuration of its structure and form. Thanks to processes of recognition and self-assembly, molecules attach themselves to a terminal group. Due to the specificity of location of a SAM and to the specificity of the molecules they attract, SAMs constitute a prime control mechanism in nano. This capacity of control is shared by other materials central to NSR, as will be demonstrated below. SAMs are often arranged in the pattern of an "array" composed of hundreds, even thousands, of molecules that are specifically positioned. The possibility of introducing and using SAMs was itself contingent on having an instrument capable of patterning and studying them, such as the AFM and STM, which enabled their chemical properties, their density and the geometry on the substrate to be examined. This will be discussed in detail below.

The story of SAMs invites comparison with earlier forms of thin layers, such as those developed by the famous chemist Irving Langmuir (1881–1957), who was awarded the Nobel Prize for chemistry in 1932. In this case, no technique existed to ascertain the composition or the precise position of the molecules which form the thin layers; in particular, no capability existed for controlling individual molecules.

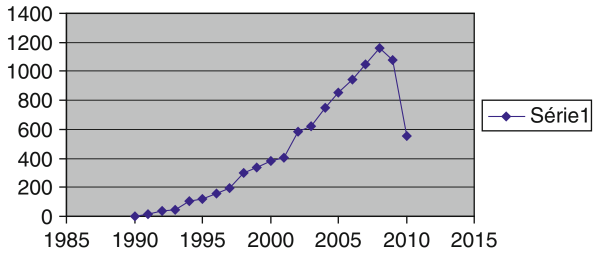


Fig. 1 Self-Assembled Molecule* (topic): 9708 records (Consulted on Isiweb 9.07.2010)

The ability to detect and to manage a single molecule is at the heart of NSR and it is this, perhaps more than any other aspect, that fuels the endeavour to control which is so characteristic of the field. Since this control is linked to the terminal group which determines functionality, this is evidence of the deterministic approach, in contrast to the stochastic view of Langmuir and his understanding of thin layers.

Work on SAMs grew exponentially during the 1990s (see Fig. 1) and studies related to SAMs proved particularly relevant to research in chemistry. As we shall see below, their importance in NSR is also linked to the fact that fundamental biological molecules such as DNA or proteins are SAMs.

The way in which the tail groups organize themselves into a straight, ordered monolayer is dependent on inter-molecular attraction, or Van der Waals forces. Van der Waals forces arise from the dipoles of molecules and are thus much weaker than the surrounding surface forces at larger scales. The assembly process begins when a small group of molecules, usually two, come sufficiently close for the Van der Waals forces to overcome the surrounding force. The forces between the molecules orient them so that they are in their straight, optimal configuration. In biological SAMs, such as DNA or proteins, this configuration dimension is particularly important.

SAMs enjoy a triple status: the status of the material itself explored during research; the status of a research instrument; and the status of a material employed in numerous domains extending from pharmacology to different forms of biological diagnosis.

Fullerenes and nanotubes: The family of materials known as “fullerenes” is the first category of nanometric substances to have been systematically built inside the laboratory. Fullerenes have been the subject of keen attention from academic scientists, from industrial engineers and from a general public either interested in innovative products or terrified by the prospect of nano pollution.

Richard Smalley (1943–2003) was awarded the Nobel Prize in 1996 for his discovery of fullerenes in 1985. Carbon 60 represents the most common kind of fullerenes, and while it exists in very small quantities in nature in the form of soot, the vast bulk of fullerenes are today produced inside the laboratory or in industrial plants. Fullerenes form a large family of materials that have a hollow spherical structure forming an ellipse or a tube. The spheres are labelled “buckyballs” and the cylinders are called carbon “nanotubes.” They can have either single or multiple layers, known as single-wall or multi-wall nanotubes.

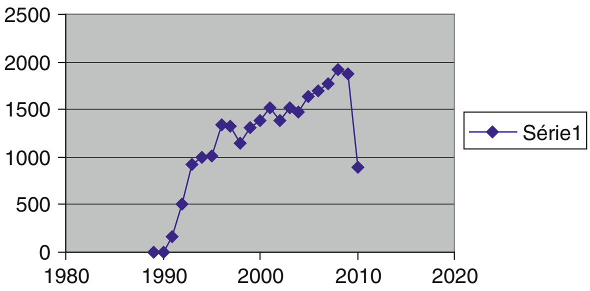


Fig. 2 Fullerene* (topic): 25 791 records (Consulted on Isiweb 9.07.2010)

One might ask why fullerenes have played, and indeed continue to play, such a central role in NSR, such that for almost 10 years now the growth in NSR has consisted largely of research on them. Activities involving fullerenes peaked once around 1995 (see Fig. 2), at the moment when Smalley received his Prize, and again at a time when the AFM, STM and SAMs had become sufficiently embedded to draw other materials into their wake. A further explanation lies in the fact that carbon nanotubes possess exceptional properties of hardness, torsion, electrical and thermal conductivity and weight that bode well for technological applications. Indeed, Smalley – fully aware of the technical possibilities inherent in carbon nanotubes – advanced the idea of using fullerenes to construct an elevator that could join the Earth to a geostationary satellite 40,000 km above the planet's surface.

Quantum dots: The first publication that presented techniques on the construction of a quantum dot and that reported on its properties and the phenomena it generates appeared in 1988 (Randall et al., 1988). A quantum dot is a crystal measuring between two and sometimes as many as thirty nanometers in diameter. It exhibits a unique property, absent from bulk materials, termed "confinement." Electrons induced in a dot remain inside briefly where they constantly shift position. Unlike quantum events occurring in alternative environments, within a quantum dot specific spaces exist where the electrons will never occur. These dots multiply the energy level of their electrons which, when re-emitted as powerful photons, make them extremely useful devices for detection in scientific research when implanted in a target material; they are also useful as diodes in practical applications such as digital display devices. Quantum dots are themselves the object of study by specialists in epitaxy who wish to streamline production or to discover new materials for their construction. They also often serve as detection systems by scientists who use them to monitor other kinds of phenomena. They are particularly abundant in biological research. Finally, the dots have been the focus of considerable theoretical reflection. They constitute the most frequent nano material in circulation today (see Fig. 3).

Quantum dots today are commonly produced through molecular beam epitaxy or chemical vapour deposition. The first candidate dot was reported in the Soviet Union where a physicist observed that one crystal under study emitted light of an unusual colour, and this was attributed, as one possibility, to confinement effects. In the early 1970s, Louis Brus at Bell Labs grew small crystals later associated with quantum dots. The optical and electronic behaviour of the crystals was unusual, and

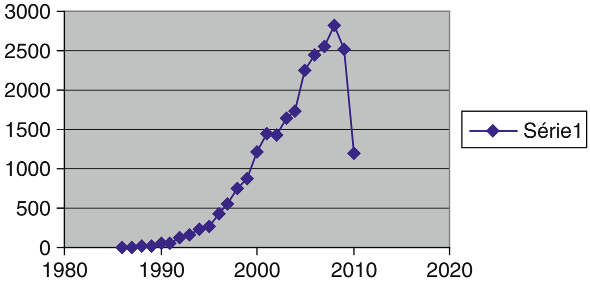


Fig. 3 Quantum dot* (topic): 24 793 records (Consulted on Isiweb 9.07.2010)

they have subsequently come to be regarded as the first dots. However, their production remained a highly unpredictable procedure and Brus soon abandoned this line of research. It was not until 1988 that scientists at Texas Instruments managed to engineer quantum dots from modified quantum wells and to explain the theory and technology of the dots' production and behaviour. Along with SAMs and fullerenes, the quantum dot today is emblematic of nano materials.

Quantum dots belong to a larger family of nano materials, known as "low dimensional materials", which also includes quantum wells and nanowires. The quantum dot is a zero dimensional material because its restricted size allows no possibility for an electron to escape in either the x, y, or z axis. The quantum well is a 2-dimentional object where an electron may circulate along two axes. It thus consists of a thin shaft in which, say, the sideways axis is so reduced that there is no opportunity for movement. In 1988 MarkReed at Texas Instruments introduced barriers in quantum wells as his technique for producing a quantum dot. Quantum dots came into full industrial production in 1992, this mass production explaining the rapid rise in their position in NSR, as shown in Fig. 3. Finally, the nanowire is the most recent member of the low dimensional materials family. It is a one-dimensional material, allowing movement along just one axis. Such a wire is often no more than one atom in diameter. It possesses exceptional properties of electrical conductivity. Where two nanowires cross, a quantum dot is formed.

To summarize, SAMs, fullerenes and quantum dots, wells and wires do not belong to pre-nano research. The phenomena of confinement, too, are largely associated with nano as a specific form of research on biological materials. These objects and items are remote from semi-conductor physics or solid state physics. They are closer to chemistry in some instances. In short, the centrality of these materials casts considerable doubt on the contention that NSR is simply the continuation of solid-state research pursued under a different name.

Instrumentation

As shown above, the demonstration by Zsygmondy that colloids consist of nanometric particles depended crucially on the introduction of the ultramicroscope. This high-resolution instrument proved essential to the discovery of the nano world.

However, during the several decades between the early twentieth century and the 1970s, no new instrument suited to nanoscale research emerged. Nevertheless during the intervening period several devices and materials were developed that indirectly paved the way to the instrument revolution linked to the emergence of nano research.

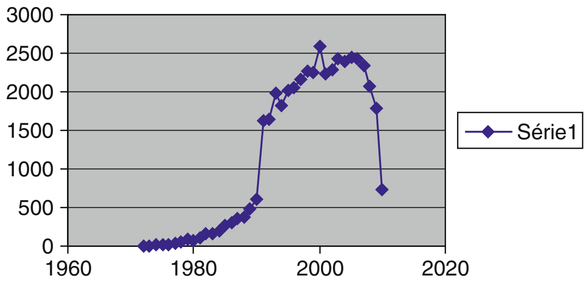
The introduction of the ultramicroscope had proved decisive for Zsygmondy to demonstrate the nanometric nature of colloids and to describe their properties. From the turn of the century up to the 1980s, the ultramicroscope constituted the principal device used to explore the only categories of nano objects studied during this period – nanoparticles. Of course, other instruments were invented during this era, such as the electron microscope in the 1930s and 1940s and the confocal microscope in the 1960s, but they only indirectly contributed to NSR. A new instrument revolution and paradigm in instrumentation would be required in order to drive the nano field forward. This would occur during the decade of the 1980s when NSR began to blossom and to expand at a remarkable pace.

The epitaxy breakthrough: Epitaxy is included here as a kind of instrument because, like devices employed for detection and control, it is an enabling device. Epitaxy technology began to develop during the early years of the twentieth century in connection with research on crystals. The term “epitaxy” derives from the Greek word “epi” signifying “on”, and the word “taxis” signifying “orderly pattern”. In the context of NSR, epitaxy refers to the fabrication of artificial materials that do not exist in nature, that is, materials whose properties are pre-determined. The birth of epitaxy constitutes a revolution in science. In the past, research was conducted on the objects of nature. Now, however, research is carried out on man-made artificial entities and, in effect, on materials by design. According to historian of science Paul Forman, this transition is crucial as it marks a reversal in the relationship between science and technology. Since the 1980s technology has come to replace science at the apex of the hierarchy (Forman, 2007). Forman sees in this switch from the natural to the artificial a corresponding switch from modernity to postmodernity. Note that in the world of epitaxy a deterministic appreciation of phenomena prevails – a posture perceptible in the nano research of Zsygmondy and in the concepts advanced by Feynman. Determinism is in turn linked to control – a central tenet of nano science and technology.

Epitaxy technologies became readily available during the 1960s, and by the 1980s they were at the heart of the electronics industry: transistors, integrated circuits and beyond were manufactured using epitaxy processes. Epitaxy was similarly omnipresent in academic science, where it was used to generate materials for novel experiments and itself became an object of research. Epitaxy fuelled the introduction and growth of the new science of materials as well as material and economic progress in microelectronics.

Molecular Beam Epitaxy: Molecular beam epitaxy became closely associated with the development of NSR. It first appeared at Hamburg University in Germany between 1911 and 1933, and then developed at Columbia University in the United States after the Second World War. It arguably attained its apogee with the research of A.Y. Cho and J. R. Arthur in the 1970s at Bell Laboratories (Cho and Arthur,

Fig. 4 Molecular beam epitaxy* OR MBE* (topics): 38 580 items = 45,002 records (Consulted on Isiweb 9.07.2010)



1975). This technology entails a substrate onto which are deposited multiple atomic layers assembled in a highly controlled structure, in order to obtain a crystal composed of a pre-determined number of different materials. The importance of MBE parallels the steady rapid growth of NSR throughout the period 1990 to 2010, and the convergence between them is particularly noteworthy for the twenty-first century (see Fig. 4 and compare with Fig. 7).

Epitaxy is the nodal point for a cluster of instruments originating in several different engineering and science communities. It thus constitutes a combinatorial which is the hallmark of research in nano science and technology. Molecular beam epitaxy involves a minimum of five different technologies: (1) ovens for the evaporation of materials (for example arsenic and gallium); (2) a computer controlled closure mechanism for the chamber that houses the substrate on which the atomic layers are deposited; (3) an ultra high vacuum pump to remove all gases from the chamber, necessary for the even control of atoms on the substrate; (4) a cryostat that lowers the temperature of the chamber to 77 K which facilitates the evacuation of impurities; and (5) an electronic system that permits measurement of the thickness of each of the deposited layers and allows for control of the entire process. As already indicated, molecular beam epitaxy increasingly lent momentum to the semi-conductor materials industry; it was this epitaxy that generated quantum wells and then quantum dots.

The scanning tunnelling microscope: Although the STM is less current in nano research than the AFM, it is nevertheless the former that is better known by nano observers and among the broader public. This is perhaps because the STM was the first instrument specifically linked to the nano world. Moreover, it is closely associated with the achievement of Donald Eigler in 1989, when the physicist published his famous article in Science which showed how he used the STM tip to arrange 35 Xenon atoms on a surface to spell out the letters IBM, the company for which he worked. It is this highly dramatic image of precisely positioned atoms that brought the existence of the nano world to the attention of many scientists and to the lay public. It was also through this publication that the STM acquired its lettre de noblesse.

The STM brought about a new scientific paradigm, as it allows one to identify, select, visualize, characterize and manipulate atoms and molecules both collectively and individually. The combination of these five capabilities is at the centre of NSR.

The STM was invented in a Swiss IBM laboratory in 1981. The mission of the instrument team was to devise an apparatus which would use topological techniques for detection and localization of defects in semi-conducting materials, notably computer hard disks. L. Young, working at the US Naval Research Center, strove to characterize the atomic structure of conducting surfaces during the 1970s. His efforts failed, unfortunately, due to the technical difficulties involved in controlling the scanning pattern.

The STM is not an optical microscope that operates using lenses and light. Its key feature is that it possesses a microscopic detection tip whose extremity often measures no more than a single atom (frequently composed of tungsten). It is this tip that explores the surface of a conducting or semi-conducting solid. The tip is electrically charged and placed several atomic diameters from the surface. It captures the electrons emitted from the surface due to the Josephson Effect. The magnitude of the current indicates the presence or absence of an atom beneath the tip. The quantitative values of the current are transmitted to a computer whose algorithms then convert the data into images which are studied by the instrument's user. One can thus "see" the number and position of each atom and can study the surface of molecules.

IBM Zurich had instructed two of its top scientist-engineer employees, Gerd Binnig and Heinrich Rohrer, to invent an instrument to identify defects, a programme the huge firm soon abandoned. The two men nevertheless secretly continued their project, leading ultimately to the invention of their STM. They subsequently had great difficulty in convincing colleagues of the fact that their instrument could in fact visualize atoms – a then unthinkable exploit for which they received the Nobel Prize in 1986 (Mody, 2006). The STM's success does not depend uniquely on its extraordinary power of resolution. Unlike many other devices, it operates in ambient conditions. This means it can be used to study biological materials without destroying them, as occurs with alternative instruments. Figure 5 indicates the rapidity of the introduction of the STM into the research community and shows how important it has become to work in NSR.

The Atomic Force Microscope: Nanoscale research was strengthened further in 1986 with the introduction of the atomic force microscope (AFM), again by Binnig and Rohrer. It is highly significant that the AFM, unlike the STM, is able to explore non-conducting materials, vastly enlarging the field of exploration at the nano scale. While it is the case that the AFM is not emblematic of NSR, its importance for the

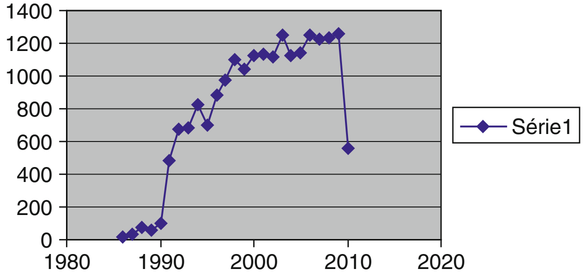


Fig. 5 Scanning Tunelling Microscope* OR STM* (topics) = 20,160 records (Consulted on Isiweb 9.07.2010)

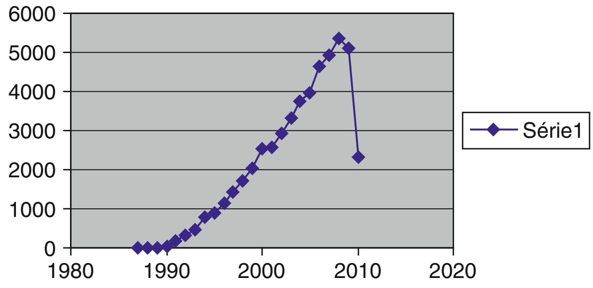


Fig. 6 Atomic Force Microscope* OR AFM* (topics) = 50,507 records (Consulted on Isiweb 10.07.2010)

field clearly exceeds that of the STM, as shown in Fig. 6. Since its invention, the AFM has been cited in articles more than 50,000 times.

The AFM consists of a tip suspended from a cantilever whose elasticity allows it to follow the contours of a surface. This instrument possesses several modes of action. The force field emanating from atoms can activate the tip and cantilever in a non-contact mode that permits remote topological mapping of a material. There is also a "tapping mode" in which the tip periodically strikes the surface of the object in order to determine the shape or to explore specific qualities of the surface, such as density, or to ascertain tiny irregularities. Alternatively, the tip can be dragged across the surface in a "continuous contact mode." The two latter modes are tactile. For each mode, the movement of the cantilever is detected by a laser and subsequently transformed by computer into images. The AFM is more versatile than the STM. It has given rise to a family of related devices such as the magnetic force microscope (MFM) and the thermal force microscope (TFM).

Most significantly, the AFM is both a detection and a control instrument. As a detection device it identifies the presence of objects or measures properties such as cohesion, resistivity, friction, and so forth. As a control device it performs the functions of depositing objects such as SAMs in a specific position, after which functional nanoparticles can be attached to them, or displacing molecules from one position to another. Here the tip is used directly to place or displace atoms or molecules.

The control function of the AFM occurs in the case of the Dip Pen Nanolithography tool (DPN). The DPN was invented in 1999 by Chad Mirkin and his team at Northwestern University. It originated in a research instrument problem, namely the formation at room temperature of water droplets at the end of the AFM's tip, which impaired the instrument's resolving power. The DPN demonstrates the utility of the AFM as a control apparatus because it sets the pattern and delivers the SAM chemical constitutive of the array that it generates – an array that primarily serves biological research programmes (Marcovich and Shinn, forthcoming).

The fact that semi-conducting materials, and the speciality of solid state physics more generally, figure so centrally in NSR perhaps motivates the often voiced comment that there is nothing new in nanoscale research – that it is little more than an old science cloaked in a new vocabulary; and that the new vocabulary is opportunistically necessary to gain access to the abundant funding made available today for nanotechnology research (see Section 4 below).

Science Policy Incentives – from Embryo to Titan

One can see from Fig. 7 that the rate of growth of publications associated with NSR has risen steadily since the late 1980s, and that growth has often been exponential. Nevertheless, the total number of publications remained relatively modest, and it is generally only from 2000 onwards that the quantity of articles has begun to take on huge proportions. The introduction of materials by design and revolutionary instruments during the 1980s definitely provided the basis for NSR, and this basis and the increasingly interesting research questions linked to nano investigations drew more and more scientists and engineers to the emerging field. The vigorous backing given to NSR by the US National Nanotechnology Initiative in the year 2000 nevertheless acted as a catalyst.

As resources poured into NSR, an increasing number of scientists looked to the domain for funding and, in doing so, frequently reoriented their research – or at least added nano to their previous agenda. Thus while materials, instruments and an emerging set of questions in many disciplines served as a precondition to the rise of NSR, appropriate and rich science policy constituted a second set of necessary conditions for the gigantic output that we witness today in the field.

Why did the United States government formulate a science policy in the year 2000 that was explicitly adapted to NSR and why was the programme so generously funded? What lay behind the science policy effervescence of 2000 and the recasting of the nation's science and technology priorities, organization and spending? The announcement of the US Nanotechnology Initiative at Caltech in January 2000 by President Bill Clinton called for a significant reorientation in science and technology research and for the investment of billions of dollars by the federal government (MacCray, 2005). Since the end of the Cold War, science and technology observers and experts and corporate leaders have increasingly addressed a triple problem, as did those in science policy circles and in academic research laboratories. This problem consisted of ever more pressing technological obstacles to the move beyond

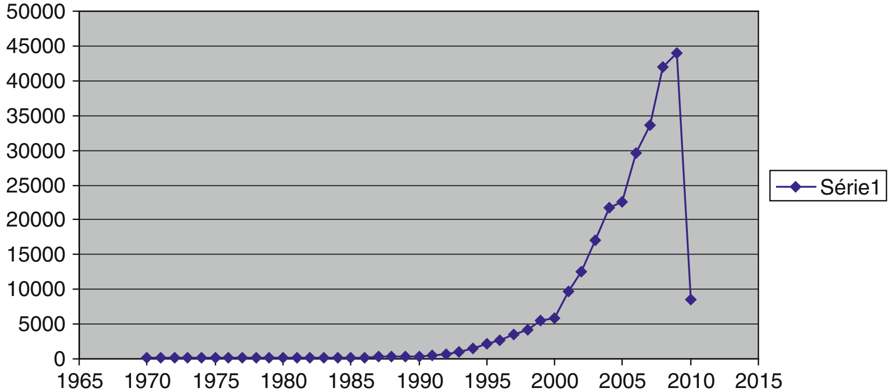


Fig. 7 Nano* (topic): More than 100,000 records (Consulted on Isiweb 9.07.2010)

material limits in the domain of microelectronics for the development of computers and other electronic devices, and it included the associated endangerment of US corporate hegemony in these areas. Many held that without radical innovation, Moore's law would be dashed. Moreover, this period of uncertainty also saw the rapid and powerful entry of new players such as Japan and other competitors in these fields, with well-financed state research programmes and a corporate rationalization designed to enhance international competitiveness.

The sense of urgency to reform and to rejuvenate application-driven programmes in selected technology fields was not shared by everyone at this time, however. In order to affect public policy it was necessary to identify key fields of potentially dangerous technical inadequacies, to propose promising paths of research, and to convince policy makers, entrepreneurs and relevant engineers and scientists to speak out and to act. For several years during the 1990s giant magnetoresistance (GMR) – discovered by Albert Fert and Peter Grünberg, who were awarded the 2007 Nobel Prize – appeared to be a possible solution to the current materials obstacle in computer memory, speed of data processing, and other electronics bottlenecks (MacCray, 2009). This approach temporarily found favour with policy makers. However, in order to mobilize broader support for new science and technology policies and for sufficiently high levels of investment, it was judged necessary to broaden the scope of appeal. The topic of giant magnetoresistance was thus somewhat eclipsed, to be replaced by a far larger domain – a domain that could sometimes be presented as an altogether fresh cognitive, material, consumer and societal paradigm, namely the world of nanoscience, nanotechnology, nanomaterials and consumer products.

In his Caltech speech President Clinton announced that the US Nanotechnology Initiative would immediately receive a budget of two hundred million dollars, soon to rise to three hundred million; today it stands at almost one and a half billion dollars. Between 2005 and 2007, the US Department of Energy created five federal NSR-dedicated research institutes with annual guaranteed funding for 10 years (20 million dollars),[2] thus encouraging long-term programmes. As the governments of many nations closely observed the research path taken by other nations, many European countries also developed NSR initiatives, as did China.[3] Today Chinese research institutes publish approximately 40% of the Web of Science listed work, the US also publishing about 40%.[4] With the incentive of readily available funding,

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[2]Interview with M. Cohen, conducted by T. Shinn, 5 Feb 2008.

[3]It was Japan first, in the 1990s under the auspices of the MITI, and then the United States that developed extensive, well-organized and long-term policy initiatives and activities in NSR. The term "nanotechnology" was first coined in 1974 by the Tokyo University professor Norio Taniguchi.

[4]For a highly useful scientometric examination of aspects of NSR, see the special issue of the journal *Scientometrics*, *Scientometrics* 7(3), 2007. For a qualitative study of some economic aspects of NSR, and of enterprise interactions, see the special issue on NSR of *Research Policy* 36 (6), 2007, pp. 807–904.

researchers often hastened to modify their work orientation in such a manner as to benefit from the fresh and entirely unanticipated flow of plentiful resources.[5]

In the absence of high levels of public funding it is likely that NSR would not enjoy the degree of expansion it is seeing today. Money speaks with a loud voice. Nevertheless, for many NSR practitioners funding represents a supplementary incentive – an added bonus to the quite independent attractions of a new frontier of artificial, made-to-order materials as objects of research and the call of a revolution in instrumentation.

The Place of Biology

It is in the wake of the enormous expansion of NSR in the years since 2000 that research on biological materials, followed by research that was more strictly biological in character, also expanded rapidly. In many respects this constitutes the vanguard of today's NSR activities. This growth can be witnessed in the expansion of research on DNA and proteins (see Figs. 8 and 9).

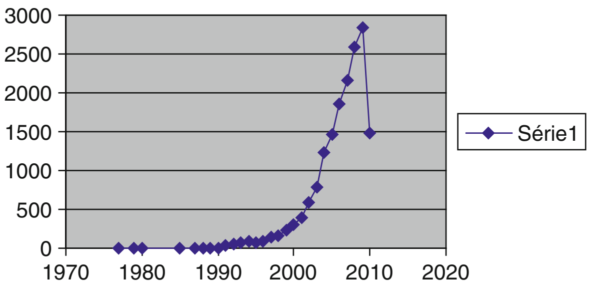

Fig. 8 DNA* and Nano* (topics) = 16,690 records (Consulted on Isiweb 9.07.2010)

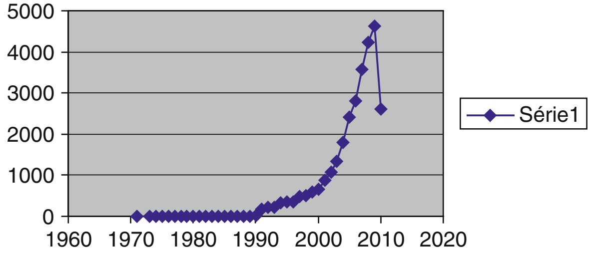

Fig. 9 Protein* and nano* (topics) = 29,300 records (Consulted on Isiweb 9.07.2010)

[5]In France, for example, one can reasonably conjecture that the 2003 intellectual and organizational merger and partial reorientation of four important Parisian laboratories was significantly influenced by recent pro NSR policy and by the money earmarked for specifically NSR endeavours. It resulted in the creation of the Institute de Nanoscience de Paris dealing explicitly with NSR-related themes. One could cite many instances of this kind for many countries.

The Foresight Institute, which annually awards the Feynman prizes, demonstrates an interest in biology through the selected news items that it offers online about nanoscale research: “Researchers use nanoparticles to shrink tumours in mice”,[6] or: “A new spin on drug delivery: Enhanced delivery of DNA payloads into cells.”[7] Looking at the group of scientists awarded the Feynman prize reveals that, since 2002, at least one laureate has always been involved in research linked at least to biological materials and at most to questions related to biology. Some pre-2002 laureates have also been selected for their work with biological materials, such as Ned Seeman in 1995 for developing ways to construct three-dimensional structures, including cubes and more complex polyhedra, from synthesized DNA molecules.

According to some, the nano scale correlates best with biomaterials. Much research on atoms, crystals, and small molecules occurs at the far smaller angstrom range and is not suitable for nano investigations. This is the first reason for the centrality of biological materials. A second reason is associated with the wealth of information contained in biological matter, which infinitely surpasses that of other forms of materials. The third reason is the capacity for self-reproduction inherent to these kinds of materials. Finally, there is the prospect of health applications – a healthier and longer life as an ultimate end.

This growing centrality of biological materials and questions in nanoscale research is linked to at least four main considerations, all of which stress the concept of control: (1) the use of biological materials for studying physical phenomena and their spin-offs; (2) shape, structure, function; (3) sensing and detecting – from physics and chemistry to biology and back; (4) from the micro to the macroscale – a crucial biological concern. We will now examine each of these points, several of which are intricately interlinked.

The Use of Biological Materials for Studying Physical Phenomena and Their Spin-Offs

It is worth noting that many researchers who use biological materials at the nano scale have made this choice for reasons other than that of biological interest. These researchers were interested in control procedures relating to molecules possessing special chemical and physical properties. Emblematic of this approach is the story of Shimon Weiss’s work on spectroscopy and the use of fluorescence resonance energy transfer (FRET) on a single molecule. Weiss, now director of the *Single Molecule Biophysics Group* at UCLA, was first trained in electrical engineering and received his PhD in non-linear optics, and then moved to semi-conductor physics which he studied with optical spectroscopy, at that time initiating exploration at the atomic scale and also of the dynamic phenomena of surfaces. This led him to focus on the study of spectroscopic properties of individual molecules when their environment

[6] http://www.physorg.com/news197887610.html (consulted 12 July 2010)
[7] http://www.physorg.com/news197897133.html (consulted 12 July 2010).

is changed. The question then was to find the good knob on a semi molecule spectroscopy experiment, where Weiss reached the conclusion that by putting another molecule just nearby, it will do the trick.

DNA was of great interest to Weiss and his group because of the almost total control obtained over its synthesis, the length and shape one can impose, and because of the possibilities of adding a dye to it. This research led to the publication of an article which appeared in *Science* in 1998 and has now been cited more than 2,777 times (Bruchez et al., 1998). The challenge faced by the group was to use changes ofa single molecule detected with a single laser for the measurement of the internal dynamics of this molecule, its relative position, and its displacement relative to other molecules. This led them to introduce quantum dots at two different places on the DNA molecule.[8] In this way they were able to measure the "dynamic distance (down to eight nanometers) changes between two sites on a macromolecule (or between two different molecules) via single-pair fluorescence resonance energy transfer (spFRET) by following spectral changes in the emission of a single donor-acceptor pair."[9] This dynamic spectroscopy method for studying molecules has spawned important spin-offs in the field of medical applications, for example in research on biological detection, particularly of cancer (we will return later to the question of detection). The technique is also moving the group toward research on more fundamental biological questions, such as the study of protein folding and transcription at the level of individual molecules.

Shape, Structure and Function

As stated in Section 2.1 above, biomolecules such as proteins and nucleic acids (DNA and RNA) are self-assembling molecules. This property is fundamental to life processes (such as DNA replication) and comprises two dimensions: the intermolecular and the intramolecular self-assembly process. The shape of molecules on which researchers work and which they artificially produce is associated in large part with the intermolecular domain, where biomolecules (usually proteins and DNA) fold in extremely complicated patterns (Marcovich, Shinn forthcoming).

The various attempts to produce artificial shapes that can serve as templates for patterning other molecular shapes draw on these biological materials by reason of their chemical properties and the shape these properties could confer on the molecule, thereby producing, for example, controlled electronic exchanges within the molecule. Another biomaterial property, namely, the strong relation between the structure of a biological molecule (its shape) and its function, has given rise to another strand of research.

A biochemist and biophysicist working with computational programs, Brian Kuhlman (from the University of North Carolina) won the Feynman prize in 2004

[8] Interview conducted by A. Marcovich and T. Shinn, Paris, 18 February 2005.

[9] http://www.chem.ucla.edu/dept/Faculty/sweiss/ (consulted 13 July 2010).

along with biochemist David Baker for their development of the RosettaDesign program. This program enabled the creation of the first protein to be totally artificially constructed and experimentally tested. It proved to have a high success rate in designing stable protein structures with a specified backbone folding structure. The novel backbone structure was found to be extremely stable and to match the predicted structure with atomic level accuracy.[10] Kuhlman and Baker started from the fact that proteins are self-assembling molecules and that they are built as a linear chain of amino acids, after which they then fold into a unique, three-dimensional conformation. This conformation is responsible for the functions, properties and activities of the protein. The idea was to conceive, through simulation and then experimentally, a sequence of amino acids which would result in a protein having a determined form (due to the order in which the amino acid molecules were bound) and whose subsequent function could then be decided. What we have here, then, is a chain of implications which begins with the definition of a determined protein configuration, designed to make it perform predetermined work. The goal is not to construct a device, but rather to acquire mastery and control over matter such that it becomes possible to decide its configurations and therefore its functions.[11]

It is important to note the key role played by simulation in this research. Simulation is a highly sophisticated technique that is emblematic of the most important epistemological trend in nanoscale research: the notion of control of matter and of its processes. The work of Christian Schafmeister (Temple University, Feynman laureate 2005) and of Homme Hellinga (Duke University, Feynman laureate 2004) exemplifies the same orientation, where the computational element is central and the aim is to design proteins according to a desired function. For Schafmeister, the first step was to buy synthesized DNA containing a specific sequence of nucleic acids in order to produce proteins from it that are capable of performing a definite task. The goal here is to understand and then to produce the necessary sequence of amino acids from which the protein will take a definite structure, and then to be able to perform a certain task. "We're trying to develop a way to make molecules that behave and do all the things that biological proteins can do. That means, catalyze reactions, bind big molecules, act as molecular devices, but in a way that is engineerable by human beings. So, we make these building blocks we connect through pairs of bonds, we call them molecular building blocks."[12] These natural protein properties that are artificially reproduced entail a sequence of operations beginning with the decision to build a protein capable of performing a certain function. The problem is the reverse of Kuhlman's perspective: Kuhlman first designs the shape of the molecule and deduces its functions from this. Schafemeister wants the molecule to have definite chemical properties (catalysis of a particular reaction, or cleaving a definite bond, etc.). He then determines what chemically reactive groups will be

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[10] http://www.nanotechwire.com/news.asp?nid=1242&ntid=116&pg=80 (consulted 13 July 2010)

[11] Interview with B. Kuhlman conducted by A. Marcovich and T. Shinn, 10 June 2009.

[12] Interview with C. Schafmeister conducted by A. Marcovich and T. Shinn, 18 June 2009.

necessary and where to position them so that they carry out that function efficiently. Schafmeister has designed fourteen small molecules, each of which is about half a nanometer across and includes two removable molecular caps. Controlled chemical reactions strategically strip away the caps, causing the molecules to link together in predictable ways with pairs of stiff bonds – similar to Lego blocks. He has snapped together 3.6 nm rods and 1.8 nanometer crescents, and has developed software that can aid in the construction of a wide variety of shapes. These nanofabricated molecules can serve as chemical sensors.[13]

One of the challenges the Schafmeister team had to face was to produce designed proteins in quantities big enough so that they can be used in medicine as chemical sensors. Schafmeister insists that his work should be application-oriented. As we shall see in the next subsection, the problem is to sense and detect nanoscale objects using designed molecules. Proteins are one of the most important application spin-offs of nanoscale research on biological materials.

Sensing and Detecting – from Physics and Chemistry to Biology and Back

Hellinga's work on proteins is clearly oriented from the beginning towards this sensing-detecting approach. Interestingly, the work begins on a conceptual plane and then moves to computational design in order to embody the concept of some protein. This computational work results in a series of predictions, that is, in a series of mutant proteins which are then produced in the laboratory and whose properties are then sometimes studied again and adjusted using simulation programs.[14]

The aim of the research is to design proteins from the outset with pre-specified functions, for example, to specify proteins in order to make them able to bind to small selected molecules, and thus to give them the capacity to act as receptors or as sensors. There is a very clear user base here, in particular in the area of clinical application, but also (not surprisingly) in military applications: these sensors can detect explosives and nerve agents.

Other types of research aimed at conceiving and building sensors are conducted using many other kinds of materials and approaches. The work of James Gimzewski, first trained in physics and now professor in the department of chemistry and biochemistry at UCLA, offers a good illustration of this point. He won the Feynman nanotechnology prize in 1997 for having realized a molecular abacus using buckyballs (60 carbon atoms). Over the last 10 years, Gimzewski has undertaken research in biophysics which he calls sonocytology. The idea here is to create tiny micromechanical drums made of silicon nitrite. These are not molecular in scale but are covered with molecular layers which he deposits on the surface of living cells. These vibrations, once amplified using computer software, create an audible sound.

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[13] http://www.eurekalert.org/pub_releases/2005-11/uop-pps111005.php (consulted 13 July 2010)

[14] Interview with H. Hellinga conducted by A. Marcovich and T. Shinn, 9 June 2009.

It was discovered that cancerous cells emit a slightly different sound than healthy cells. Gimzewski and his colleagues hope that sonocytology may someday have applications in early cancer detection and diagnosis, and that it will have multiple applications in the consumer sector, in areas of health, as well as in other fields.[15]

As in the case of Weiss's research on FRET and his endeavours to implant quantum dots on DNA, this is highly sophisticated research involving the control of material, in which non-organic bodies are introduced into biological molecules. This combination of materials and competencies is paramount for the underlying epistemology of control in nanoscale research, where biology is a privileged field of reflection. It is equally crucial to see that the choices made by physicists or chemists to work on biological materials can often be seen as a continuation of their research on topics of interest within their own discipline.

For example, Marvin Cohen and Steven Louie,[16] working on the friction of telescoped nanotubes (one inside the other), realized that the smaller tube is so small that it could sit on the back of a virus. It thus drew the interest also of scientists working on molecular motors with biological implications.[17] As stressed above, in biology the function of a molecule derives from its structure and shape. The nanoscale is the scale at which fundamental physical and chemical processes can be linked to the shape of the object studied. In this perspective, biology and life processes play the role of models for reflection in other domains. This is how physicist Gimzewski puts it:

> Actually, I'm inspired by biology. Biological inspiration means, for me, that what I make is not biological necessarily, but I have to look at biology to understand how to make something small on that scale, you see. I can't differentiate biology from non-biology. I certainly am inspired by looking at the cantilever bridges of the past. I'm inspired by looking at old machines. But that's not going to help me work in this area on the nano scale. Nature is the master of the nano scale, so it's worthwhile to look in and see what you can pick up.
> How does the coral have that form? And it's just an incredible combination of processes on an atomic scale, and then on a molecular scale. Then there are diffusion processes on the micron scale. But the scales go and go until you come to the scale of the ocean current. The end result has to do also with the global warming—the form of that coral, a shell. All of these shells, they have this incredible—bio-inspired people who looked at shells and thinking, "how can we make a material like a shell?" The military would like to make some bulletproof vests like this.[18]

When physicists or chemists turn their attention to biological materials and medical implications, they sustain their primary disciplinary identity and continue to

[15] Interview with J. Gimzewski conducted by T. Shinn, 22 January 2008.

[16] Drs. Marvin L. Cohen and Steven G. Louie of the University of California at Berkeley, Department of Physics, received the theoretical prize for their contributions to the understanding of the behavior of materials. Their models of the molecular and electronic structures of new materials predict and understand properties such as structure, surface conditions, and interactions with other materials. Many of these predictions have since been confirmed experimentally; see http://www.nanotech-now.com/Foresight-release-10152003.htm (consulted July 14, 2010).

[17] Interview conducted by T. Shinn, 29 January 2008.

[18] Interview conducted by T. Shinn, 22 January 2008.

reflect with reference to it. Nevertheless, it is worth interpreting physicists' interest in biology also as a symptom of what the scientists themselves claim: with nanoscale research, biology becomes physics. This is what biochemist Hellinga expresses, for example, when he says that "electron transfer is fundamental to all of biology. Mary Shelley said that life is electricity. She was right. Just a little detail that was brought to the light bulb. But the flow of electrons in biological systems is what life is all about. It's the great source of energy in life."[19] Working at the nano scale, it becomes possible to relate a physical phenomenon such as a redox reaction (oxidation reduction reaction) to the structure, shape and activity of a molecule.

These considerations highlight how biology opens the way to new perspectives in research at the nano scale. The question remains: what does the nano scale bring to biology?

From the Micro to the Macroscale – A Crucial Biological Concern

One of the most fundamental questions in biology concerns the link between processes and phenomena taking place at the molecular level, and the functions, processes and evolutions involving an organism as a whole. Will the understanding of this link explain the origin of life?

The question of the relationship between the shape and function of biomolecules opens the way to investigation of this vast theme. To cite one of the scientists interviewed: "How does one unit talk to the next, and the next, and the next and so forth. . .?" Or, to cite another:

> In biology in general actually, we are dominated by a triangle. The triangle connects structure, sequence, and function. They're almost inextricably linked. That is to say, the sequence determines what the structure of a protein will be. If you change the sequence, you will change the structure. The structure of a protein determines what its function will be . . . It's like a jigsaw puzzle. If you change the shape of a puzzle piece, that will change how it's connected to each other. That will manifest itself when two proteins come together, they form a complex, a diamond.

Furthermore, this biological jigsaw puzzle is a non-linear system with feedback processes. Non-linear systems in general are non-predictable and chaotic. However, as biophysicist David Bensimon (Laboratoire de physique statistique de l′Ecole Normale Supérieure de Paris) stresses,[20] contrary to these general systems and despite the fact that these feedback loops are non-linear, biological systems are predictable and relatively stable. This is probably a consequence of evolution, he says. And here he understands evolution both from a general point of view of living systems through the ages and from the point of view of the more limited time-scale of cancer research:

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[19]Interview conducted by A. Marcovich and T. Shinn, June 2009.

[20]Interview conducted by A. Marcovich and T. Shinn, 12 June 2008.

> One of the questions here is that cancerous cells mutate in huge quantities, . . . they mutate and they evolve very rapidly. They have lost their inhibiting control system. If you give a medication in order to kill these cells, the problem is that if these cells mutate, the cancer will become resistant and re-develop and re-initiate cancerization of the organism. Our question is, what are the mechanisms involved? How do these cells adapt to new conditions? On a molecular level what are the events which contribute to these changes?[21]

It is interesting to note that, contrary to many other research fields at the nano scale, the environment is conceived here as part of the problem situation. The question is to understand how a system as a whole functions and adapts to its environment and to new conditions. The environment can be the surrounding molecules, such as proteins to other proteins, or the protein matrix in which the redox reaction takes place, or again the cell considered as a milieu, or the physical conditions to which the whole system is subject – temperature, for example. These are all analysed as relevant parameters for understanding the functioning of the system under consideration. The quality of these relations is part of the question being explored. Take, for example, the sensors which are placed on a molecule to detect reactions within a cell. This very complex task integrates the structure, shape, properties, functions and behaviour of molecules in their environment and the functioning of systems as a whole, be it a DNA-protein relation, a cell or a living organism. The process raises two issues: on the one hand, the question of genetic codes responsible for the synthesis of proteins; and, on the other, the relation between determinist and stochastic views and, through this, the relationships between the macro level and the micro level.

One can see in all this the fundamental question raised by Erwin Schrödinger, whose *What is life?* (Schrödinger, 1944) greatly influenced the birth of molecular biology.[22] In his preface to the French version of the book, the French mathematician and biologist André Danchin summarizes Schrödinger's question in terms of complementarity: "How can we connect the swarming of atoms to the movement of cells? The whole question is to give structure and shape to this swarming" (Danchin, 1992).[23] This evokes once more the relationship between predictable behaviours and reactions in living organisms and chaotic feedback loops which occur at the atomic and molecular level. Announcing his principle of "order-from-disorder", Schrödinger stated that most physical laws on a large scale are originate from chaos on the small scale. Life greatly depends on order, and he assumes that the master code of living organisms depends on a large number of atoms.

Schrödinger believed the material of heredity to be a molecule which, unlike a crystal, does not repeat itself. He calls this an aperiodic crystal. Aperiodic nature makes it possible to encode an almost infinite number of possibilities using a small number of atoms. But the term "code" seems to him too narrow. He believes that

[21] Interview with D. Bensimon conducted by A. Marcovich and T. Shinn, 12 June 2008, translated from French.

[22] His book was based on a course of public lectures delivered under the auspices of the Dublin Institute for Advanced Studies at Trinity College, Dublin in February 1943.

[23] This is freely translated from the French.

chromosomic structures are equally useful for performing the development that they symbolize. They are both the law code and the executive power, both the architectural design and the work of the entrepreneur.

The different issues highlighted in Schrödinger's reflections point to the very strong links between the concepts of information, code, order, stability and durability of living organisms on the one hand, and the statistical laws in which everything is grounded on the other. Thus Schrödinger places emphasis on this constant and necessary back-and-forth between local and more global dynamics which sustain the creation of stable macroscopic forms. This deterministic-stochastic relation between the atomic and molecular scale and the macro scale assigns a central position in nanoscale research to the problem of forms and structures. Whatever level and associated functions one considers (the whole organism, organs, all the way down to cells and proteins with their DNA sequences, etc.), shapes and structures are understood to be the scaffolding of functions in life processes. And here, a last point could be added: the simulation technique which is at the heart of nanoscale research could well be the tool which facilitates understanding of the passage between the macro and micro and between the stochastic and the deterministic levels.

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Part IV
Science, Values and Society: Economic, Political and Public Relations of Research

Bringing the Marketplace into Science: On the Neoliberal Defense of the Commercialization of Scientific Research

Justin Biddle

It is no coincidence that our present troubles parallel and are proportionate to the intervention and intrusion in our lives that result from unnecessary and excessive growth of government.... In the days ahead I will propose removing the roadblocks that have slowed our economy and reduced productivity.... Progress may be slow, measured in inches and feet, not miles, but we will progress. It is time to reawaken this industrial giant, to get government back within its means, and to lighten our punitive tax burden. And these will be our first priorities, and on these principles there will be no compromise.

– Ronald Reagan, First Inaugural Address (January 20, 1981)

We are currently witnessing profound changes in the way in which scientific research in the United States is organized. In 1964, 30.8% of U.S. R&D was funded by industry, while 66.8% was funded by the federal government. The years between 1964 and the present have witnessed an almost exact reversal; in 2004, 63.8% of national R&D was funded by industry, while only 29.9% was funded by the federal government (National Science Board, 2006, 4–12). Not only is industry funding more research, but the boundaries between business, on the one hand, and government and university research, on the other, are becoming ever more blurry. For-profit corporations are increasingly funding university research.[1] The number of patents taken out by U.S. universities is rising dramatically, more than doubling between 1979 and 1984, more than doubling again between 1984 and 1989, and more than doubling again in the 1990s (Nelson, 2001, 13). University-operated technology

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This paper developed out of my participation in the research group, *Science in the Context of Application*, at the Center for Interdisciplinary Research (ZiF) at Bielefeld University. The paper benefited greatly from the comments of the organizers of the group, Martin Carrier and Alfred Nordmann, and from the rest of the Fellows, especially Torsten Wilholt.

[1] For example, in 1984, approximately 46% of life science companies supported university research; by 1994, the percentage had risen to approximately 92% (Blumenthal et al., 1986, 1996).

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transfer offices, which assist universities in filing patent applications and in licensing patents to private corporations, numbered only 25 in 1980; they now exist on virtually every U.S. research campus (Nelson, 2001, 13). These offices, moreover, are increasingly affecting the research activities of university professors (e.g., Kleinman, 2003, 133–136). Growing numbers of both university and government scientists are developing financial relationships with private corporations, and many university scientists are starting their own companies, simultaneously playing the roles of academic researcher and entrepreneur.[2] The result of these changes is that commercial considerations are exerting increasing influence within the practice of science, in both universities and governmental laboratories.[3]

There are many who applaud this trend. Henry Etzkowitz, for example, defends what he calls the "assisted linear model of science and innovation policy," according to which close cooperation between federal funding agencies, universities, and for-profit corporations will result in continued scientific success coupled with more effective translation of scientific results into marketable products (Etzkowitz, 2006).[4] Etzkowitz's line of reasoning, moreover, is echoed by many within university administration. For example, Gordon Rausser, the Dean of the College of Natural Resources at the University of California at Berkeley from 1994 to 2000 and the architect of the much-discussed Berkeley-Novartis deal, argues that "the University's mission requires us to contribute to the state's economic growth and to facilitate the transfer of good ideas into private commerce"; extensive cooperation between universities, government, and industry, he maintains, is a highly effective way of doing this (Rausser, 1999). Yet, defenders of the commercialization of science are often unclear about precisely how closer cooperation between universities, government, and industry is supposed to result in these benefits. Why is it that the introduction of market values into the practice of science will supposedly result in continued scientific progress coupled with stronger economic growth? Why won't it instead result in a degradation of the quality of research, a sacrifice of epistemic standards at the altar of profit?

The aim of this paper is to identify and evaluate the theoretical justification for the commercialization of science. To do this, I examine the arguments put forward by one of the most prominent early proponents of commercialization, George Keyworth II. Keyworth served as Presidential Science Advisor to Ronald Reagan and Director of the White House Office of Science and Technology Policy from 1981 to 1985, the period during which numerous pieces of Congressional legislation encouraging extensive university-industry collaboration were passed. While best

[2] According to the editors of the *New England Journal of Medicine*, it is now almost impossible to find biomedical scientists to write review articles who do not have financial conflicts of interest with industry (Drazen and Curfman, 2002, 1901).

[3] The commercialization of science is also having a profound impact upon the ways in which industrial research is conducted. For discussion, see Mirowski and van Horn (2005).

[4] The linear model refers to Vannevar Bush's argument in *Science: The Endless Frontier* (1945), according to which generous government funding for basic scientific research will lead inevitably to technological progress. The "assistance" in Etzkowitz's assisted linear model comes in the form of the expertise of industry.

known as a staunch defender of Reagan's Strategic Defense Initiative, or "Star Wars," Keyworth was also a major proponent of developing closer ties between the private sector and what he called "the basic research establishment," including both universities and government laboratories (Keyworth, 1983a, 1123). One of his tasks as Science Advisor was to defend these ties to the scientific community, a task that he carried out in a series of articles in the journal *Science* in the early 1980s (Keyworth, 1982; 1983a, b, 1984).

An examination of Keyworth's arguments reveals the profound role that neoliberal political and economic thought played in his defense of the commercialization of science. Keyworth argued that Reagan's science and technology policy would stimulate economic growth by reorganizing scientific research along neoliberal lines. More specifically, by expanding the domain of voluntary exchange in which scientists operate – i.e., by removing the government-imposed barriers between scientific research and the marketplace – this policy was supposed to facilitate the flow of information between sectors that were previously cut off from one another, thereby encouraging the sharing of expertise and the transfer of scientific research into marketable products. The end result, Keyworth claimed, would not only be high-quality science, but also improved technological development, economic growth, and ultimately, social progress. In the second part of this paper, I will argue that there are strong reasons to question this conclusion.

In arguing that the model that Keyworth defended is an outgrowth of the neoliberalism that characterized the political and economic policies of the Reagan Administration, I am not arguing that the Reagan Administration is solely responsible for the commercialization of scientific research or that commercialization represents a kind of sudden break that occurred in the early 1980s. As many scholars have pointed out, there were policy initiatives that occurred well before the 1980s that can be seen as beginning to pave the way toward the organizational changes that have now transformed the practice of science (e.g., Nelson, 2001). What I am arguing is that the particular form of commercialization that we are now witnessing is grounded in a particular political and economic viewpoint – one that characterized the policies of the Reagan Administration but that also had been gaining support since at least the mid-1960s – and that there are strong reasons to question the wisdom of this form of commercialization.

As a final preliminary note, I restrict my discussion of the commercialization of research to those organizational shifts that are transforming scientific research in the U.S. Whether there are alternative institutional means of encouraging closer university-government-industry relations that avoid the problems discussed in this essay is an issue that is beyond the scope of this paper.

Keyworth on Science and the Economy

The 1970s was a period of economic decline in the U.S. While the oil crisis of 1973 played a significant role in this downturn, the U.S. was also facing increasing economic competition from abroad, especially from Japan and West Germany. The period of unrivaled technological dominance that the U.S. had enjoyed since the end of WWII was now coming to an end. While the causal story behind this decline is complex, there were many who maintained that a primary cause was an outdated science and technology policy.[5] Many within the Reagan Administration, including Keyworth, held this view. In articulating and defending the Reagan Administration's science and technology (S&T) policy, Keyworth was keen to draw a connection between S&T policy and economic growth. For example, in an essay entitled, "The Role of Science in a New Era of Economic Competition," Keyworth wrote:

> No conference on federal R&D priorities can ignore the overriding significance of our country's economic condition. It is the dominant factor in virtually all deliberations on policy issues at the White House. . .. In thinking about R&D we have to consider more than what kinds of science and technology we can afford in today's economy. It is more important to consider the reverse. How can science and technology help the economy? (Keyworth, 1982, 606, 608)

Given the country's economic woes, stimulating economic growth was one of the most urgent goals of the Reagan Administration, and it viewed its S&T policy as an important tool for accomplishing this.

But how was an S&T policy supposed to do this? After all, the predominant view among conservatives during this period was that the country's economic ills resulted from high taxes, runaway federal spending, and excessive government intrusion into the private sector; as Reagan famously stated in his 1981 inaugural address, "In this present crisis, government is not the solution to our problem; government is the problem" (Reagan, 1981). If government was the problem, how could a new S&T policy be a part of the solution? The answer to this is that the neoliberal worldview that underpinned Reagan's suspicions of government also influenced his administration's S&T policy. In particular, defenders of this policy maintained that the key to developing new and innovative technologies was more extensive contact between the private sector and the basic research establishment. For example, in an article entitled "Federal R&D and Industrial Policy," Keyworth wrote:

> American technological progress suffers badly from the artificial barriers between industry and the bulk of the basic research establishment. Most academic and federal scientists still operate in virtual isolation from the expertise of industry and from the experience, and guidance, of the marketplace. One can make a convincing case that this separation is a root cause of our sluggishness. . . in turning research into products (Keyworth, 1983a, 1123).

One of the primary aims of Reagan's S&T policy was to remove these "artificial barriers" and end the enforced isolation of the basic research community from "the reality and stimulation of the marketplace" (Keyworth, 1983b, 1124).

It should be clear that the Reagan Administration's S&T policy did not amount to a whole-scale privatization of science – i.e., the complete removal of government

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[5] See, for example, Rosenzweig (1984, 42–43). In addition, those who pushed for legislative changes to American S&T policy, such as Birch Bayh and Bob Dole, justified the need for such changes by appealing to the deteriorating condition of the U.S. economy (Washburn, 2005, 60). The Bayh-Dole Act is discussed in Section "The Bayh-Dole Act".

from the S&T enterprise. Government, Keyworth argued, has an essential role in funding basic research, which in turn can be drawn upon for the development of various technologies (Keyworth, 1983b, 1124). Moreover, in the realm of military research, the administration allowed the government an important role in influencing the directions of scientific research, as well as in purchasing the products of military research. Rather than removing government from the equation, Reagan's S&T policy was intended to restrict in significant fashion the role of government in scientific research and, with the exception of military research, to allow market forces to guide research. In other words, while government would play a role within R&D, it would play a "properly limited role. . ., one that makes sense for a free-enterprise economy" (Keyworth, 1982, 607).

Reagan's S&T policy retained crucial features of Vannevar Bush's linear model of science and technology. Bush maintained that there was a linear relationship between basic research, technological development, and social progress, such that significant government funding for basic research would lead inevitably to technological progress, which in turn would lead to social progress (Bush, 1945). Reagan's S&T policy maintained an important role for government funding of basic research; it differed from the Bush model, however, by denying that progress in basic research would lead inevitably to technological progress. What was needed in order to accomplish this transition, Keyworth argued, was "the experience and guidance of the marketplace" (Keyworth, 1983a, 1123). The policy that Keyworth defended is thus in many respects similar to Etzkowitz's assisted linear model; it requires extensive government funding of basic research, but also assistance – in the form of market values – in translating research into commodities.

The Bayh-Dole Act

But how, in more specific terms, was the experience and guidance of the marketplace supposed to be brought into the basic research establishment? The answer to this can be found by examining a piece of legislation that helped to encourage greater university-industry collaboration, namely the U.S. Congressional Patent and Trademark Amendments Act of 1980 – more commonly known as the Bayh-Dole Act.[6] Prior to the Bayh-Dole Act, it was by and large the case that the results of federally funded research remained in the public domain, while the results of privately funded research could be privately owned. In some cases, it was possible for universities to obtain patents on federally funded research, but only after a process of receiving special approval. The Bayh-Dole Act changed this, allowing universities

6 The Bayh-Dole Act and the legislative initiatives that followed it have been discussed extensively elsewhere. For a discussion of the Bayh-Dole Act and the Congressional debate leading up to its passage, see Washburn (2005, 60–69). See Slaughter and Rhoades (2004, Chapter 2) for a discussion of additional pieces of legislation.

and private corporations to obtain patents on the results of federally funded research, without going through any special approval process.[7]

A couple of examples will help to illustrate the implications of this act. The first is the much-discussed agreement, signed in November 1998, between the University of California at Berkeley and the Swiss pharmaceutical company, Novartis.[8] According to this deal, Novartis gave $25 million over 5 years to fund research in the Department of Plant and Microbial Biology in exchange for receiving first right to negotiate licenses on approximately one third of the patents generated by department research, including research funded by the public. In addition, Novartis received a say in how its funding would be distributed; two out of the five members of the research committee – the committee that made funding decisions – were company representatives. Faculty researchers who participated in the cooperation would receive access to Novartis's proprietary databases, on the condition that they signed confidentiality agreements in which they agreed not to publish any findings without the consent of the company.

One potential outcome of this deal – an outcome that Novartis certainly hoped to see realized – was that university research would lead to patents that would be owned by the university and then licensed to Novartis.[9] Depending upon the negotiations, moreover, the patents could be licensed exclusively to Novartis. In this situation, Novartis would obtain exclusive rights to the results of this research, *even if the research was funded by the public*. The Bayh-Dole Act made such situations far easier to realize, and it is for this reason that U.S. Senator Russell Long, one of the most prominent critics of Bayh-Dole, called the act one of "the most radical and far-reaching giveaways I have ever seen" (quoted in Washburn, 2005, 61).

One example of precisely the kind of scenario that Long feared concerns the sequencing in 1994 of BRCA1, a gene that is associated with breast cancer, by a team of scientists led by Mark Skolnick at the University of Utah.[10] (They later sequenced BRCA2, which is also associated with breast cancer.) The funding for the research on BRCA1 came from a mix of public and private sources, including the

[7] As originally passed, the bill allowed only universities and small businesses to obtain title to research that was funded by the public; large corporations were specifically excluded, in part to appease critics of the legislation. Shortly after the passage of the original bill, an attempt was made to extend the bill to include large corporations, but it failed to make its way through Congress. It was only through a 1983 Presidential Memorandum by Reagan, which directed executive agencies to extend the policy to large corporations, that this extension took place. A 1987 Executive Order eventually made the extension permanent (Washburn, 2005, 60–69).

[8] The Berkeley-Novartis deal is discussed in a number of places, including Krimsky (2003, 35–39) and Washburn (2005, Chapter 1). For a defence of the deal by Robert Berdahl, the Chancellor of Berkeley from 1997 to 2004, see Berdahl (2000).

[9] It is worth noting that the 5-year deal between Berkeley and Novartis was not renewed. While the reasons for this are complex, the fact that the deal was not, in the end, financially profitable to Novartis, as well as the fact that the university received intensive negative publicity from the deal, no doubt contributed to the decision.

[10] The story of the sequencing and patenting of BRCA1 and BRCA2 is told in Dalpé et al. (2003).

NIH (roughly $4.6 million) and Myriad Genetics, a start-up company co-founded by Skolnick (roughly $10 million). Shortly before publishing their results, Skolnick and others applied for patent protection and entered into an agreement with Myriad Genetics, giving the company an exclusive license to test for the gene. Once the patent was granted, Myriad received a monopoly upon the test for the BRCA genes, even though a significant portion of the research that led to the discovery was publicly funded.

The purpose of the Bayh-Dole Act was to encourage the kinds of public/private cooperation that are evident in the Berkeley-Novartis deal and the Myriad Genetics case. In emphasizing this Act, I do not mean to suggest that it alone was sufficient for creating the extensive forms of collaboration that currently exist between universities, the federal government, and private corporations. This collaboration is the result of a large constellation of events, including other pieces of congressional legislation (e.g., the Stevenson-Wydler Technology Innovation Act of 1980 and the Economic Recovery Tax Act of 1981),[11] legal decisions (e.g., the 1980 U.S. Supreme Court decision, *Diamond v. Chakrabarty*),[12] and international trade agreements (especially the Trade Related Aspects of Intellectual Property Rights, or TRIPS, agreement of 1994). The Bayh-Dole Act was, however, important in paving the way for many of these additional events, and it illustrates well the kinds of collaboration that the Reagan Administration's S&T policy encouraged.

The Road Not Taken: Japan and the Planning of Science

Thus far, I have discussed Reagan's S&T policy and contrasted it with Bush's linear model. The linear model, however, is not the only alternative way of organizing research. Since at least the 1930s, a number of commentators have argued that government should not only play a role in funding research but also in determining which problems to address – an idea that was anathema to Bush, who maintained that the scientific community alone should have control over how funds are used (e.g., Zachary, 1997, 232–234). An important element of the government-planning model was a strong emphasis upon applied research directed at specific social problems. The British socialist and physicist J. D. Bernal was an important proponent of such a model in the 1930s and 1940s (Bernal, 1939), and Harley Kilgore, the U.S. Senator from West Virginia, defended a version of this approach in the 1940s and 1950s.[13]

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[11] See Slaughter and Rhoades (2004, Chapter 2) for discussion.

[12] See Kevles (1998) for discussion.

[13] Michael Polanyi and Bush were highly critical of the claims of Bernal and Kilgore, respectively, that the government should play a role in the planning of science. Both Polanyi (1962) and Bush (1945) maintained that scientists alone should decide which areas of research are pursued. See McGucken (1978) for a discussion of the Bernal-Polanyi debate and Zachary (1997, 232–234) for a discussion of the Bush-Kilgore debate.

As a part of his defense of the Reagan Administration's S&T policy, Keyworth was keen to denounce the government planning of research. This topic was especially important, because Japan had adopted a kind of government planning model and had subsequently become very successful at developing marketable new technologies. While the U.S. had been spending considerable sums of money on basic research and was ostensibly seeing relatively little payoff in terms of economic success, Japan was spending relatively little on basic research, was allowing a prominent role for the government in determining research directions, and was achieving significant technological development and economic growth. Given the apparent success of the Japanese model, one possible solution to America's economic woes would have been to follow Japan's lead; Keyworth, however, rejected this strategy, and his reasons for doing so provide important insight into his support for greater university-industry collaboration.

One of Keyworth's criticisms of the Japanese model was its emphasis upon applied over basic research. Like Bush, Keyworth argued that technological development requires a stockpile of basic research from which to draw. On this view, Japan's economic success depended upon previously existing wells of basic research; in failing to replenish those wells, Japan was setting itself up for a technological drought and thereby putting its long-term economic growth at risk (Keyworth, 1982, 608).

More importantly, however, Keyworth was very skeptical about the government's ability to plan research successfully; in particular, he maintained that the government planning of research stifles individual freedom and creativity. On his view, the linear model fosters individual creativity by creating structures of governance that allow individual scientists a high degree of freedom – for example, freedom to choose the kinds of problems that they address. In so doing, systems of basic research provide uninhibited spaces that nourish creativity and encourage the expression of individual genius. Government plays a role in this system, but not an active one; scientists themselves decide which problems to pursue and how to pursue them, while government merely provides the funding that makes the research possible. An S&T policy such as Japan's, however, stifles the expression of individual creativity. "Japanese leaders," Keyworth asserted without argument, "are... worried about their relative inattention over the years to basic research and to the encouragement of creativity in general"; how well the Japanese system "will continue to flourish without fresh creative input is an interesting question" (Keyworth, 1982, 608).

Keyworth did not stop there. He took aim not only at Japanese S&T policy but also at the collectivist attitudes that he believed informed this policy, and that he took to be prevalent in Japanese culture more generally. He quoted approvingly a recent article in the *New York Times*, entitled "Japan Struggling with Itself," which asserted that Japan's "emphasis on community, obedience, and uniformity, all of which have been crucial to its highly efficient assembly lines, has discouraged individual creativity and, with it, far-reaching product inventions" (Lohr, 1982, quoted in Keyworth, 1982, 608). The collectivist attitudes that "brought so much success to Japan's carefully planned and integrated industries," Keyworth asserted, "now threaten the country's industrial future" (Keyworth, 1982, 608).

Science in a Neoliberal World

Thus far, Keyworth has argued that neither Bush's linear model nor a government-planning model, such as that of Japan, is acceptable. What is needed, on his view, is a model that continues to encourage individual creativity and freedom – as the Bush model does – while at the same time spurring economic development. The right model for this task, he claimed, is the assisted linear model – a model that introduces market norms into the practice of science, such as was done by the Bayh-Dole Act, in order to facilitate the commercialization of scientific results.

At this point, the obvious question to ask is how the assisted linear model can accomplish both of these tasks. While it is at least plausible to believe that the introduction of market norms into scientific research would be a boon to industry,[14] it is much less clear that this would result in good science, epistemically speaking. Why wouldn't the introduction of market norms into scientific research degrade the quality of research by encouraging scientists to put profits ahead of epistemic rigor? Why wouldn't it compromise the freedom of scientists by forcing them into areas of research that have the potential to be profitable, rather than allowing them to decide for themselves which questions to investigate?

Keyworth did address these issues, albeit very briefly. He maintained that extensive university-industry collaboration would benefit scientists involved in basic research by opening them up to the "expertise of industry." Greater collaboration, he continued, is

> not simply a matter of industries 'buying' research from the campus; the process involves a sharing of ideas and of people. . .. There is going to be an extremely active and intellectually stimulating interface developing between some universities and some industries. We are already seeing examples of this in biotechnology (Keyworth, 1982, 609).

As is suggested by Keyworth's defensive assertion that greater university-industry collaboration is "not simply a matter of industries 'buying' research," he was well aware that many university scientists were hesitant or unwilling to build extensive ties to for-profit corporations. He attributed this unwillingness, however, to either a failure of scientists to understand their role as servants of society – where serving society meant doing defense-related work or furthering economic growth[15] – or a failure to understand the various benefits that come from greater collaboration. With regard to the latter, for example, he mused: "I am always puzzled that so much of the academic research community has failed to notice how successful and mutually beneficial those industrial interactions have proved to be" (Keyworth, 1983b, 1123).

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[14] There are reasons to question even this claim. See Section "The Anticommons" for discussion.

[15] In this vein, Keyworth writes: "In February I wrote an editorial [Keyworth (1983a)] in which I addressed the notion that federal support for R&D is an entitlement, that it is going to come off the top of the budget independent of economic pressures or national priorities. If my message in that editorial seemed harsh, it is because I see that attitude as being destructive for science and for the nation. The research community has an important role to place in this country's future, but it has to come to grips with the realities of the 1980s" (Keyworth, 1983b, 1123).

Keyworth's remarks on this issue are unsatisfying. He suggests that the commercialization of scientific research will be (1) "intellectually stimulating," in that it will facilitate the flow of information and result in greater "sharing of ideas" and (2) that it will do this while maintaining scientific freedom. Unfortunately, he provided little justification for these claims. Is there a way of making sense of the view that commercialization will have these effects?

While Keyworth does not address this question explicitly, a central component of the theory underlying his view is what one might call the "neoliberal perspective on social organization," according to which the most effective way of organizing society is to ensure that exchanges between individuals are voluntary – and, in particular, to ensure that government intervenes as little as possible in these exchanges. More specifically, neoliberalism maintains that systems of voluntary exchange, or free markets, are ideal information transmitters that maximize individual freedom.

The view that systems of voluntary exchange are ideal information transmitters is motivated by the arguments against socialism given by Ludwig von Mises and Friedrich A. Hayek. Von Mises argued that a free market is the only system of organization that can ensure economic efficiency and social stability, because free markets, via prices, are the most efficient means of informing producers about the values of different products and consumers about the kinds of products that are available to them (Mises 1935). On von Mises's view, the reason for this is a practical one: it is practically unfeasible for a socialist planning board, or some other centralized agency, to make the calculations required for determining the values of different products. Hayek went further, arguing not only that socialism is practically unfeasible, but also that it is premised upon an epistemological error (Hayek, 1988, 7). Drawing upon Polanyi (1958), Hayek argued that economic efficiency and social stability require knowledge that is tacit and local. For example, the determination of whether there is a market for a given product often requires local and tacit knowledge about how a particular social group would respond to a particular product, and as Polanyi argued, many aspects of both research and development require knowledge that is tacit. But local, tacit knowledge is also non-propositional, and hence it is precisely the kind of knowledge that centralized planning agencies are incapable of possessing. According to Hayek, only a free market can "possess" and distribute the required information; "there is no known way, other than by the distribution of products in a competitive market, to inform individuals in what direction their several efforts must aim so as to contribute as much as possible to the total product" (Hayek, 1988, 7).

The claim that systems of voluntary exchange are ideal information transmitters is an important premise in the justification of the commercialization of scientific research. It suggests that expanding the domain of voluntary exchange – or, to put it another way, "deregulating" science by providing scientists with the freedom to enter into cooperative exchanges with industry – will enhance the flow of information between sectors that previously were separated by walls of bureaucratic red tape. In particular, such deregulation will encourage scientists engaged in basic research to enter into dialogue with investors and product developers. Scientists who work in areas that are commercially relevant will now know that they are doing so, and they will be motivated to work with businesses to bring the results of their research into the marketplace. Corporations, on the other hand, will develop closer ties to scientists, and will consequently develop a better understanding of the sciences that are related to their products and will build relationships that could be financially fruitful.

What about the claim that commercialization will not compromise scientific freedom? There are a couple of reasons that the neoliberal account provides in this regard. The first concerns the nature of freedom – and, more specifically, that freedom should be understood in purely negative terms, as freedom from external obstacles, rather than in positive terms, as freedom to act in particular ways or achieve particular results.[16] If one interprets freedom in purely negative terms, then it is at least plausible to argue that commercialization increases scientific freedom, in that it removes obstacles that, prior to the Bayh-Dole Act, impeded the ability of scientists to enter into potentially fruitful exchanges with industry.

A second reason stems from the neoliberal view of the relationship between free markets and political and economic power. More specifically, neoliberalism maintains that systems of voluntary exchange ensure that political power does not become dangerously concentrated, because free markets distribute economic power over all individuals, which in turn provides a check upon the growth of political power. As Milton Friedman writes:

> Economic arrangements are important because of their effect on the concentration or dispersion of power. The kind of economic organization that provides economic freedom directly, namely competitive capitalism, also promotes political freedom because it separates economic power from political power and in this way enables one to offset the other (Friedman, 1962, 9).

With respect to science, it is the tendency of free markets to serve as a check upon political power that supposedly ensures that commercialization does not inhibit scientific freedom. In other words, by expanding the domain of voluntary exchange, commercialization reduces the power of government over individual scientists, thereby ensuring their scientific freedom.

Keyworth, again, does not draw explicitly upon the neoliberal perspective on social organization in his defense of the assisted linear model. There are, however, reasons for maintaining that this theory was in the background. It is well known that the policies of the Reagan Administration were heavily influenced by the views of Hayek, von Mises, and other neoliberal thinkers; Reagan acknowledged as much himself (Evans and Novak, 1981, 229). Additionally, Keyworth highlighted in a number of places the affinities of the Reagan Administration's S&T policy with neoliberal thinking. For example, he described this policy as being characteristically American in its emphasis upon free enterprise and individualism:

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[16] The classic formulation of the distinction between positive and negative freedom, and the classic defense of negative freedom, is Berlin (1969). The negative conception of freedom is also defended in Hayek (1960, 11–21).

> Embedded in our society even a century ago were two... important driving forces: a free-enterprise system that thrived on innovation and invention and a form of society that encouraged and admired independent thinking and creativity (Keyworth, 1982, 609).

Reagan's S&T policy is characteristically American, Keyworth suggested, because it incorporates both of these emphases.[17]

Keyworth's own personal history also evinces a dedication to the neoliberal cause. Currently, he is the Chairman of the Progress and Freedom Foundation, a conservative think tank that advocates the deregulation of communications markets.[18] According to its mission statement, the Foundation seeks to "educate policymakers, opinion leaders and the public about issues associated with technological change, based on a philosophy of limited government, free markets and individual sovereignty."

Finally, neoliberalism does provide a seemingly plausible account of why the commercialization of research might have the benefits that Keyworth claimed that it would. If it were really true that systems of voluntary exchange were ideal information transmitters; if it were really true that freedom is best interpreted in purely negative terms, and if it were really true that free markets had the effect of distributing power widely over individuals, rather than concentrating both political and economic power in the hands of a few, then we might expect research under the assisted linear model to have both the epistemic and social benefits that its defenders claim that it has.

Evaluating the Neoliberal Defense of Commercialization

There are strong reasons to believe that the commercialization of science has neither the epistemic nor the social benefits that Keyworth and its other supporters claimed that it has. These reasons concern the biasing effects of conflicts of interest, the inhibition of the free flow of information that results from the proliferation of patenting and licensing, and the restrictions on scientific freedom that result from greater corporate control over scientific decision making.

Bias and Conflicts of Interest

The commercialization of scientific research has led to a rapid rise in the existence of financial relationships between university and government researchers, on the one hand, and for-profit entities, on the other. These relationships involve such things

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[17] Recall also Keyworth's previously cited statement that, while government should play a role in R&D, it should play a "properly limited role..., one that makes sense for a free-enterprise economy" (Keyworth, 1982, 607).

[18] The complete mission statement can be found at: http://www.pff.org/about/ (accessed March 10, 2008).

as the funding of research projects by for-profit entities, consultancy arrangements, gifts, stock ownership, and management of start-up companies. In some areas of research, such relationships have become so prevalent that it is difficult to find university researchers without them (Drazen and Curfman, 2002, 1901). There is increasingly strong evidence that these financial relationships, at least in many areas of science, compromise the epistemic integrity of research by biasing scientists toward those companies with whom they have relationships.[19] This evidence, in turn, represents a strong reason for doubting that science under the assisted linear model satisfies rigorous epistemic standards.

Evidence that financial relationships between scientists and for-profit entities compromise the epistemic integrity of research comes in the form of quantitative studies comparing research conducted by scientists with and without financial conflicts of interest (e.g., Als-Nielsen et al., 2003; Bekelman et al., 2003; Friedberg et al., 1999; Stelfox et al., 1998; vom Saal and Hughes, 2005). Virtually all of this research concludes that researchers with conflicts of interest are more likely to draw industry-friendly conclusions than those without.[20]

As an illustration, consider the study by Stelfox et al., entitled "Conflict of Interest in the Debate over Calcium-Channel Antagonists," published in the *New England Journal of Medicine* (1998). This study was designed to shed light on the question of whether there is "an association between authors' published positions on the safety of calcium-channel antagonists and their financial relationships with the pharmaceutical industry" (Stelfox et al., 1998, 102). They focused their attention on articles evaluating calcium channel blockers (CCBs), which were controversial drugs used to treat hypertension. The authors wished to know whether the controversy within the medical community came down upon funding lines – i.e., whether those who supported the use of these drugs tended to have financial ties to drugmakers, and whether those who did not tended to have no such ties. The authors examined 70 articles evaluating particular CCBs published between March 10, 1995 and September 30, 1996; they found that the vast majority of researchers who evaluated these drugs favorably (96%) had financial relationships with the makers of these drugs, whereas only 37% of the researchers who evaluated them critically had such financial relationships. They concluded that "the results demonstrate a strong association between authors' opinions about the safety of calcium-channel antagonists and their financial relationships with pharmaceutical manufacturers" (Stelfox et al., 1998, 103–104).

The findings of this study, moreover, are by no means exceptional, as is evident from the review article of Bekelman et al. entitled "Scope and Impact of Financial Conflicts of Interest in Biomedical Research: A Systematic Review," published in the *Journal of the American Medical Association* (2003). The authors examined 37

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[19] For a discussion of the kinds of bias that can result from such relationships, see Wilholt (2009).

[20] For further discussion of conflicts of interest in science, see Elliott (2008) and Krimsky (2003, 125–140).

studies that evaluated the extent and effects of conflicts of interest in biomedical research, and they concluded the following:

> Strong and consistent evidence shows that industry-sponsored research tends to draw pro-industry conclusions. By combining data from articles examining 1140 studies, we found that industry-sponsored studies were significantly more likely to reach conclusions that were favorable to the sponsor than were nonindustry studies (Bekelman et al., 2003, 463).

These studies, then, provide strong reasons for believing that the proliferation of conflicts of interest in one very important area of research – namely biomedical research – tends to bias that research and is thus epistemically worrisome.[21] (In this regard, it is also worth recalling Keyworth's claim that biomedical research is one area in which we are supposedly "already seeing examples... [of an] intellectually stimulating interface... between some universities and some industries" (Keyworth, 1982, 609).)

The problems resulting from the growth of financial conflicts of interest among *individual* researchers are particularly difficult to handle given the growth of *institutional* conflicts of interest. Following the passage of the Bayh-Dole Act and other pieces of legislation in the early 1980s, it has become increasingly common for universities to obtain patents and to share these patents with the university scientists who performed the relevant research. Moreover, in many cases in which a university professor founds a start-up company in order to profit from the patent, the university that shares the patent becomes a significant investor in that start-up. According to a study by the Association of University Technology Managers, "approximately two thirds of academic institutions hold equity in 'start-up' businesses that sponsor research performed by their faculty" (Bekelman et al., 2003, 463). In these situations, universities themselves have significant financial stakes in the outcomes of their professors' research. Thus, while one might think that universities would be ideally suited to manage the conflicts of interests of its faculty, the reality is that universities themselves have conflicts of interest. Who will manage these conflicts?[22]

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[21] One could object to this line of reasoning by acknowledging that a correlation exists between industry funding and industry-friendly conclusions and explaining this correlation with the following hypothesis: industries are so careful about which drugs they research that they only pursue studies of those drugs that have a very high probability of being safe and effective (Bok, 2003). This objection is problematic for a number of reasons, the strongest being that it cannot explain the results of the quantitative studies discussed above. Consider, again, the study by Stelfox et al. (1998). The results of this study cannot reasonably be explained by appealing to the expertise of industry in predicting the outcomes of future research, for if the relevant industries rightly predicted that their CCBs would be safe and effective, then the vast majority of *all* of the scientists evaluating these drugs – regardless of their source of funding – should come to the same conclusion. Obviously, this was not the case.

[22] For further discussion of institutional conflicts of interest, see Task Force on Research Accountability (2001).

The Anticommons

A second reason for questioning the conclusions reached by Keyworth and other defenders of the assisted linear model concerns the effects that the proliferation of patenting and licensing – a direct result of the commercialization of scientific research – has upon the sharing of information. While Keyworth argued that commercialization would increase the free flow of information by expanding the domain of voluntary exchange, there are reasons to believe that, in many cases, the exact opposite is occurring; as a result, the assisted linear model is, in many cases, *discouraging* research that could lead to commercially viable and/or socially beneficial products.

Michael Heller and Rebecca Eisenberg first advanced this argument with respect to biomedical research in 1998 in the journal *Science*. More specifically, they argue that, as the number of patents and licenses on upstream research increases, people who wish to turn this upstream research into products downstream will be faced with growing obstacles, especially in the form of higher transaction costs, to the point that they will increasingly turn their attention elsewhere.

> The tragedy of the anticommons refers to the more complex obstacles that arise when a user needs access to multiple patented inputs to create a single useful product. Each upstream patent allows its owner to set up another tollbooth on the road to product development adding to the cost and slowing down the pace of downstream biomedical innovation (Heller and Eisenberg, 1998, 699).

Heller and Eisenberg discuss two avenues from which an anticommons in biomedical research can arise: concurrent gene fragments and stacking licenses. The problem of concurrent fragments stems, in part, from the fact that the United States Patent and Trademark Office (PTO) allows patents not only on genes but also upon gene fragments.[23] As a result, those who wish to develop, for example, a diagnostic test for a genetic disease that would test for a constellation of patented gene fragments, might find it excessively complicated and/or prohibitively expensive to acquire the rights to do this. An example of the problem of stacking licenses is the use of reach-through license agreements. A reach-through license agreement is an agreement according to which the possessor of a given material agrees to transfer this material to another scientist only on the condition that the latter grant to the former rights to any subsequent discoveries that are made through the use of the material in question.[24] If a researcher finds that the development of a product requires the signing of a number of reach-through agreements, she might find it more worth her while to focus her energies elsewhere.

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[23] For discussion of the kinds of patents allowed by the PTO, see the section of the United States Human Genome Project website devoted to gene patenting: http://www.ornl.gov/sci/techresources/Human_Genome/elsi/patents.shtml (accessed 6 August 2008).

[24] For further discussion of reach-through license agreements, and material transfer agreements more generally, see Mirowski (2008).

Anticommons in biomedical research could lead to deficiencies in both the quality of research and the quality of medical care. One example of this concerns the previously-discussed diagnostic test for genes BRCA1 and BRCA2, which are associated with breast cancer. Myriad Genetics, again, owns the patents on the BRCA1 and BRCA2 genes, and it has an exclusive license to test for these genes. Exclusive licensing not only raises the costs of these tests (from approximately $960/test to $2400/test) (Krimsky, 2003, 67); it also inhibits the ability of others to improve the test. It is not only the BRCA1 and BRCA2 genes themselves that are associated with breast cancer, but also mutations of these genes, and Myriad's test is not able to detect all of these mutations (Walsh et al., 2006). According to many researchers in this area, the development of tests that would detect more of the relevant mutations would occur far more quickly and efficiently if the test were not licensed exclusively to one company (Pollack, 2006; Walsh et al., 2006).

A second example concerns the diagnostic test for two alleles of the HFE gene. These two alleles are responsible for 80–85% of haemochromatosis, which is a recessive disease that causes the body to absorb excessive amounts of iron (Merz et al., 2002, 577). In 1998, a patent covering the diagnostic test for HFE was awarded to Mercator Genetics, and according to surveys conducted by Jon Merz and colleagues, the result of this was to discourage other laboratories from developing their own tests for the gene – tests which, again, could have improved our diagnostic capabilities. Of the 119 laboratories that Merz et al. surveyed, 30% of them reported discontinuing research on genetic tests for this disease after the patent was granted and after steps were taken to enforce the patent (Merz et al., 2002, 577, 578). It is likely that the primary reason for this were the high costs imposed by the holder of the license for the test, SmithKline Beecham Clinical Laboratories (SBCL). Shortly after the patent was granted, SBCL sent out letters to other laboratories, "stating its willingness to grant sublicenses for an up-front fee of $25,000 from academic laboratories, and 5–10 times more than this from commercial laboratories, plus royalties of as much as $20 per test" (Merz et al., 2002, 578).

These examples illustrate the problems that can arise as a result of increasing patenting and licensing. More specifically, they call into question the belief that expanding the domain of voluntary exchange within which scientists work will lead to epistemic and social benefits by facilitating the flow of information.

Freedom and the Corporate Directing of Research

The final criticism of the commercialization of science is that it gives private corporations too much influence over scientific decision making, including choices of which kinds of research to undertake. This criticism comes in two varieties. According to the first, commercialization skews the reward structure of science in such a way as to encourage only research into areas that are likely to be financially profitable. A result of this, for example, is that research on such topics as the beneficial health effects of exercise will be sacrificed in favor of research on drugs that treat diseases – in particular, diseases that affect members of wealthy nations. This version of the criticism has received extensive treatment elsewhere (e.g., Brown, 2008, 197–199); as a result, I will not discuss it further here. The second form of this criticism is stronger. It states not merely that scientists are receiving too few incentives to investigate issues from which there is a low probability of financial gain; it goes further in claiming that scientists will face sanctions or other kinds of external obstacles if their research is not "industry friendly."[25] It follows from this that commercialization is compromising the freedom of research.[26]

Two examples will help to illustrate and elaborate this criticism. The first involves David Healy, who in 2000 was a psychiatrist and a specialist in the history of psychiatry at the University of Wales.[27] In September 2000, he was offered and accepted a position as Professor of Psychiatry and Clinical Director of the Centre for Addiction and Mental Health at the University of Toronto; 2 months later, he traveled to Toronto in order to settle moving arrangements and to give a lecture to his future colleagues. Shortly after giving this lecture, he received word from David Goldbloom, the director of the Centre, that his job offer had been rescinded: "Essentially, we believe that it is not a good fit. . .. This view was solidified by your recent appearance at the Centre in the context of an academic lecture" (quoted in Washburn, 2005, 123). According to Paul Garfinkle, who was then executive director of the Centre, there were two claims that Healy made in this lecture that strongly upset his would-have-been colleagues; he expressed his worry that there might be a causal connection between Prozac and suicidal thoughts, and he claimed that the discipline of psychiatry, under the influence of the drug industry, was over-prescribing certain medications (Dyer, 2001, 591). After hearing Healy's lecture, a number of members of the Centre decided that they would simply not be able to work under him, and as a result, the job offer was rescinded (Dyer, 2001, 591).

Approximately 1 year later, Healy filed suit against the university, claiming a violation of academic freedom. More specifically, he claimed that his job offer was rescinded because of his criticisms of Prozac and the drug industry. This claim is made plausible by the dependence of the Centre upon pharmaceutical-industry funding. Around that time, the university received a \$1.5-million donation from Eli Lilly, the maker of Prozac, and over half of the Centre's budget (52%) came from the pharmaceutical industry (Dyer, 2001, 591). In 2002, Healy and the university settled their dispute for \$9.4 million (CAD); as a part of the settlement, the

[25] Krimsky (2003) argues that the commercialization of scientific research is bringing about the demise of public-interest scientists, or scientists who work on problems that are solely in the public interest, rather than in the interest of a private entity. While he does not distinguish between the two versions of the criticism that corporations are gaining too much influence over the choices of problems to address, he discusses examples that support both versions.

[26] This criticism does not assume that scientists prior to commercialization had *complete* scientific freedom. On the contrary, scientific decision making – including decisions regarding which problems to address – have virtually always been constrained by moral, social, political, and economic factors. What this criticism asserts is that the commercialization of research reduces the freedom that scientists have, and that this reduction of freedom will likely have important social and epistemic consequences.

[27] This case is discussed in Washburn (2005, 122–123), among other places.

University of Toronto granted Healy the title of "Visiting Professor," and Healy accepted assurances that the pharmaceutical industry played no direct role in causing the offer to be rescinded (Spurgeon, 2002, 1177). He continues to maintain, however, that the Centre rescinded the offer because of his criticisms of Prozac and the pharmaceutical industry and that, as a consequence, it violated his academic freedom.

The second example concerns Nancy Olivieri, again of the University of Toronto, who in 1993 was a professor, a researcher at the University's Hospital for Sick Children, and the director of the Hospital's hemoglobinopathy program.[28] Beginning in that year, she received funding from Apotex, Canada's largest pharmaceutical company, to perform randomized trials comparing its drug, deferiprone, against another drug, deferoxamine. Deferoxamine was the standard treatment for thalassemia, a disease that inhibits the body from producing normal blood cells; Apotex hoped that Olivieri's research would show that deferiprone is more effective than deferoxamine.

Upon receiving funding from Apotex, Olivieri signed a confidentiality agreement that prohibited her from communicating the results of her work without the consent of the company. Early in the trial, deferiprone performed wonderfully, but by the end of the trial, she questioned its efficaciousness. As a result, she began a second trial; this time, her agreement with the company did not include a confidentiality clause. As this research progressed, Olivieri not only became convinced that deferiprone was less effective than the standard treatment; she also began to worry that it was hazardous to patients. Upon communicating these results to Apotex, the company accused her of violating standard research procedures, threatened to sue her if she communicated her results, and terminated her contract. Despite this, Olivieri felt that she had a moral obligation to publish her results; she published two abstracts based on the results of her trials, and she presented her results at a conference in 1997.

It was not only Apotex that responded harshly to Olivieri's research. Following the publication of her abstracts, the University's Hospital for Sick Children falsely accused her of violating its regulations, it removed her from the directorship of the hemoglobinopathy program, and it ordered her not to discuss these matters publicly (Bok, 2003, 73). Furthermore, she was surreptitiously attacked by one of her university colleagues:

> A faculty associate sought to discredit her by sending disparaging anonymous letters to colleagues and the media and by publishing contrary findings without either informing her or disclosing that his work was being generously funded by Apotex (Bok, 2003, 73).

Soon after this, an explanation for the university's actions was revealed: for years it had been in discussion with Apotex about a multi-million dollar gift. According to Krimsky, a "nonbinding agreement was reached in 1998 under which

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[28] The Olivieri case is discussed in Krimsky (2003, 45–47), among other places.

Apotex would donate $12.7 million to finance a research center at the University of Toronto" (Krimsky, 2003, 26).[29]

After widespread publicity and the interventions of distinguished scientists from the United States and Britain, the university finally intervened and mediated an agreement with the hospital to have Olivieri restored to her former position. The university and Apotex broke off discussions regarding the company's financing of a research center; soon afterward, Apotex withdrew from its previous agreement. A number of investigations into Olivieri's conduct were undertaken, all of which cleared her of any wrongdoing.[30]

There are a number of important issues raised by the Healy and Olivieri examples. Firstly, the examples suggest two distinct freedoms that the commercialization of science can restrict: (1) the freedom to choose which problems to address and which conclusions to draw and (2) the freedom to communicate research results freely. The University of Toronto's decision to rescind its job offer from Healy represents a violation of both (1) and (2). In particular, the message communicated by the decision was that, as a psychiatric researcher, one will find better job prospects if one neither investigates nor communicates the potential safety hazards of financially-profitable medications and, instead, investigates the potential beneficial effects of drugs. Apotex's threat to file suit against Olivieri, its termination of her contract, and the university hospital's decision to remove her from the directorship of the hemoglobinopathy program all send the message that one's professional career will go more smoothly if one accedes to the demands of one's corporate funders, including the demand to refrain from communicating the results of one's research.

The Healy and Olivieri examples also suggest two different sources from which restrictions of freedoms can come. The first, and most obvious, source is private corporations, as illustrated by Apotex's treatment of Olivieri. The examples also show, however, that universities can be a significant source of such restrictions. As universities develop closer ties to private corporations, they become increasingly hesitant to jeopardize their relationships with industry. In certain cases, this could be because a university, or a program within a university, is heavily dependent on corporate funding, as was the case with Toronto's Centre for Addiction and Mental Health. In other cases, even if a university is not highly dependent upon corporate funding, it could have developed a kind of corporate culture and/or structure of governance, such that university administrators increasingly evaluate performance in terms of corporate benchmarks.[31] In such a culture, criticisms of industry might simply be unwelcome.

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[29] Krimsky further notes that Apotex "made the gift contingent on the university's lobbying the Canadian government to delay regulations that were unfavorable to the generic drug industry" (2003, 46).

[30] Further information on these inquiries can be found on the website of the Canadian Association of University Teachers: http://www.caut.ca/pages.asp?page=199&lang=1 (accessed August 24, 2008).

[31] For a discussion of the commercialization of the university as a whole, see Bok (2003), Brown (2000), and Slaughter and Rhoades (2004).

Finally, it is important to note that the violations of freedoms that occurred in the Healy and Olivieri examples were both violations of *negative* freedoms. In both cases, it was not simply that scientists had greater incentive to investigate industry-friendly issues in an industry-friendly fashion; in addition, they were faced with *external obstacles* to conducting their research in the way that they saw fit. This is especially clear in the Olivieri case, as having one's research contract terminated, being removed from one's position as director of a program, and being threatened with lawsuits are very clearly external obstacles to freedom. In these cases, then, it does not matter how one interprets the nature of freedom; whatever one's theory, the commercialization of scientific research led to restrictions scientific freedom.

These two cases are very dramatic illustrations of the ways in which the commercialization of research can restrict scientific freedom. They are, however, only two cases. Is there evidence that commercialization is leading to widespread, even if less dramatic, violations of scientific freedom? Answering this question is made more difficult by the fact that it is virtually impossible to obtain precise statistics on the frequency of such violations. While we hear about the cases in which scientists are threatened or punished for criticizing a company's product or publicizing results that reduce profitability, it is impossible to know the number of instances in which scientists, under pressure from a university or a company, decide to take the easier route. Despite this difficulty, however, there is evidence that restrictions of scientific freedom are becoming more widespread.

This evidence comes in the form of changes in the structure and priorities of universities, which have resulted in part from the Bayh-Dole Act and other pieces of legislation passed in the early 1980s. To begin with one example, consider the rise of technology transfer offices (TTOs) on university campuses. TTOs encourage the patenting of research results on the part of universities, and they help with the processes of filing patent applications and licensing patents to private corporations. The rise of TTOs was made possible by the Bayh-Dole Act, because this act, again, made it much easier for universities to patent and then license discoveries funded by the public. Recently, TTOs at major American universities were surveyed in an attempt to better understand their priorities. The results of the survey were unambiguous: the objective that was far and away the most significant was royalties generated by license fees (Thursby et al., 2001, 65–66). While this result is perhaps not surprising, it is nevertheless significant, especially given that TTOs are increasingly influencing the priorities of university scientists. If TTOs, and universities more generally, become increasingly profit oriented, it stands to reason that the employees of universities will increasingly be evaluated based on their ability to generate profits, which in turn will restrict their freedom to engage in activities that are directed toward other ends. Is this worry justified?

According to interviews conducted at large American research universities by Steven Vallas and Daniel Kleinman, it is; universities are increasingly operating according to "organizational logics" derived from the corporate sphere, which put formal pressures upon scientists to engage in commercially profitable research (Vallas and Kleinman, 2007). For example, the dean of science at a "prestigious Massachusetts university" said:

> We are not given the privilege any longer of doing research just because we're curious about an answer. . .. Because nowadays I think it's absolutely critical that we justify the use of taxpayer money based upon the fact that it has some potential to have impact on people. I don't know whether or not the committees that are evaluating people for promotion and tenure are now beginning to understand that *they must take into consideration numbers of patents, numbers of companies, the commercialization and the impact of that on the economy of the area*. But I'm assuming that if we're going to encourage that, which I know we are, that that will start to become part of the equation, if it isn't already (Quoted in Vallas and Kleinman, 2007, 292, emphasis added).

Another dean is even more explicit about the changing role of the university in becoming a handmaiden of industry.

> Right now as a university we're going through a fairly [major] search and re-evaluation of who we are and what we do, and how well we're doing it in view of budget cuts, how we should react, and what I've tried to convince my colleagues is that it would be reasonable to think of a university *as a manufacturer of capital goods*. We manufacture minds, ideas, patents in some cases, and these are the capital goods that industries are built around (Quoted in Vallas and Kleinman, 2007, 292, emphasis in original).

According to both of these deans – who are expressing views that are increasingly common in American academia[32] – the function of the university itself is changing. Universities are increasingly orienting themselves toward commercial ends, whether that is manufacturing capital goods "that industries are built around" or growing companies themselves. Given these changing aims, university scientists – and university faculty more generally – will increasingly be evaluated according to their ability to contribute to these goals.

The result of this reorientation, again, is not merely to skew the reward structure of university research toward problem areas that are potentially profitable – although this is an important result. In addition, these changes pose obvious worries for scientific freedom, and for academic freedom more generally. Depending upon how significant the demand for commercialization is, it could have the effect of virtually *requiring* scientists to engage in commercially-oriented research and virtually *forbidding* research that is not commercially viable, including research that has no practical applications and research that tends to undermine the profitability of a product (e.g., research that concludes that there is a link between suicidal behavior and certain anti-depressants). It could not merely be the case that scientists who work on commercially-relevant problems will find better opportunities for obtaining funding; in addition, scientists who decide against working on such problems could increasingly find themselves failing to be promoted, including failing to receive tenure, and thus falling out of academia altogether.

Given these threats to scientific freedom, the arguments put forward by Keyworth and others that the commercialization of scientific research will provide spaces for free and open inquiry that will encourage individual creativity and stimulating intellectual exchanges do not succeed. On the contrary, the commercialization of research threatens to restrict scientific freedom in such a way as to drive out of

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32 Ibid.

the academy any research that does not contribute to industrial development. As the dean from Massachusetts stated, investigating problems that have little practical implication is a privilege that we can no longer afford.

Conclusion: Neoliberalism, Freedom, and Power

According to defenders of neoliberalism, free markets are ideal instruments for stimulating the free flow of information and for maximizing individual liberty. One of the underlying reasons for this is that the free market ostensibly puts power in the hands of individuals. While government planning inevitably involves the concentration of political power in the hands of a few, thus threatening the liberties of individuals, the free market tends to distribute power by giving individuals the freedom to make economic decisions for themselves; in this way, it acts as a check upon government's ability to acquire power. Moreover, by reducing the power that government has over the decisions of individuals, systems of voluntary exchange help to ensure that information is distributed efficiently.

In his defense of the Reagan Administration's S&T policy, Keyworth drew upon this neoliberal background to argue that expanding the domain of voluntary exchange in which scientists operate – or bringing the marketplace into science – would result in both economic and epistemic benefits. By removing government-imposed barriers between industry and the basic research establishment, members of these different communities would enhance their ability to enter into dialogue with one another, which in turn would facilitate creative exchanges and stimulate the translation of scientific research into marketable products. Furthermore, expanding the domain of voluntary exchange would ensure that interactions between individuals would be unconstrained by government funding agencies; in this way, scientists would retain, if not enhance, their freedom to pursue their research as they saw fit. Maintaining scientific freedom, in turn, would help to safeguard the epistemic integrity of research.

There is strong evidence, however, that the commercialization of scientific research, as it is occurring in the U.S., is failing to result in the benefits that Keyworth predicted. More specifically, there is strong evidence that it is raising the incidence of bias, inhibiting the sharing of scientific information, and restricting the freedom of scientists – especially the freedom to pursue research that has little or no prospects of commercial gain. One of the underlying reasons for this is that the commercialization of science, far from putting power into the hands of individual scientists, is actually helping to concentrate the power of the private sector over scientific research.[33] As scientists become more and more dependent upon the private sector for funding, industrial interests will increasingly shape the kinds of problems that scientists address. As corporations obtain control over research entities or processes – e.g., in the form of patents or exclusive licenses – they acquire greater

[33] I argue for this claim in another context in Biddle (2007).

power over the kinds of research that is, and is not, performed with these entities or processes. As universities become increasingly commercialized, the private sector will, at least indirectly, exercise greater control over the activities of academic researchers – e.g., via the restructuring of university priorities and performance evaluations, such that faculty are increasingly judged upon their abilities to contribute to economic growth.

All of this suggests that, instead of continuing to allow the private sector to concentrate its power over scientific research, it is important that we maintain spaces that are relatively free and open in which the practice of science can proceed. It suggests, in other words, the importance of a robust notion of *the commons* for an account of how scientific research can simultaneously meet high epistemic standards and benefit the public. It provides grounds, for example, for holding the results of basic scientific research in common, in order that everyone – private corporations and the public at large – can benefit from them. Moreover, it illustrates the importance of spaces in which scientists can work in a relatively unconstrained fashion and, in particular, are free to investigate problems that have the potential to benefit not just selected private entities but also support the common good. Traditionally, universities have been regarded as spaces in which such research can take place. If they are to serve as such spaces in the future, then the forces that are currently transforming them into handmaidens of industry must be resisted.

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Medical Market Failures and Their Remedy

James Robert Brown

The daily news reminds us just how troubled is current medical research, especially the pharmaceutical component. A headline from a Reuters report stated: "Study doubts effectiveness of antidepressant drugs" (Reuters, February 26, 2008). The first paragraph continued: "WASHINGTON – Antidepressant medications appear to help only very severely depressed people and work no better than placebos in many patients, British researchers said Monday." One wonders, how could it be that after 20 years of use, billions of dollars in royalties, and thousands of suicides, we only now discover that Prozac and other SSRI-based antidepressants work in most cases no better than placebos? Was it because the true effectiveness of SSRIs was hard to detect? Or was it because of corporate influence?

More recently, a June 10, 2008 *New York Times* Editorial entitled "Hidden Drug Payments at Harvard" stated:

> Three prominent psychiatrists at the Harvard Medical School and its affiliated Massachusetts General Hospital have been caught vastly under reporting their income from drug companies whose fortunes could be affected by their studies and their promotional efforts on behalf of aggressive drug treatments.
>
> Under pressure, two of the researchers acknowledged receiving $1.6 million apiece in consulting fees from drug companies between 2000 and 2007 and the third reported earning more than $1 million. That was far more than the researchers had originally reported, a number that Mr. Grassley pegged at a couple hundred thousand dollars apiece. Even the updated numbers left out other payments that drug companies reported separately that they had made to the trio.

These disturbing items are just reminders (for more, see Angell, 2004). The deeply disconcerting stories stemming from the pharmaceutical industry in recent years are widely know, and there is no need for me to review them. More important is how we react. The common response is to suggest new regulations or to tighten those already in place. They usually amount to proposing some sort of full disclosure, which typically includes: all data from clinical trials is to be made public, any financial conflicts of interest the researchers have is to be made public, and so on.

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These suggestions are all well-meaning, but someday – soon I hope – people will realize that ever more regulation is not doing the trick. There is far too much money at stake, far too many clever people who want a chunk of it, and far too many illicit opportunities for them to do so. My own solution, which I will not argue for here, is to terminate intellectual property rights in medical research (See Brown, 2004, 2008, and various forthcoming). Without patents, monstrous profits are not possible; their elimination would end the chief corrupting motivation.

However, it is not the corrupting influence of market-based medical research that I want to discuss here, but another set of problems that stems from leaving things to the market. These are problems that would likely arise even if everyone honestly and sincerely followed every conceivable regulation put forward. The first is the lack of research on medical problems of the poor, especially diseases of the underdeveloped world. The second problem is the lack of research on diseases that are rare. The difficulty we face is the same in both cases: there is little or no profit to be made from solutions to such health problems. If we can get over the problem of funding for diseases of the poor and somehow launch serious research into them, we nevertheless come up against an additional problem in the case of rare diseases: how should we evaluate proposed solutions? This problem arises in the rare diseases case, because it is very difficult, sometimes impossible, to run adequate trials due to the small number of people affected.

Both of these problems are called "market failures." Even within the framework of for-profit medicine, these are acknowledged health problems that are not and presumably cannot be solved by the market alone. I shall begin with a recent proposal for dealing with diseases of the underdeveloped world. Later I will deal with rare diseases and orphan drugs. The kinds of solutions that are offered are, in both cases, still within the market-driven framework for medical research, and therein lies their shortcoming.

Advanced Market Commitments

On February 9, 2007 Canada, Italy, Norway, Russia, the UK, and the Bill & Melinda Gates Foundation launched the first AMC (Advanced Market Commitment) to develop a vaccine for pneumococcal disease, which kills about 1.6 million people each year. The donations were in amounts from 50 to 635 million dollars, for a total of 1.5 billion. A pharmaceutical corporation (or anyone else, for that matter) that is able to develop a vaccine that meets specifications will reap the reward.

This innovative way of funding is seen as a solution to a market failure, namely, the reluctance of profit-seeking corporations to fund expensive medical research when those who might consume the product could not afford to pay for it. The idea has been well-received by a wide variety of interested parties, including: philosophers, economists, the pharmecuitical industry, various NGOs, and even the Pope. The World Bank is deeply involved, which is often a bad sign, but UNICEF and the WHO are supportive, which is more encouraging. Here, for instance, is a sample of opinion: (See http://www.vaccineamc.org/supporters/index.html for the following quotes and many more.)

> The IFPMA welcomes the public commitment made by governments to fund the pilot Advance Market Commitment (AMC) to stimulate the development of new pneumococcal vaccines. Our research-based vaccine companies around the world have the necessary know-how and the AMCs' innovative financial incentive can help to encourage them to take on the risk associated with bringing new vaccines from the laboratory to the poorest countries that need them (Dr. Harvey E. Bale, Director General of the International Federation of Pharmaceutical Manufacturers & Associations).
>
> AMCs are innovative, market-based financing mechanisms that hold great promise in expanding access to much-needed vaccines in the developing world. The donors, World Bank and GAVI deserve important recognition for their significant efforts in moving AMCs from concept to reality (Margaret McGlynn, President, Merck).
>
> As part of the coalition calling for the rapid introduction of pneumococcal vaccination in developing countries, Meningitis Research Foundation welcomes the recommendation for a pilot Advance Market Commitment (AMC). Pneumococcal disease, including pneumococcal meningitis is responsible for up to 1 million deaths in the under fives worldwide, and many of these deaths could be prevented by immunization (Denise Vaughan, Chief Executive, Meningitis Research Foundation).
>
> The creative and promising initiative launched today seeks to counter this trend, since it aims to create "future" markets for vaccines, primarily those capable of preventing infant mortality. I assure you of the Holy See's full support of this humanitarian project, which is inspired by that spirit of human solidarity which our world needs in order to overcome every form of selfishness and to foster the peaceful coexistence of peoples (Pope Benedict XVI).

AMCs are monetary commitments to financially reward the developers of a product that has not yet been developed and would likely not be developed but for the promise to pay and to pay generously. If a vaccine, for instance, is wanted by developing countries, then rich countries, and others such as the Gates Foundation, promise to put up a considerable amount of money for a vaccine that meets specified requirements.

As one might have guessed, there are different versions of the general idea. A commitment could be to grant an outright financial award, or to purchase some specified quantity, or to subsidize the future purchase of the product, and so on. The specifications for any successful product might be precise or vague. In dealing with a disease, the AMC might specify a vaccine that is effective in 90% of cases or it might just ask for a partial cure of any sort.

The Nobel-winning economist, Joseph Stiglitz, has been one of the most prominent advocates. He briefly described his vision of AMCs as follows:

> A medical prize fund provides an alternative [to current practice]. Such a fund would give large rewards for cures or vaccines for diseases like malaria that affect millions, and smaller rewards for drugs that are similar to existing ones, with perhaps slightly different side effects. The intellectual property would be available to generic drug companies. The power of competitive markets would ensure a wide distribution at the lowest possible price, unlike the current system, which uses monopoly power, with its high prices and limited usage.
>
> The prizes could be funded by governments in advanced industrial countries. For diseases that affect the developed world, governments are already paying as part of the health care they provide for their citizens. For diseases that affect developing countries, the funding could be part of development assistance. Money spent in this way might do as much to

> improve the wellbeing of people in the developing world—and even their productivity—as any other that they are given (Stiglitz, 2006b, 1279).

Stiglitz's remark that AMCs could be seen as development aid deserves comment. It is a perfectly natural suggestion, especially in cases of a disease affecting people only in the developing world. Support might well come from a rich country's foreign aid budget, not its domestic health budget. Natural though it is, I can think of at least three considerations that speak against this suggestion. A minor consideration is the fact that researchers will be forced to deal with two bureaucracies when trying to gain funding, each trying to pass the issue on to the other budget line. Perhaps more important is the fact that the aid budget – never large to begin with – will be cannibalised, as funds are shifted to, say, worthy vaccine research from even worthier clean water projects. Finally, there might be a very good reason for thinking of any AMC project as relevant, if only indirectly, to our own health concerns. Pneumococcal disease does most of its damage in developing countries, but it occurs world-wide. It is often responsible for pneumonia and meningitis. Standard treatment is with penicillin, but drug-resistant strains are becoming ever more common. A vaccine would not only be the best way to fight the disease in the world's poorer regions, but it might be highly useful in richer realms, as well. If nothing else, we could think of it as a prudent form of research. And even if biological phenomenon such as pneumococcal disease had no impact directly on our health, learning about it will almost surely shed useful light on many other things that do.

How do AMCs fit into the general framework of market-based medical research? Stiglitz sees AMCs as a complement to the existing patent system.

> The medical prize fund could be one of several ways to promote innovation in crucial diseases. The most important ideas that emerge from basic science have never been protected by patents and never should be. Most researchers are motivated by the desire to enhance understanding and help humankind. Of course money is needed, and governments must continue to provide money through research grants along with support for government research laboratories and research universities. The patent system would continue to play a part for applications for which no one offers a prize. The prize fund should complement these other methods of funding; it at least holds the promise that in the future more money will be spent on research than on advertising and marketing of drugs, and that research concentrates on diseases that matter (Stiglitz, 2006b, 1280).

The philosopher Thomas Pogge (2005) proposes a different version of the same basic idea. He would set aside a fixed amount of money for the solution to a health problem, such as malaria or pneumococcal disease. Particular solutions would receive a piece of the pie in proportion to their impact on the disease. If two or more corporations were to put forward working solutions, they would share the rewarded in keeping with their relative contributions. Presumably, some expert body would be created to adjudicate the relative effectiveness of contributions. One can imagine lots of conflict here, but it should be no worse than other conflicts when large amounts of money are at stake.

Pogge would leave it optional for corporations to place their products in the existing IP regime (which he calls Patent I) or place it in the AMC regime (Patent II). One imagines that a vaccine for malaria would go into the Patent II regime, since the main consumers are in the developing world. But what about an AIDS vaccine? Though the developing world needs it badly, the royalties from the rich countries might be so great that financial considerations would lead them to put it in Patent I. In this case, Pogge is still faced with the problem that motivated him in the first place, namely, how to get affordable drugs to people in poor countries. The situation will be slightly paradoxical. The best hope for the poor is that the disease has no impact on the rich, then discoveries would fall into Patent II and become accessible to them. But such diseases will be less likely to be funded by the rich, since there would be less benefit to them.

Pogge notes two great virtues of his proposal. First, it is the moral thing to do. Rich countries should certainly help poor countries, and this, he thinks, is an excellent way to do so. Second, it is prudent. Pogge notes that some diseases of the developing world can quickly become diseases of the rich – SARS and Avian flu, for instance. It would be prudent, he says, for rich countries to nip them in the bud.

These two considerations are surely right. It is hard to imagine anyone opposing either. Even corporate advocates of AMCs are likely to endorse his motivation. They are, of course, moral considerations, as we might expect from a moral philosopher, such as Pogge. But are AMC proposals equally commendable from an epistemic point of view? I see several problems, in addition to the one I mentioned earlier concerning AIDS. I would not say they are insurmountable problems, but they are serious.

First of all, specific AMC targeted health problems will have to be very straightforward and easily measurable. This is certainly the case with the first one chosen, a vaccine for pneumococcal disease. We know how many die each year from it (1.6 million), and we can easily measure the effect of any vaccine, simply by measuring the reduction in the number of deaths. Appropriate payment from the AMC fund is easy to calculate. However, a disease that is debilitating to some degree and that varies from person to person, will not be easily measured. The impact of a new drug upon such a disease cannot be easily measured either. I strongly suspect that diseases involving "quality of life" that cannot be sharply defined will never be chosen for AMC support, yet they, too, are important for wellbeing.

Second, the way AMCs are currently envisioned, they are aimed at pharmaceutical corporations. A great many health problems are going to be solved with vaccines or drugs – but not all will be best solved this way. Some will involve the environment or diet and exercise. Clear drinking water, for instance, would do more to eliminate infant deaths from diarrhea than would any drug or vaccine to treat it. And yet this does not seem to be part of any version of the AMC package.

Third, who is to do the testing, the evaluation of proffered solutions? The current way of evaluating new drugs is by means of randomized clinical trials (RCTs), run by the corporations themselves. There have been no end of problems with these. Even though it is much easier to evaluate the success of a vaccine for pneumococcal disease than for, say, a blood pressure medicine or an anti-depressant, there are still lots of issues that come up, including checking for serious side-effects over a long period. The motivation to cheat is the same whether the product falls into the normal patent system or into the AMC system.

Finally, why put up money for corporations to find a solution, anyway? Why not put the same money directly into medical research that we can do ourselves? Why reward private corporations, when normal university researchers would do better work, motivated by a combination of curiosity and humanitarian concern? I will deal with this in more detail below.

AMCs constitute a major advance over pure market-driven medical research. But they leave much undone. I will now turn to the second problem, rare diseases and orphan drugs, but first, I want to introduce a way of thinking about the whole issue of medical research.

An Economic Analogy

There are numerous ways we could see various proposals for funding medical research. I'll mention three. One way is analogous to the outlook of Milton Friedman, Margaret Thatcher, Ronald Reagan – the magic of the market solves all problems. We should treat medical services, they would say, as we should any other commodity. The state should not interfere except to grant patents and enforce them. If there is a medical need, the market will satisfy it, and the best thing any government could do is get out of the way. This view has been very influential throughout Western countries for the past generation. Some regulation is compatible with this position, provided it is narrowly limited. For instance, tests for safety and effectiveness could be required and conflict of interest rules could also be added to prevent outright cheating. But such regulations should be kept to a minimum on the free market outlook on the grounds that government is generally a hindrance to a well-run economy.

A second view goes well beyond regulation and includes outright government intervention in an otherwise market-based economy. Some economists follow John Maynard Keynes in holding that a market by itself does not work all that well and on occasion will need to be stimulated in various ways. Keynesianism held sway for much of the twentieth century, especially since WW II, but fell out of favour in the Regan-Thatcher years. AMCs are rather obviously (if not self-consciously), a Keynesian view of how to promote medical research. We can see as much from the remarks of Italy's finance Minister, Tommaso Padoa-Schioppa, whose government has made the largest commitment, namely, US$635 million, to the AMC pilot described above. "The AMCs are an absolutely innovative approach which combines market-based financing tools with public intervention." (Padoa-Schioppa, http://www.vaccineamc.org/media/launch_event_01.html)

The same holds for various proposals involving rare diseases, as we will see shortly. The Keynesian outlook holds that much medical research can be left to the market, but it holds that market failures can and do occur. Some of these failures are very serious and require government intervention.

Keeping to the economic analogy, it will come as no surprise that a third view would round out the alternatives, namely, socialized medical research. This is my own view and I will outline and defend it below.

Because these issues are so politically and economically sensitive, it is perhaps no surprise that discussions of the funding of medical research should parallel discussions of the economy in general. It was not so long ago that virtually all medical research was done with no patents involved. Governments and foundations paid the bills and almost no one patented anything. Such a proposal today is hardly given the time of day. Sheldon Krimsky, for instance, is a strong critic of current medical research and a champion of extensive regulation, and yet he remarked that "no responsible voices call for an end to corporate sponsorship." (2003, 51) It is a sign of the times, especially in the US, that advocating a return to the pre-1980 medical funding situation is called "irresponsible." Krimsky is not alone. Stiglitz said much the same.

> Drug companies claim that without strong intellectual property protection, they would have no incentive to do research. And without research, the drugs that companies in the developing world would like to imitate would not exist. But the drug companies, in arguing this way, are putting up a straw man. Critics of the intellectual property regime are, by and large, not suggesting the abolition of intellectual property. They are simply saying that there is a need for a better balanced intellectual property regime (Stiglitz, 2006a, 106).

Stiglitz is doubtless right that drug companies are ignoring many alternatives to the current regime. But some of us really do want to abolish intellectual property, at least when it comes to medical research.

Rare Diseases and Orphan Drugs

As I mentioned at the outset, it is commonly acknowledged that there are two serious cases of "market failure" in free-enterprise medical research. Actually, there are many more, but these are the two that are not disputed, the two that I am dealing with here. The first, concerning medical problems of the poor and underdeveloped world, was described above. The second, to be described now, is the problem of rare diseases.

Rare diseases are defined as affecting a small number of people. To count as rare in the US the number of people with the disease is defined to be fewer than 1 person in 1,250 and in the European Union it is fewer than 1 in 2,000. The line, of course, is somewhat arbitrary; the term "ultra-rare" is occasionally used for those that are even rarer. With this definition, the number of known rare diseases is greater than 5,000. The number of people with a rare disease is about 25 million in North America and 30 million in the Europe Union. The list of rare diseases includes many that are well-known: Crohn's, cystic fibrosis, muscular dystrophy, and Lou Geherig's. They tend to be genetic, very often involving metabolic problems, and for the most part affect children.

As with diseases of the poor, research into rare diseases are not well-funded, since there is little or no profit to be made. Hence, the term "market failure." Such drugs as do exist are often hugely expensive. Individuals would find them prohibitive and even national medical services will often balk at paying the costs. For instance, Scotland's NHS (National Health Service) will not pay for laronidase, a drug used to treat an enzyme deficiency. The NHS in Wales, however, will. But it should be noted that they have only two cases costing £180,000 each. The Netherlands government is not convinced of the effectiveness of the drug, but will pay for two cases each year in order to gather evidence of the drug's effectiveness.

There are strong political and moral considerations in favour of full funding of treatment and research for rare diseases. Whether these are good reasons remains questionable. The most popular considerations include the claim that health care is a right of citizenship and that everyone is entitled to the best health care regardless of the rareness of the disease. Moreover, it is psychologically difficult to stand by and watch a child suffer when treatment is possible (at least potentially possible, given the right research). Indeed, lamenting Canada's record on orphan drugs, André Picard, who writes a regular medical column for a national newspaper, urged more support for rare disease research and cited a particular case.

> ... access to orphan drugs in this country [Canada] is poor at best and horribly uneven. For example, Hunter syndrome (also known as MPS II), an enzyme disorder that affects only about 40 people nationwide, can be treated effectively with a drug called Elaprase. That drug, which is covered by drug plans is Alberta and British Columbia, is not covered in Ontario, so the six sufferers in that province are out of luck. The drug, which needs to be infused weekly, costs about $400,000 a year. The reality is that orphan drugs are costly (Picard, 2008).

The case illustrates a major social problem – the huge cost. Picard takes the cost to justify demanding that the public pay, but the high cost could be said to point in the opposite direction. It seems an open question whether such costs of treatment can be justified. Similar questions are debated in other areas of health care, often under the heading: "rationing medical resources."

Some of the reasoning about the cost of orphan drugs is seriously flawed. For instance, Hughes and co-authors argue that

> Given the small number of patients eligible for ultra-orphan drugs, the total cost impact on health services is limited. Even for treatments that cost £50,000 per patient per year, for instance, but for which only 50 patients in a given country are eligible, the annual net budgetary impact is likely to be no greater than £2.5m. Evidence from past decisions suggest that this level of cost is sufficiently insignificant, despite treatments not being cost effective, to warrant funding (Hughes et al., 2005, 832).

But they fail to note this is just one disease. If there are 5,000 such diseases, the sum would be £50,000 $\times$ 50 $\times$ 5,000 $=$ £10,000,000,000, which is anything but a "limited" amount. Of course, this calculation is not meant to be a serious representation of the actual situation. The point is only that there is more than one rare disease and when we consider them all, the total cost will be considerable and the money might be much better spent in other areas.

It is not for me, however, to say what the morally right course of action is in these cases. The best (and perhaps only) justification for research into rare diseases is epistemic. This may be especially so for ultra-rare cases. But this fact alone is sufficient to consider rare cases further. Great expense can be justified in the investigation of rare diseases, because we will gain a better understanding of disease pathogenesis, and this in turn might shed light on all diseases, common and rare alike.

Orphan Epistemology

RTCs are not practical in evaluating orphan drugs for the simple and obvious reason that there are not enough patients. For instance, in one case it took 10 years to recruit 39 patients for a rare fungal infection trial. In another, the FDA granted a licence for a drug tested on a mere 16 trial subjects. Since small effects will not appear except in a large sample, one commentator noted that the number needed to achieve statistical confidence can be prohibitive. "... is early diagnosis of congenital adrenal hyperplasia ... beneficial? One aim of such screening is to prevent death in male babies with a severe salt-losing phenotype during an adrenal crisis. Detection of a 50% increase in deaths in the unscreened, when compared with screened babies (and surely the percentage would be less), would require 2,500,000 in each arm of the trial" (Wilcken, 2001, 293)

If RCTs are not the typical source of evidence in the evaluation of orphan drugs, then what is? Wilcken describes a typical instance, from a time before RCTs became the "gold standard" of evidence.

> One of the first trials of treatment in a rare inborn error of metabolism, phenylketonuria, (McKusick 261600) was undertaken by Professor Horst Bickel and his colleagues, and reported in a preliminary communication in the Lancet (Bickel et al. 1953). Their patient was 2 years old, "an idiot, unable to stand, walk, or talk." She was placed on a specially prepared low-protein diet and over a few months improved markedly. Then (without the mother's knowledge) 5 g per day of phenylalanine was added back into the diet. Within 6 h she started to bang her head as formerly, and within days she had lost all the ground previously gained. To test this further, she was admitted to hospital, where the experiment was repeated (with her mother's permission), with similar results. Professor Bickel had performed a study with single-blind and open-label phases. The conclusion was that "In this child at least, the benefits of a low-phenylalanine intake seem unequivocal." There have been no randomized trials of treatment (versus no treatment) of phenylketonuria (Wilcken, 2001, 292).

Wilcken draws a strong and crucially important conclusion concerning RCTs and their absence in cases of rare diseases.

> Because of the many biases that can arise in observational studies, most people would agree that, where possible, a randomized trial is the preferred model for clinical trials. Although different questions require different trial methodologies, the hierarchy of evidence is generally agreed to be
>
> - Randomized controlled trials, and their derivatives (systematic reviews of RCTs)
> - Controlled observational studies

- Uncontrolled studies
- Expert opinion.

> It is unfortunate that scientists and clinicians dealing with the very rare diseases often seem to be locked into the bottom rung of this hierarchy (Wilcken, 2001, 292).

The hierarchy of evidence that Wilcken outlines, has become orthodox in recent years, under the banner "Evidence Based Medicine." In many ways it is problematic, but there is also much that can be said in its favour. In any case, it will not be criticized here, but rather will be accepted for the sake of getting on to the main point, which is the evaluation of proposed treatments for rare diseases.

The US "Orphan Drug Act" was passed in 1983 to cope with some of the problems of rare diseases, the difficulties of funding their research and of their evaluation.[1] Among its provisions, the act allows for additional public funding for research, it adds extra patent protection for any discovery, and it lowers clinical trial evidence requirements. The last of these is the focus of Wilcken's worry, and rightly so. It is conceded by most commentators that getting away form rigorous RCTs involves many dangers and that the chance of serious bias at "the bottom rung of this hierarchy" is great.

As with AMCs, the Orphan Drug Act is Keynesian in spirit. It intervenes to solve a market failure, but it does so within a market framework. While we might concede that it can stimulate research into rare diseases that would not otherwise be done, it has nothing to say about the problem we face in evaluating orphan drugs. The rareness of rare diseases means we must forgo RCTs and must instead rely on things such as observation studies and expert opinion, the types of evidence generally considered to be of the lower grade. The upshot is that we must now rely on observations and expert opinions made by those who may have a significant financial interest in the outcome. Such evidence is quite unreliable and the situation is intolerable, even if the experts in question makes a full disclosure of what they stand to gain.

There is really only one solution to this. Evaluation must be taken out of the hands of anyone with a financial stake in the outcome. Disclosure is not sufficient, even if it is full and complete. Often people do not even recognize their own biases. Evaluation must be by a neutral agency that has nothing to gain but useful knowledge. It could be an independent, publicly supported agency or perhaps properly funded university based scientists, who have no hope of acquiring any IP rights.

Above, I concluded the discussion of AMCs with a list of shortcomings. One of these was that it might be better to give the equivalent funding to independent university-based researchers, rather than to corporations. Governments could still specify the health condition they wanted researched, such as pneumococcal disease, but they could leave the approach and the type of solution open. As things currently stand for both AMCs and rare diseases, pharmaceutical companies are given the job

[1] Europe has something similar. USFDA (Food and Drug Administration) and EMEA (European Medicines Agency) now agree on common product application, making it easier and quicker to market orphan drugs. However, drug approval is still distinct.

with the understanding of all parties that solutions to health problems will be drug solutions. This will sometimes be the best solution, but there is no good reason to assume it a priori. The Orphan Drug Act and the way AMCs are envisioned skew research in these two fields toward drugs. This to the detriment of diet, exercise, environmental solutions, which are often preferable.

The two market failures, diseases of the poor and rare diseases, dovetail to some extent. The Keynesian approach to each is similar; it is also a failure in both. By socializing these two areas of medical research we can overcome the epistemic problems involved with each. In both cases, public agencies devoted to such research would be best. Disinterested university scientists with no possible financial stake, no possibility of acquiring IP rights, and so on, are the obvious ones to carry it out. Until a generation ago, this is how things were done. And medicine made progress. There were mistakes, some serious, but we did not see headlines on a regular basis reporting yet another scandal, with countless numbers of people affected by yet another shoddy though highly profitable product.

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Thoughts on Politicization of Science Through Commercialization

M. Norton Wise

Politicization

The current politicization of science in the US – by which I mean the attempt politically to control the content of knowledge and not just the direction of research – is arguably unprecedented in history, aside from a few famous and anomalous examples like the Galileo and Lysenko affairs. Although complaints have been developing for years, the first major public protest against the abuse of science by the current administration was the statement published by the Union of Concerned Scientists in February 2004, "Scientific Integrity in Policymaking: An Investigation into the Bush Administration's Misuse of Science." It charged the administration with "a well-established pattern of suppression and distortion of scientific findings by high-ranking Bush administration political appointees across numerous federal agencies" and with "a wide-ranging effort to manipulate the government's scientific advisory system to prevent the appearance of advice that might run counter to the administration's political agenda."[1] Its signatories grew to include thousands of scientists and many former government officials, with 48 Nobel laureates, 62 National Medal of Science recipients, and 135 members of the National Academy of Sciences (Mooney, 2005, 225). This consensus is as unprecedented as the scope of the abuses it protests. Because I am primarily concerned with its significance for knowledge in a democratic society, I will draw my analysis from widely accessible public sources.

Chris Mooney, in *The Republican War on Science*, provides a well documented account of the origins of the recent political manipulation of science, which goes back to the 1970s but flourished after the Republican takeover of Congress in 1994 known as the "Gingrich Revolution," with its assault on federal regulation. The signal event was the dismantling of the OTA (Office of Technology Assessment), which had functioned for 24 years as Congress's source of independent advice on issues of science and technology. The tactics of the Gingrich assault were borrowed

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[1] http://webexhibits.org/bush/index.html

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from the long experience of the tobacco industry in defusing claims of the harmful effects of smoking. They simply funded their own research, which was aimed at casting doubt on the certainty of the claims of harm even when it did not contradict them. The effectiveness of the technique is shown by a 1998 study in *JAMA* (*Journal of the American Medical Association*) on review articles of research done on second-hand smoke, which showed that a "not harmful" conclusion was 88.4 times higher if authors had industry affiliation (Mooney, 2005, 10).

Increasingly important in the new version of this technique were think tanks with sponsorship from industries seeking to block regulation: American Enterprise Institute, Heritage Foundation, Pacific Legal Foundation, George C. Marshall Institute, Annapolis Center for Science-Based Policy, and others. But perhaps most intriguing in Mooney's analysis, is his account of the adoption by opponents of regulation of a systematic rhetorical strategy. Research results that opposed or minimized the need for regulation, typically industrially funded, would be labeled "sound science" while pro-regulation research, usually carried on at universities and government laboratories, would be labeled "junk science," in the interest of manufacturing scientific doubt. The genius in this move is that the term "sound science" has been picked up in reporting by the mainstream media, often without recognizing its loaded meaning or that it is inscribed in such conservative organizations as The Advancement of Sound Science Coalition. A countermove on the part of the Union of Concerned Scientists to recapture rhetorical control has apparently not been so effective. Its Sound Science Initiative[2] is an "email-based vehicle for scientists to respond to and influence fast-breaking media and policy developments on environmental issues" (Mooney, 2005, 65–76).

Examples of overt attempts to control the content of science during the current administration could be taken from virtually any area of political significance: global warming, endangered species, ozone depletion, chemical pollution, or oil drilling, without even entering the fraught areas of abortion, stem cells, evolution, brain death, or the morning after pill. One example may serve to indicate how far this movement has progressed. In March 2006 the *Los Angeles Times* published a penetrating investigation by Ralph Vartabedian of the controversy over the solvent TCE (trichloroethylene), under the headline "How Environmentalists Lost the Battle over TCE." After a 4-year study, the EPA (Environmental Protection Agency) concluded that TCE was 40 times more likely to cause cancer than previously thought and issued a preliminary report in 2001 aiming to begin setting rigorous new standards to limit exposure. Although now largely eliminated from most applications, TCE had formerly been widely used at military installations throughout the country for degreasing metal parts and then dumped into pits where it entered the groundwater. It is reportedly the most widespread water contaminant in the nation, involving 1,400 DOD (Department of Defense) sites, with 67 EPA Superfund sites in California alone. Huge plumes spread for miles, sometimes under heavily populated areas, and are correlated with elevated risks for cancer and birth defects (Vartabedian, 2006a, b).

[2] http://www.ucsusa.org/global_warming/sound-science-initiative.html.

What makes this story particularly interesting is the sharp upturn in politicization that it evoked. It involves the DOD, which has traditionally been a rather apolitical organization with respect to domestic issues and, according to the lead author of the 2001 EPA report, had done everything possible to ensure environmental safety. This time, however, faced with monumental costs, the DOD joined with the Energy Department and NASA (both of which also have contaminated sites) to launch a full-blown attack on the EPA's science, apparently to at least delay any further remediation. Not surprisingly, they mobilized the rhetoric of "sound science" to minimize the risks of TCE while accusing the EPA of a left-leaning bias and "junk science." They obtained the "sound science" backing of a toxicologist from the organization representing TCE manufacturers, the Halogenated Solvents Industry Alliance, who said that "If TCE is a human carcinogen, it isn't much of one." And they were also able to rely on the Bush-appointed research director at EPA, Paul Gilman, who claimed that "Inside the Beltway, it is an accepted fact that the science of EPA is not good" and that an entire consulting industry had sprung up in Washington to attack the EPA and sow seeds of doubt about its capabilities. In this politically constructed climate the DOD, NASA, and the Energy Department appealed their case directly to the White House, where it was taken up by a working group made up largely of officials from their own agencies, who had originally been assembled in 2002 to combat the EPA's assessment of another pollutant, Perchlorate. Ultimately they referred the dispute to the National Academy of Sciences for more study (Vartabedian, 2006a).

In July 2006 the National Research Council of the National Academies issued its report, largely supporting the findings of the EPA, though criticizing some technical aspects of their study. They judged that the evidence of health risks had increased since 2001 and urged that the agencies "finalize their risk assessment with currently available data so that risk management decisions can be made expeditiously" (National Research Council, 2006). But 7 years have now gone by since the initial EPA report and no risk assessment has been issued. The Senate Environment and Public Works Committee, chaired by Barbara Boxer (Democrat, California), is presently (spring 2008) considering bills to require the EPA to set standards for both TCP and Perchlorate, which the EPA now seems disinclined to do. A final irony in this saga is that some of the committee's minority members, Christopher Bond (Republican, Missouri) and James Inhofe (Republican, Oklahoma, see below) are now charging the committee with politicizing science (Cone, 2008).

Meanwhile, the Union of Concerned Scientists surveyed EPA scientists about politicization. More than half who responded, or 889 of 1586, reported at least one type of interference within the last 5 years. Many charged the White House Office of Management and Budget with interfering in decision making and with delaying EPA rules they did not like (Pasternak, 2008). Similarly, responding to widely publicized complaints in 2006 of suppression of climate change results at NASA – complaints lodged most prominently by NASA's leading climate scientist James Hansen – and to a request for an inquiry from fourteen Senators, the agency's inspector general carried out an extensive investigation and issued a report in June 2008. It blamed political appointees and "politics inextricably interwoven" into the operations of the agency's press office for the fact that studies on global warming between 2004 and 2006 had "reduced, marginalized, and mischaracterized" information on climate change science. "Worse, trust was lost… between an Agency and some of its key employees and perhaps the public it serves" (Winters, 2008, 47–48). Frank Lautenberg (Democrat, New Jersey), who wrote the original request for an inquiry, has put the problem succinctly: "The Bush Administration's manipulation of that information violates the public trust"(Revkin, 2008).

Indeed, the public trust is at stake in the politicization of science. But what has this to do with university research and the pursuit of commercialization?

Commercialization

Just as prominent as politicization of science in popular reports has been its commercialization. In *Universities in the Marketplace: The Commercialization of Higher Education*, Derek Bok, former president of Harvard University, has given an accessible overview of the problems commercialization poses for the entire life of the university. I consider here only the research component. Industrial consulting, patenting, industrially funded research, and spin-off companies are not new at American universities but they have grown dramatically since the 1970s, with the percentage of academic research funding increasing from 2.3 to 8% by 2000. The turning point in commercialization came in 1980 with the Bayh-Dole Act, which made patenting of federally funded research – by the National Science Foundation (NSF), the National Institutes of Health (NIH), and other agencies – much more attractive than it had previously been by granting the right to exclusive licensing. The basic idea was to promote the public good by decreasing the time elapsed between research findings obtained with public funds and useful products. Profits to universities and researchers would be the motor. And it was a powerful motor. By 2000, university patenting had increased 10 times, earning more than $1 billion per year, and 12,000 academic scientists had established industrial connections (Bok, 2003, 12; Mowery et al., 2004). Since then, according to an NSF report, patenting has remained nearly constant, as has industrially funded research, although it has dropped significantly as a percentage, from 7.4% in 1999 to 4.9% in 2004 (Rapaport, 2006).

These aggregated figures are somewhat deceptive because they mask a remarkable concentration of industrial funding in a relatively small and decreasing number of universities. Those receiving more than 10% of their R&D funding from industry declined from 52 in 1998 to 21 in 2004. But most research universities are aggressively pursuing intellectual property agreements, industrial partnerships, and joint university/industry research parks. An extreme example is Arizona State University, which President Michael Crow announces as "A New American University: The New Gold Standard" as opposed to "*the gold standard of the past*" represented by the traditional elitist research universities. In addition to fostering many laudable social goals, this new university will be entrepreneurial, committed to an "enterprise imperative" and to "use-inspired" scholarship.[3] Its entrepreneurial

[3] http://www.asu.edu/inauguration/address/

centerpiece for moving into the future is an enormous new Biodesign Institute headed by George Poste, who came to the job from 20 years at SmithKline Beacham Pharmaceuticals. In four football field size buildings with 800,000 ft^2 of floor space (half completed), it supports industrial partnerships for translational research in biotechnology, nanotechnology, and vaccinology that will carry visionary projects all the way from discovery to commercial development. Already in 2004, when the first building opened, they "filed 17 patent applications, launched three spin-out companies, and increased grant funding 30%."[4] This is the face of commercialized academic research. It involves faculty whose commitments are forthrightly split between commercial and academic interests and whose prestige and promotions depend increasingly on money brought in from patents, contracts, and licensing agreements.

The Biodesign Institute may very well represent the future of university research; certainly it has more limited analogues at all research universities today. "Academic Business," one prominent educator labeled the new commercial institution, asking: "Has the modern university become just another corporation?" (Delbanco, 2007). The verdict is not yet in. Hopefully, the gains for research will be great. But serious thought needs to be directed toward some obvious difficulties for the public interest. Consider patenting. From the perspective of the traditional values of science, which rest on the free exchange of information, patenting has the potential to disrupt scientific progress: through decreased willingness to share information, materials, and instruments; monopolistic licensing practices; and the inhibition of downstream research. Anecdotal examples abound but as yet no rigorous statistical study has confirmed the inhibition effect. If confirmed, it would imply that the university, by expanding its patenting of research results, would be undermining its own mission to promote research and the acquisition of new knowledge.

Even more compromising to the university's mission are contracts that grant to corporate sponsors proprietary rights over research results. A particularly dramatic example has come to light at Virginia Commonwealth University where a little-known contract in 2006 with Philip Morris USA granted to the corporation not only patent rights but also the right to refuse publication or even discussion of research findings by the university researchers involved, violating both the university's own rules requiring freedom of publication and retention of all intellectual property rights. This case is unusual and involves a university that carries on relatively little sponsored research – though a similar case involving Novartis occurred at Berkeley in 1998 (Bok, 2003, 151; Salgado, 2008) – the arguments made by both the university and Philip Morris to justify the contract in terms of a new relationship aimed at protecting corporate interests is disturbing. "It's counter to the entire purpose and rationale of a university… its not a consulting company; it's not just another commercial firm," commented David Rosner at Columbia (Finder, 2008).

It should be clear from the outset in discussions of commercialization that research for profit is not necessarily research in the public interest. It may be, but it may also skew the path of research in directions that are not of most benefit to

[4] http://www.biodesign.asu.edu/about/overview/#funding

society. This long-term consequence is a structural one and may not seem sufficiently concrete. More immediately apparent is the potential for active subversion of the public interest by the distortion of research results. Because reports of this kind have become so numerous, especially in medical research, one case may serve for the genre.

Over the last several years a controversy has blossomed over "aspirin resistance," the claim that many who take aspirin as an anti-clotting agent to reduce the risk of existing or potential heart ailments may be resistant to the drug, are at increased risk of heart attacks and strokes, and should perhaps be taking other anti-clotting drugs. An article by David Armstrong in the *Wall Street Journal* in April 2006 brought the issue to widespread public attention under the title, "Aspirin Dispute is Fueled by Funds of Industry Rivals." Researchers raising the aspirin alarm have largely been funded by Accumetrics, who make the most widely-used test for resistance, and by Schering-Plough and Bristol-Myers Squibb, who market alternative drugs. Plavix, sold by Bristol-Myers Squibb and Sanofi-Aventis, with $5.9 billion in sales in 2005, lags behind only the anti-cholesterol drug Lipitor. On the other side, some of the leading researchers protesting aspirin resistance have been funded by the big aspirin maker, Bayer (Armstrong, 2006).

Since the majority of the research involved has been carried out at universities, one may wonder whose interests they and their scientists represent. An instructive example is that of Dr. Daniel Simon, associate professor at Harvard Medical School. Simon published an article in the trade journal *Physician's Weekly* in 2005 reporting that perhaps 30% of the 25 million people taking aspirin for heart problems were aspirin resistant. The article did not disclose that Simon had research funding from Accumetrics and Schering-Plough nor that he was a consultant and paid speaker for Schering-Plough. Instead, *Physician's Weekly*, who knew of the connection, said that their policy is not to disclose such potential conflicts of interest but to use the connection for things like securing advertisements to be placed next to the article from the sponsoring companies. The irony in this circle of interests – from manufacturer to researcher to publisher to manufacturer and back to researcher again – is that rather than leading to professional censure it led Dr. Simon to new studies of aspirin resistance funded by the same companies and to a new position at Case Western Reserve University. As for *Publisher's Weekly*, their cynicism seems to be even-handed. Dr. Charles Hennekins of the University of Miami School of Medicine, who had done basic research in the 1980s on the benefits of taking aspirin daily, objected in the journal in 2004 to the resistance scare, saying that "this undocumented phenomenon may have the negative consequence of reduced aspirin use." His connection to Bayer was not disclosed (Armstrong, 2006).

Such practices are not only a matter of the trade press but also show up in the most prestigious of journals. The *New England Journal of Medicine*, for example, got caught up in the scandal over Vioxx as a result of having published the report in 2000 that exaggerated its safety. The study was sponsored by Merck, the maker of the drug, and suppressed Merck's own evidence that Vioxx was more dangerous than its equally effective competitor naproxen (Aleve), available over-the-counter at one-tenth the price. The journal rightly blamed Merck, but in 2001 it violated its own conflict of interest rules when it published a review article dismissing the dangers of Vioxx written by two authors with financial ties to Merck. The difficulty may be not only that clinical studies funded by drug companies are three times more likely to favor the sponsor's drug (according to a 2003 report in JAMA) but that academic journals are as dependent as academic researchers on commercial funding. "Three quarters of the clinical studies published in the three most respected medical journals (NEJM, JAMA, Lancet) are now commercially funded" (Abramson, 2006).

The potential for distortions of research inherent in these developments, which seem to have become epidemic in biomedicine if not yet in other less commercially lucrative areas of science, has sent universities, science publishers and federal agencies like the NIH scrambling for remedies. Before taking up remedies, however, I want to consider the seriousness of the problem.

Public Trust and Threat to Democracy

The sagas of aspirin resistance and of the Vioxx report are examples of what Sheldon Krimsky has aptly labeled *Science in the Private Interest*. His worry is not simply that this or that researcher produces distorted results, or even fraudulent claims, but that the entire system of biomedical research, especially as carried out at universities, may no longer be serving the public interest (Krimsky, 2003). If that is the case, or if it is widely perceived to be the case by the consuming public, then research for profit in universities will make them look increasingly like think tanks funded by private interests. And this is precisely the ground on which politicization of science has become such a virulent problem since the early 1990s. If it were just a matter of Bristol-Myers Squibb competing with Bayer to gain market share, we would likely look at the squabble as merely a matter of advertising claims, from which we might hope to extract some humor if not much objectivity. But when the research of corporations is backed by the credentials of universities as servants of the public interest, then we have a different situation. Should the public put its trust in what is seen to be the corporate research of the University of Miami, Harvard, or Case Western, as compared with laboratories funded by the tobacco industry? Or is university science providing just the latest example of "sound science" as compared with the "junk science" that used to be done at universities?

If the commercialization of academic science comes to have the character of science in the private interest, then it is the status of universities in the polity that we need to be concerned about, not merely the objective validity of some particular research report. Along this route lies politicization. Only let the aspirin question become one of proposed regulation and it will immediately become a candidate for politically motivated attempts to control the regulatory outcome by controlling the content of research results. The only thing that saves academic research from this fate – to the degree that it does escape – is its claim on science in the public interest.

An example of how the process of politicization has been working of late can be seen in the role that Willie Soon, a Harvard-Smithsonian astrophysicist, and

David Legates, a University of Delaware climate scientist, played in the contest over global warming. This is of course one of the most egregious recent examples of political misrepresentation and distortion (as above). Soon and Legates both did research supported directly or indirectly by the American Petroleum Institute, the George C. Marshall Institute, and/or Exxon-Mobil, though apparently that was not widely known. They were called to present their findings against global warming to the Environment and Public Works Committee of the US Senate. They came at the invitation of the committee chair Senator James Inhofe of Oklahoma. Inhofe is the man who once called the Environmental Protection Agency a "Gestapo bureaucracy" and global warming a "hoax." He aimed to use Soon and Legates to discredit the "junk science" carried on at universities and government laboratories that made global warming a practical certainty. The issue here is not so much whether Soon and Legates did valid research; it is that the commercialization of their university research brought them into the process of politicization just as if it had been the work of an ideological think tank, which it was in part. Commercialization can make the distinction hard to draw. And if Inhofe could have made the claim stick that the weight of other academic climatologists represented special interests and uncertain science then he would have had a much easier time making Soon and Legates politically credible. In this case the tactics did not succeed and it seems that much of the public trust in academic climate research remains more or less intact, though the struggle has been hard fought and might have ended up otherwise.

The problem is a very deep one for democratic societies as we know them, because universities play a key role in our decision-making processes. They are our primary institutions of trustworthy knowledge. By trustworthy knowledge, I do not mean that it will always turn out to be correct, but that it is worthy of our trust because we believe that the people and institutions who produce it have made every attempt to ensure that their interpretations are valid in the current state of research. Such sources of knowledge are crucial to the effective functioning of both legislators and the voting public. Without them, decisions can only be made arbitrarily or politically, in the worst sense of the term, meaning purely ideologically or for a particular interest, without adequate ground for judging what would best serve the public good. Collective, deliberative civic life depends on an informed public and informed legislators, whose knowledge is widely distributed.

The historical and theoretical basis for this view of democracy as dependent on widely distributed knowledge is the subject of a new book by Josiah Ober on classical Athenian democracy, aiming to show that "putting knowledge into action is the original source of democracy's strength... [and] remains our best hope for the future" (Ober, 2008, 2). Or as John Adams put it in 1765, "Liberty cannot be preserved without a general knowledge among the people, who have a right, from the frame of their nature, to knowledge.... The preservation of the means of knowledge among the lowest ranks, is of more importance to the public than all the property of all the rich men in the country" (Ober, 2008, vii).

Granted, this sounds rather idealistic. But the university as a source of trustworthy knowledge has a status in a democratic society similar to that of two other fundamental institutions: the free press and the independent judiciary. Of course the press and the judiciary are never quite free and never quite independent but the ideal is extraordinarily valuable nonetheless, and both depend on trustworthy knowledge.

This critical role of trustworthy knowledge was celebrated in a national conference in April 2007 sponsored by the two most venerable institutions of enlightenment in the United States, the American Academy of Arts and Sciences, proposed by John Adams and others during the Revolution and formalized in 1780, and the American Philosophical Society, established with Benjamin Franklin's leadership in 1743. Under the banner of "The Public Good: Knowledge as the Foundation for a Democratic Society," prominent academics, legal experts, and journalists reiterated the ideals of Adams and Franklin for the two sponsoring societies and of Abraham Lincoln for the National Academy of Sciences. In his keynote address, Don Michael Randel, President of the Mellon Foundation, put the point succinctly: "Democracy dies if the citizenry is not told the truth" (Randel, 2008, 11). The present political climate provided a constant backdrop for the need to reiterate this basic truth about truth. As the writer E.L. Doctorow said: "history seems to be running in reverse, and knowledge is not seen as a public good but as something suspect, dubious, or even ungodly" (Doctorow, 2008, 77). Such worries, although expressed with less direct reference to the Bush administration, supplied motivation for exploring current threats to truth and to democratic principles in a politicized judicial system and in control of the news media by ever-larger corporations. Strangely, however, no one broached the issue of commercialization of the research university, the mainstay of the learned academies, as an equally significant threat to public trust.

Pure-Applied and Academic-Industrial Distinctions

Public trust in the claims of science has long rested on the belief that, generally speaking, scientific results are objective. Indeed, non-objective means non-scientific. Objectivity in this sense does not refer to ultimate truth but to objective validity: other people doing similar work would get corroborating results. Interpretation of the results, furthermore, is aimed at providing the most plausible account of them in relation to other empirical and theoretical findings. The best guarantee for such trust in the objectivity of science has usually been thought to be a separation of the pursuit of truth from the pursuit of material interests. Like other teachers and scholars, scientists should not be motivated by personal gain or ideological interests. As Jacques Loeb of the Rockefeller Foundation put it early in the twentieth century, "if the institutions of pure science go into the handling of patents I am afraid pure science will be doomed." (Weiner, 1986, 35; Bok, 2003, 139).

In the United States, our intuitions about this standard view of disinterested science as the guarantee of objective science have been supported by two canonical texts, Henry Rowland's "Plea for Pure Science" of 1883 and Vannevar Bush's

Science the Endless Frontier of 1945. Writing in the midst of the "golden age" of American industrial growth, Rowland's plea rested on the belief that university research and education should answer to a higher moral purpose, that the search for truth epitomized by the sciences served to produce citizens with integrity and discipline. Implicitly, he was attacking the popular hero Thomas Edison, whose phonographs, telephones, and electric lights epitomized scientific accomplishment to much of the public. Such pursuit of profit, in Rowland's view, compromised the ideals of science (Rowland, 1902).

Vannevar Bush, writing at the end of World War II at the request of President Franklin D. Roosevelt, agreed about the values of pure science but articulated a way around worries like those of Rowland and Loeb. He represented the great contributions of science during the war as products flowing precisely from the distinction between basic research and applied research. The flow was a one-way stream, from the basic (pure) to the applied. Thus the question of whether profits would infect the source never arose. Vannevar Bush could depict science as an endless frontier of progress and prosperity at the applied end without compromising the ideal of knowledge for its own sake from which this bounty emerged. Critical to this image was the institutional distinction between universities, on the one hand, as the location of basic research, and industry and the military, on the other hand, as the location of applications (Bush, 1990, 6–7, 12, 19–22). This understanding of the pure/applied and university/industrial distinctions has continued to supply the basic terms of discussion throughout the growth of federally funded research administered through the NSF, the NIH, and other agencies, until recently.

It may very well be that the canonical distinctions have helped to insulate academic research from the threat of compromise by material interests, thereby maintaining objectivity and the public trust. The question remains, however, whether the separation has been, or is, necessary for this purpose. Does pursuit of profit necessarily undermine pursuit of truth? And even more fundamentally, are the distinctions historically valid?

Historians of science have by now shown repeatedly that far from being the derivative products of research for its own sake, technological developments have just as often been the source of basic experimental and theoretical pursuits. A paradigmatic case for me is that of William Thomson, Lord Kelvin, the very image of science for the British professional and popular audience of the late nineteenth century and a founding theorist of modern energy physics. In each of the areas of his foundational work: electromagnetism, thermodynamics, mechanics, and the sought-after vortex atom, his theoretical perceptions depended critically on his deep engagement with concrete technologies, most notably the submarine telegraph, the steam engine, and the vortex turbine. His patenting and marketing of telegraphic instruments made him a wealthy man, as symbolized by his ocean-going, 126 ton schooner-yacht, the *Lalla Rookh* (Smith and Wise, 1989). Another example is the great chemist Justus Liebig, whose pioneering work in rationalized agriculture and chemical fertilizers, as well as the production of meat extract and baking powder for the kitchen, accompanied his laboratory analysis of substances like superphosphate, his theoretical discoveries of radicals and isomers, and his monographs on organic chemistry (Schwedt, 2002). It is illuminating to recognize that even some of the most esoteric conceptual developments of modern theoretical physics have been rooted in part in quite practical concerns. Einstein's theory of special relativity, with its elegant analysis of the problem of simultaneity using moving railway cars and exchange of light signals, was grounded in the problem of synchronizing clocks for railway networks and of the practice of exchange of telegraph signals (Galison, 2003).

These few examples serve to make two obvious but oft-forgotten points: the pursuit of truth and the pursuit of profit have often stood in a complementary relationship; and one of the most fertile sources of scientific creativity has always been engagement with technological practices. As a historical matter, then, the pure/applied and university/industry distinctions, especially on the Vannevar Bush model of a one-way flow of knowledge from pure to applied and from university to industry, have never been valid, although the ideals embodied in the distinctions served an important purpose in elevating truth above profit in the scale of both academic and public values and certainly guided science policy for decades (Grandin et al., 2004; Johnson, 2004).[5] Recent developments within the sciences, furthermore, have made the distinctions increasingly untenable, even as ideals. Several interrelated developments are readily identifiable.

First, the pure/applied distinction rested in part on maintaining the status of quite general and abstract theoretical physics, particularly elementary particle physics in the twentieth century, as the ideal of "fundamental" science. That ideal focused on finding high-level general laws – covering laws – that would explain (in the sense of derivation) lower-lying and more specific phenomena. This model of what science should be has lost much of its sway in the last 30 years, in part because it was artificially supported by the prestige that it acquired from the atomic bomb project, which was subsequently maintained during the Cold War but rapidly declined after the collapse of the Soviet threat in 1989.

Secondly, it has became increasingly apparent, even in physics, at least from the 1960s, that high-level theories of quantum mechanics, general relativity, and elementary particle physics offer little in the way of explanation in the world of everyday materials and processes that populate other areas of physics. The argument has been made in an accessible manner by condensed matter physicist and Nobelist Robert Laughlin in *A Different Universe: Reinventing Physics from the Bottom Down*. In many other areas new ideals of scientific explanation have emerged: chemistry, geology, climate studies, genetics, and others (Laughlin, 2005; Laughlin and Pines, 2000; Wise, 2004). These are the areas where the sciences of complexity have grown up. Equally, biology has replaced physics as the dominant science of today. And in neither biology nor the sciences of complexity does the ideal of covering laws have much purchase. On the contrary, these sciences are highly dependent on

[5] The papers collected in Grandin et al. (2004) provide a thorough critique of the pure-to-applied and university-to-industry model, or the "linear model."

technological mastery: computer simulations, model organisms, polymerase chain reactions (PCR), microchip arrays, imaging technologies, and nano-engineering.

This critical role of technologies constitutes a third characteristic of contemporary science. The technologies provide tools to think with, tools that are all the more important in the absence of organizing laws (Creager et al., 2007). Here the distinction of pure and applied is very hard to make. Indeed, the noted historian of science Paul Forman, has argued at length that technology has acquired primacy over science in the "postmodern" era. He sees the acceptance of this new state of affairs, among scientists and philosophers alike, not as a response to any basic change in science or technology, but as an unfortunate reaction to cultural change (Forman, 2007).[6] Although I disagree with Forman's diagnosis of cultural culpability, since scientists are as deeply responsible for the changing climate as is the general culture, he is certainly correct in recognizing the powerful role of technology in current intellectual life as well as popular consciousness.

Finally, it has become evident that what used to be called applied research is carried out also at universities and that basic research is carried out also in industrial laboratories. In these circumstances, where it is widely recognized that "applications" are actually one of the most fruitful resources for creative science, there can be no question of preserving the purity of science by dreaming of the "endless frontier" in the form that Vannevar Bush projected. The frontier may be endless but if so it depends on cross-cultivation of academic and industrial science. One can only conclude that commercialization in some form is inevitable in a healthy scientific environment. But what form?

The question goes deep. Consider once again the aspirin resistance story. The most far-reaching issue is not who is right, though it would be nice to know, nor even slanted research reports, though the potential is clearly present, but that the public has no trustworthy way to get information about the validity of the claims, either for or against aspirin resistance, because the studies are nearly all funded by the corporations who manufacture the tests and the drugs and by researchers with material interests in the outcomes of the studies. As Dr. John Eikelboom of McMaster University put it, "there is a real issue of who you can get unbiased opinion from in medicine." He was an author on a study in 2002 that first raised the specter of aspirin resistance and has consulted for companies on both sides. "It is a terrible problem. . . I try to be honest with myself, but I can't pretend I will always be as honest as necessary." Eikelboom is right about the problem. Thus Daniel Simon says it would be a mistake to dismiss the views of researchers with conflicts of interest because industry is one of the main sources of medical progress, so that those people without conflcts "are not truly expert" (Armstrong, 2006).

[6] Forman worries that the cultural primacy of technology is justifying the view that ends justify means, as opposed to the principle long established in democratic societies that ends must be judged in terms of the means for accomplishing them. Current political developments countenance the worry, as in the Bush administration's arguments for torture, but I am skeptical that technology is the culprit.

Occasionally the conflicts are extraordinary, as in the case of three prominent Harvard psychiatrists who each received from $1 million to $1.6 million in largely unreported consulting fees from drug companies (Harris and Carey, 2008). These scientists had been leaders in the soaring diagnosis of bipolar disorder in children and its treatment using anti-psychotic drugs. Their clinical trials and treatment practices had been controversial in any case but are now further compromised by the hidden money. Their case highlights the more general problem. Among experts serving on the institutional review boards that oversee clinical trials at medical schools and research hospitals, over one-third report having financial interests in companies producing drugs and medical devices and 7% had a direct conflict of interest with respect to reviews in which they participated (Campbell et al., 2006; Gellene, 2006).

So far universities and federal research organizations have largely approached the conflict of interest problem among their researchers by limiting gifts and honoraria from industrial sponsors and by requiring disclosure of research funding and other forms of financial interest. Although these steps are certainly laudable, its is unlikely that by themselves they can cure the disease. Research on bias shows that it is unintentional, unconscious, and indirect; that even very small gifts like pens and notepads are highly effective; and that disclosure of conflict of interest does not eliminate bias, though it may warn others (Dana, 2003). To be effective, it seems, controls on conflict of interest would have to actually prohibit all forms of financial relationship, including research funding and consulting fees, but that would disrupt the cross-fertilization between academic and industrial pursuits.[7]

The New Landscape of Science

To recapitulate, in his book on *Invention: The Care and Feeding of Ideas*, the mathematician Norbert Wiener, who had himself done exceedingly important work on feedback control of guns and other weapons during WWII and pioneered cybernetics, delivered an impassioned critique of the "megabuck scientist" of the twentieth century, arguing that "there is at present [1950s] a fundamental opposition between the spirit of the free creative scientist, the originator of ideas, and that of the scientist who is working in an organization which is primarily adapted. . . for commercial exploitation." I have argued that in this specific form the critique is historically

[7] For example, the University of California has drafted a *Health Care Vendor Relations Policy*, emanating from the Office of the Provost and Executive Vice President and dated 25 January 2008, that sets minimum standards for its ten campuses. Supported by a bibliography of six articles in respected medical journals, the Univeristy recognizes that "A growing body of research has consistently demonstrated that nominal gifts from vendors [e.g., drug makers], heretofore thought to be innocuous, unconsciously affect provider [physicians'] behavior," and is attempting to find a compromise, prohibiting obvious abuses such as gifts to individuals while allowing donations that support its mission. Allowed exceptions include educational donations, competitive prizes, research donations, and "honoraria for a specific service rendered" (e.g., delivering a speech, which can be a large exception). Research contracts, consulting fees, royalties, and stock ownership are not controlled, except in the requirement of disclosure.

untenable. On the other hand, and this was Wiener's ultimate concern, research for profit does threaten to undermine such traditional academic values as free exchange of information, which he rightly coupled with the "good of the people" in both material and political terms: "The truth can make us free only when it is a freely obtainable truth"(Wiener, 1993, 106, 154). Even more fundamentally for the democratic process, the freely obtainable truths must be seen to be trustworthy. This will not be the case if commercialization has the effect of politicizing academic research by putting it on a footing similar to that of the "sound science" of corporate-sponsored think tanks. The threat is already clearly visible in biomedicine. If it spreads much more broadly than it has so far, it will severely erode public confidence in the trustworthiness of academic research. And with that it will compromise one of the pillars of our knowledge-based democratic society. So we need to reevaluate the landscape of current science.

One attractive illustration for university/industry relations can be seen in Princeton University's award of its 2006 James Madison Medal, its highest honor for a graduate alumnus, to Arthur D. Levinson for his "success in bridging the worlds of science and business." Levinson earned his Ph.D. in biochemical science in 1977 and has gone on to become the CEO of Genentech, where he has developed collaborations with universities and cancer research institutions. The company's scientific studies are regularly cited in peer-reviewed journals at the same time as it is included on *Fortune* magazine's list of the best companies to work for. Levinson has served on the editorial boards of several journals of molecular biology and virology, has been a leader in directing rigorous clinical trials, and is an author on over 80 scientific articles.[8] So Levinson surely belongs on the dream team for university/industry collaborators. But his activities also epitomize the inbreeding of research, journal publication, clinical trials, patenting, and boardroom decision-making. We need to be asking if such inbreeding, in general, serves the public good, since these are precisely the sorts of relationships that have sometimes led to what Krimsky calls "science in the private interest": skewed research, monopolistic patenting and licensing practices, suppressed test results, non-disclosure of conflicts of interest. They also yield fertile ground for politicization. Can the public trust survive the inbreeding? What controls ought to be in place to see that it does survive?

A second example of university/industry interrelationships may help to address these questions. It concerns the "Bio-Fab Group." The group consists of nine colleagues and friends who are contributing to biological engineering, or more specifically to the fabrication of biological systems from component parts – BioBricks – by analogy with the fabrication of semiconductor chips. They aim for a vertically integrated production system, building up from the specification of DNA sequences with particular characteristics at the bottom; to the manufacture of biological parts that realize the desired characteristics; to the assembly of these parts into devices (like inverters and switches); and up to more complex systems (like transistors and circuits). Circuitry is only one example. They are also working

[8] http://www.gene.com/gene/about/management/exec/levinson.jsp. *Princeton*, Spring 2006, 6.

on the manufacture of compounds that would be effective in preventing the spread of malaria and HIV and they envisage novel proteins for gene therapy and energy production (Baker et al., 2006).

In terms of the new landscape of science, the fab group is interesting in several respects. First, they come from universities throughout the country: University of Washington, Harvard Medical School, Boston University, MIT, UC Berkeley, Duke, Cal Tech, and Princeton, suggesting the geographical landscape of collaborative research in the age of the internet. Second, as a highly multidisciplinary group, representing a diverse range of expertise in molecular biology, computational biology, and biological engineering, which is required for the vertical integration of the fabrication process that they envisage, they completely scramble the pure/applied distinction. Third, they are all directly involved as founders or scientific advisors of commercial companies aiming to market fabricated biological systems, vitiating the university/industry dichotomy. Fourth, they (and the larger synthetic biology community) are deeply concerned with developing ethical codes and regulatory agreements both to address ecological and criminal risks associated with devices that could replicate and evolve and to ensure biological justice.[9] And fifth, one of them has spearheaded the organization of a non-profit foundation, the BioBricks Foundation, that seeks to maintain open public access to a library of BioBricks, to encourage codes of standard practice, and to provide professional and public education.

These last two elements of the synthetic biology enterprise begin to look like institutional forms that could seriously cope with the problems of commercialized science that can undermine public confidence in the trustworthy character of academic research. It seems crucial that their ethical and regulatory concerns have emerged from within their own ranks in their attempt to preserve – simultaneously – the best interests of academic research, industrial production, and the public interest. It is also crucial that in its attempt to exercise an oversight role, the BioBricks Foundation is a non-profit organization, although founded by engineers and scientists who are directly involved in commercial biotechnology research. Just how and whether such an oversight organization will be able to maintain an independent critical judgment remains a large question, but such institutions should be systematically explored at all research universities.

Finally, and turning to a more traditional form of oversight in the public interest, it should be perfectly clear by now that dismantling the Office of Technology Assessment was a very bad idea indeed. It provided Congress with in-depth assessments of technical issues, not for the purpose of making specific policy recommendations but to give an accessible account, specifically for informing legislators, of differing views and alternative courses of action. Efforts to revive the OTA have been launched by the Union of Concerned Scientists and the Federation of American Scientists. A former physicist, Congressman Rush Holt (Democrat, New Jersey),

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[9] http://hdl.handle.net/1721.1/32982. Public Draft of the Declaration of the Second International Meeting on Synthetic Biology.

is one of the leaders of the initiative in Congress, arguing that legislators are ill-equipped to deal with the array of complex technical issues that come before them and that the OTA served as an admirably independent source for their deliberations. It should be rejuvenated with a director named in as apolitical a manner as possible (perhaps by the National Academy of Sciences) and generously funded to carry on the public's work.[10]

In short, a system of non-profit, independent oversight institutions, both private and public, needs to be put in place to buttress public trust in the claims of scientific research to objectivity and to serving the public interest.

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[10] See the Union of Concerned Scientists at http://www.ucsusa.org/scientific_integrity/restoring/scientific-advice.html. The Federation of American Scientists has launched a new archive of OTA documents at http://fas.org/ota/, which also contains a widely distributed video interview with Rush Holt, http://fas.org/ota/2008/07/23/watch-rush-holt-talk-about-ota/. Princeton University is host to an archive containing all formally issues reports of the OTA and other documents at http://www.princeton.edu/~ota/.

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Political Effectiveness in Science and Technology

Daniel Sarewitz

Prologue: Why Do Some Things Get Better?

Imagine yourself living 300 years ago. At that time, which of the following predictions might have seemed plausible?

- That all people would learn how to read with fluency.
- That the world would be inhabited by six billion people, and that enough food would be produced to feed all of them.
- That floods and hurricanes would be predictable days in advance.
- That 95% of all children in Europe and North America would survive through their fifth year, driving life expectancy into the 70s.
- That smallpox would be wiped out in the world.

Three hundred years ago, only one of these predictions was less than crazy. Literacy expectations were high: In the mid-seventeenth century, for example, Massachusetts colony was sufficiently confident in the feasibility of achieving widespread literacy that a law was enacted requiring that reading be taught in the home. In contrast, even a century later there were no general expectations for reducing childhood mortality, or smallpox. "A dead child is a sign no more surprising than a broken pitcher or a blasted flower," said one of Massachusetts' most famous citizens, Cotton Mather, in 1721 (quoted in Allen, 2007, 28). He knew this well: only two of his 15 children survived to adulthood. Yet when Mather sought to implement smallpox variolation in Boston, he was repudiated by the medical profession and his house was firebombed.

Today the situation is reversed. In the United States, decades of efforts to improve the reading skills of our secondary school students have proved remarkably inef-

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fective. In contrast, we have reduced childhood mortality to a tiny fraction of what it once was, and eliminated smallpox from everywhere except a few weapons laboratories.

I want to explore the question of why humans make considerable gains in their efforts to solve some problems, whereas even persistent and significant effort on other problems yields little advance (cf. Nelson, 2003). What are the sources of effective human action? And in exploring this question I want to say something about the political meaning of technology, and of science, and their inherent differences. And then I'll make some unabashedly normative observations about what this might mean for progressive politics.

I began with this very brief historical perspective in an effort to tutor our intuitions. In areas of human problem-solving where progress has been significant, the reasons may seem patently obvious in retrospect, but they did not necessarily seem so before-the-fact. So today it might seem ridiculous to compare, say, the teaching of reading to the prevention of childhood infectious diseases. Of course the reading problem is more difficult: the context for applying know-how is deeply complex, and includes a mélange of behavioral, political, and biochemical factors, such as the conditions in the home, in the school, and in the student's brain. Childhood diseases, in contrast, are contextually simple, their prevention depending only on a simple intervention in the human immune system. But this obviousness was once obscure – who knew what an immune system was in 1700? – and even just a century ago the prospect of reducing childhood mortality to a few percent would have seemed a utopian delusion, especially in contrast to our ability to achieve widespread literacy.

The political perspective is not dissimilar: The best methods for protecting children from measles and other childhood infectious diseases are pretty generally accepted, whereas the benefits of competing teaching techniques are often bitterly contested. But when someone firebombed Cotton Mather's house in 1721, the problem of smallpox prevention was every bit as politically laden as the vitriolic debates that go on today in the U.S. over the proper way to teach reading.

So I want to begin by claiming that the sources of the human ability to make progress on solving particular problems are non-obvious. In particularly, I'm going to argue that when we do end up making progress on a seemingly difficult problem, the progress is typically not easily explained by the level of resources or effort that we throw at the problem. Rather, significant progress depends on the existence of a core element of know-how – of the ability to reliably achieve a desired consequence – that acts as a fulcrum for, and a magnifier of, effective action. This core element of know-how embodies several attributes:

- Condensed cause-and-effect linkages
- Observable and thus demonstrable cause-and-effect linkages
- Context-independence
- Reliability
- Transferability
- Capacity for incremental improvement

I'm further going to make the argument that in cases where significant progress in solving a problem occurs, political convergence around a solution typically is a consequence of that progress, not a cause of it. This is important because it says that "political will" to solve a problem is catalyzed by know-how relevant to solving the problem, not vice versa. And if this is sometimes, or often, the case, then it perhaps can tell us something about the types of problems that are most amenable to making progress on, and equally important it might help us see when our efforts are misplaced, misdirected, or likely to be frustrated.

Science, Technology, and the Political Logics of Climate Change

My interest in this set of issues and questions arose in part from years of great frustration about the problem of climate change. Now I understand that in Europe, and among political progressives in the U.S., the major obstacle to making progress on climate change for years was seen as the political intransigence of the U.S. But I think this was incorrect – tragically and profoundly incorrect. It is of course true that the U.S. was intransigent until President Obama was elected in 2008, but it is equally true that Europe has made no significant progress in dealing with the climate problem, and that legislative proposals being considered in the U.S. as I write this chapter are unlikely to deliver much progress either. Overall, lack of progress on climate change reflects confusion about where, and how, we ought to expect progress to occur.

Now the mental model surrounding the formal approach to climate change – represented by the United Nations Framework Convention on Climate Change and the Kyoto protocol – treats scientific research as the engine of necessary political change. First, we develop a comprehensive understanding of the fundamental behavior of the coupled ocean-atmosphere system, including the impacts of human activity on that system, and prediction of future evolution of the system, as reported every several years by the Intergovernmental Panel on Climate Change. This knowledge then compels action, because it proves that there is a problem, and demonstrates the need for action. This mental model has motivated the expenditure of enormous sums on climate science research, and on setting up an international governance regime aimed at mobilizing the nations of the world to cooperate in reducing emissions of gases that contribute to greenhouse warming.

Keep in mind that action is to be motivated by prediction of the bad things that will happen in the future as a result of our actions today – bad things like more floods and bigger hurricanes, longer droughts, stressed ecosystems and agricultural systems, and resurgent infectious diseases – amplification, that is, of major problems that we already face, and that we have confronted with highly variable attention and success absent the motivating fear of climate change. So the chain of logic here is that science will motivate nations to cooperatively establish policies that will force them to reduce their emissions by changing their behavior, and incentivize them to invest in more efficient technologies. The result of these actions will be a reduction of the future magnification of bad things.

Let me probe this logic. Problems like natural disasters, biodiversity loss, declining availability of clean water, and infectious diseases are already very serious challenges to human well-being, challenges that have been growing worse by the decade, for reasons mostly of human development patterns such as urban and coastal population growth. Climate change will make many of the problems even worse, however. Scientific research on climate will motivate people and nations to take actions aimed at slowing this "even worse" part of the problem, reducing its effects some time in the distant future. In other words, climate science will make more compelling the reasons for addressing a range of climate-related issues that already greatly challenge society, and which we have yet to address effectively. Yet, to be clear, when thinking about such challenges as natural disasters, biodiversity loss, or infectious diseases, the approaches to reducing vulnerability depend little, if at all, on scientific knowledge about climate change. Apparently, then, climate change knowledge points to the need to reduce emissions of greenhouse gases so that these problems won't become even worse in the future, but it does not seem to advance action through other means.

So climate change discussions implicate, and confuse, two very different types of problems. The first is the problem of societal vulnerability to a variety of multi-causal but climate-related challenges – floods, droughts, and the like. The second is the problem of reducing the greenhouse gas emissions that will make these challenges even worse. Now these problems are distinct in two key dimensions. First, as a temporal matter, the inertia in both the global energy system and in the behavior of greenhouse gases in the atmosphere means that reduction of the magnifying effect of global warming on already existing problems cannot occur for many decades. Second, as matter of effective action in the world, reducing greenhouse gases is a much more tractable problem than reducing social vulnerability to climate impacts.

Here's what's happening: The moral challenge of reducing social vulnerability to climate impacts is being exported to motivate action on the largely technical challenge of reducing carbon emissions, action apparently justified by the scientific evidence for anthropogenic climate change. Solving the technical challenge will not resolve the moral one, however. If greenhouse gas emissions instantaneously went to zero, societal exposure to climate impacts would continue to grow, as it has over the past centuries.

Why is this a problem? Isn't it all for a good cause? Indeed, some may be offended by my characterizing emissions reductions as a technical challenge, rather than a moral imperative. But let me return to my theme. I am interested in effective action. Let's compare these two problems of carbon emissions and climate impacts. I'll use natural disasters as a proxy for climate impacts.

While carbon dioxide emissions have been rising progressively since the industrial revolution, these increases have also been accompanied by a progressive decarbonization of primary energy sources, and by a progressive decarbonization of economic activities in industrialized countries (e.g., Nakicenovic, 1996). To be sure, because overall energy use and economic activity continue to rise, emissions continue to rise as well – but the history of energy technology tells us that we can continue to decarbonize the energy system, and the more recent history of technological innovation tells us that we can significantly increase the rate of decarbonization through an appropriate portfolio of investments and regulations. This is the type of problem that technologically sophisticated countries have learned how to solve. In the case of decarbonization, however, we have barely tried (though the widespread adoption of nuclear power by France perhaps puts them farther along the path). For example, G-7 nations *disinvested* in energy research and development between the early 1980s and late 2000s, perhaps by as much as 65% – a trend that is only reversing itself after 25 years.

But the learning curves and the historical trends are in the right direction. The challenge here is to accelerate the process. And what I will argue here is that the best way to do this is not through moral arguments, bolstered by science and intended to overcome competing interests and get people to modify their lifestyles in particular ways so that they use less energy, which would at best have a small effect on emissions, but through the potential of technological innovation to satisfy multiple, competing interests *without* demanding significant behavioral change.

The climate impacts problem is inherently different. Unlike carbon emissions, the underlying trends are mostly in the wrong direction. In the case of disasters, both the numbers and the costs continue to increase, not because of global warming but because of demographics and socioeconomics. In particular, more people are moving to coasts, more poor people are moving to cities (which are often on coasts), and more development is occurring in environmentally unsustainable ways. Disasters disproportionately harm poor people in poor countries because those countries typically have densely populated coastal regions, shoddily constructed buildings, sparse infrastructure, and inadequate public health capabilities. Poor land use leads to widespread environmental degradation, such as deforestation and wetlands destruction, which in turn exacerbates flooding and landslides. Emergency preparation and response capabilities are often inadequate, and hazard insurance is usually unavailable, further slowing recovery, which in turn fuels vulnerability to future disasters.

In 1998, 5,000 people died in Nicaragua in a matter of a few minutes from a mudflow triggered by Hurricane Mitch. Hurricanes are relatively common in Central America, but the problem was that the people were living on a deforested, and thus unstable mountain slope. When the slope became saturated with water, it collapsed into a wave of mud.

The devastation of New Orleans by Hurricane Katrina in 2005 tells a similar tale. The progressive development of the city and the environmental destruction of the surrounding wetlands rendered it increasingly vulnerable to hurricanes, and the levees that were designed to protect the city under precisely the circumstances that Katrina presented were poorly designed and maintained. While there was enough suffering and loss to ensure that most everyone living around New Orleans got a good dose, it was the poor, the disenfranchised, the infirm, and the historically discriminated-against who suffered most and were disproportionately left behind to fend for themselves. Indeed, New Orleans, with its stark juxtaposition of the affluent and the poor, provided a synoptic portrayal of the global climate impact challenge.

Now I want to emphasize two attributes of this challenge. First, the level of scientific understanding surrounding the causes and impacts of natural disasters can be very high. For example, scientists have known – and warned for years – that the location of New Orleans on a rapidly subsiding river delta in the heart of the hurricane belt made some version of Katrina entirely inevitable (for example, see Fischetti, 2001). Similarly, the conditions leading to the Nicaraguan mudflow had been well-modeled and accurately predicted.

Second, the natural disaster problem, and climate impacts more generally, cannot be linked to a coherent technological solution path. Many well-tested policies are available to help reduce vulnerability to natural disasters. These range from building codes that can keep structures from collapsing in a storm, to land use regulations that limit construction in flood-prone areas, to environmental laws that preserve natural features, such as wetlands and forested slopes, that act as buffers against disasters. Yet all such policies are complex to adopt and implement, typically pitting vested interests against one another and demanding reasonably functional enforcement at local levels. They are, that is, politically difficult to implement. Historically, the clearest path to reduced climate vulnerability has been increased and better-distributed wealth, and in this sense the problem is simply a subset of the larger problem of addressing global poverty.

So climate change is two problems that are profoundly different in their essences. The first is the largely technological problem of reducing greenhouse gas emissions; the second is a much more difficult problem of social infrastructure and wealth creation necessary to protect people against climate impacts. And our inability, our unwillingness, to think clearly about the different essences of these two problems is, in my view, one of the reasons why we have made so little progress in addressing either of them over the past 20 years or so.

The Mysterious Case of the Missing Causal Agent: Technology and the Ozone Problem

Now I want to say something further about why we should expect problems that can be solved technologically to show more progress than those that must be solved through political and policy processes. The story I want to mention tells how in the 1980s the nations of the world came to an agreement – the Montreal Protocol on Substances that Deplete the Ozone Layer – to phase out the production of chlorofluorocarbons (CFCs), a class of technologically and economically important refrigerants and solvents that also happen to destroy the stratospheric ozone layer that protects Earth from harmful ultraviolet radiation.

The popular narrative, highly simplified, goes something like this: The CFC-ozone problem was discovered by basic scientific research on atmospheric chemistry; the results raised public and political concern; science evolved over the next 20 years and eventually stimulated an international response, the Montreal Protocol, which was first signed by 24 countries plus the European Union in 1987 amidst continuing scientific controversy and opposition from many other countries. Conclusive scientific demonstration of the causal relations between CFC emissions and loss of stratospheric ozone over Antarctica came shortly afterwards, at which point opposition to the treaty from many nations and from the chemicals industry disappeared. The treaty came into force in 1989 and by the early 1990s most nations of the world had signed on. The lesson? Faced with definitive knowledge of a clear, shared risk, the nations took effective action. This is a story of science forcing right behavior, a story that provided the model upon which the response to climate change was later based.

But isn't there something missing here? Did the many nations that agreed to phase out CFC production decide to live without the benefits of keeping refrigerators and buildings cold, or keeping semiconductors clean? Of course not. The missing element here is the technological. In reports, articles, and books about the ozone story, much is made about how the science brought industry to the negotiating table. In particular, confirmation of the link between CFCs and the Antarctic ozone hole supposedly led DuPont to immediately declare that it would stop producing CFCs, and made it impossible for the chemical industry as a whole to oppose the treaty. This version of events satisfies the perspective of both the scientific world, because we see facts stimulating rational action, and the commercial world, because we see corporations having no choice but to act responsibly in the face of evidence. But the chemical industry had been exploring CFC substitutes since the mid 1970s. By the mid-1980s, DuPont in particular came to realize not just that an array of alternatives were feasible, but that they offered a route to significant new profitability and competitive advantage.

My intent, by the way, is not to criticize DuPont; on the contrary, they were doing what firms are supposed to do. The real point is that the success of the Montreal Protocol was made possible because effective alternatives to CFCs were coming online. Science, scientific assessment processes, corporate self-interest, and diplomacy all helped create the conditions for following this path. Indeed, one observes that particular interpretations of the CFC and Montreal Protocol story seem very much to reflect the particular disciplinary or professional orientation of the person doing the interpreting: The critical role of science gets star billing in the versions written by scientists; private sector roles are highlighted by people at business schools (Maxwell and Briscoe, 1997); diplomacy is the central causal agent in accounts written by diplomats (Benedick, 1998); government assessment processes dominate in the versions written by those who work on technological assessment (Parsons, 2003); narratives and social construction get center stage in the science studies versions (Litfin, 1994). One gets a strong scent of both intellectual silos and overdetermination when surveying this literature. And certainly each of these perspectives is valuable. But I want to emphasize that, in the absence of a potential for technological substitutes, concerted global action would simply not have been possible, because society was highly dependent on the functions served by CFCs, and would not have done without them. There would have been plenty of room both for continued political conflict and continued scientific debate. Technological capacity eliminated the need for disagreement, and thus allowed the other factors to come into play. So I want to suggest that the availability of effective technological alternatives stands as a deeper, more fundamental causal factor than these disciplinary explanations.

The availability of alternatives to CFCs made it possible to meet the goals of multiple constituencies with conflicting values and worldviews: for example, those whose primary interest was to protect the ozone layer, those whose primary interest was to make money producing chemicals, and those, especially in the developing world, who were unable to give up on the benefits that CFCs alone could provide in an economically viable way. This story is perhaps less satisfying than the tale of science convincing people to make sacrifices for the good of the planet and humanity, to do the right thing regardless of worldly consequences, but it has the virtue of actually explaining how effective action was able to come about.

One Works, the Other Doesn't: Technology and Science in Politics

So now I'm beginning to home in on a way to think about the politics of technology – or, more generally, of effectiveness – that is, perhaps, rather different from the way this issue is typically framed by those of us who study technology and society.

We're all comfortable with the idea that science is always connected to action via the values and interests of those who want to act in a particular way. When a scientific fact – say, that the earth's atmosphere is warming – becomes associated with a political agenda that supports a particular type of action – say mandated emissions reductions – the science shoulders the values and interests of those who are pushing that agenda. Science becomes a tool of political persuasion, a lens for focusing many values and interests on a single type of action. This difficult task is further compounded because as science approaches the cutting edge, it tends to raise as many questions as it resolves, so there is always room for debate about what the science is actually saying. And even if scientists could confidently predict the societal consequences of global warming (which they can't), such knowledge would not dictate any particular path of action. So the current media hemorrhaging about the scientific "consensus" over global warming, triggered by the release of the latest IPCC assessment, will lose its glow as soon as the talk begins to get serious about what needs to be done. This process was very much on display during the U.S. Congressional debates over climate change legislation in 2009, and especially with the collapse of international negotiations at the Copenhagen climate conference later in that same year.

Technology is different. Technology is itself the embodiment of reliable action. Technologies are cause-and-effect machines. And in this capacity, what's especially powerful about technologies is that often they can serve a variety of preferences simultaneously. A commuter who wants to reduce spending on gasoline, an environmental group that wants to reduce greenhouse gas emissions, and an automobile manufacturer that wants to develop new, profitable product lines all find their interests converging in, say, the development of hybrid vehicles. This is a trivially obvious example but it says something about the relationships between technology and politics that is absent from the relation of science and politics. People holding diverse and even strongly divergent values and interests may converge around a particular technology that can advance multiple interests. Technology, that is, can overcome political conflict not by compelling diverse interests and values to converge – the job often assigned to science – but by allowing them to co-exist in a shared sense of practical benefits. This does not mean that some people or groups may not reasonably (or unreasonably) oppose the use of a technology – but in so doing they choose not to share in the technology's ability to reliably achieve a certain outcome. They choose to marginalize themselves from the effectiveness of the technology.

Now I need to make several things clear about the limits of my argument. First, when I use the term "technology" I mostly mean to refer to physical artifacts that embody some particular, predictable action, but I also want to include well-specified routines or protocols in this definition. Second, in speaking of what technologies *do*, I am focusing on the stripped-down action essence of the technology – the core of reliability – not whatever complexities may occur as a result of secondary and perhaps unintended interactions and consequences. Third, I'm not making a normative claim here – not yet, at least. I'm trying to be descriptive. Finally, I am not making a general argument about what *all* technology does, but a general argument about what *some* technologies can do. And I will not get too far into the question of why some technologies show this behavior and others do not, because I haven't figured that out yet.

Returning to the stratospheric ozone depletion story, we can schematically imagine an array of different constituencies involved in the negotiation of an international agreement to deal with the problem. Each of these constituencies can be characterized by some combination of values, interests, incentives, and ways of thinking about how the world works, which I'll call "ways of knowing" (cf. Feldman et al., 2006). So, for example, international environmental organizations, chemical companies, and the governments of developing countries, while of course not monolithic categories, nevertheless can be expected to embody rather distinctive assemblages of these sorts of attributes, especially when they are engaging one another in a debate over the interpretation of complex science and the regulation of an important chemical.

As I've said, the standard story is that the science drives a convergence of values, interests, and ways of knowing, as everyone comes to recognize the shared risk of ozone depletion and recognizes the right thing to do. But what really happened is that a new technology – hydrochlorofluorocarbons (HCFCs) – offered to resolve the local source of conflict among the various key contending constituencies without demanding major change in their defining attributes.

And in the U.S. we're beginning to see exactly this type of phenomenon on the emissions end of climate change with ethanol. As described in a recent *Economist* (Anonymous, 2007) article: "Farmers love [ethanol] because it provides a new source of subsidy. Hawks love it because it offers the possibility that America may wean itself off Middle Eastern oil. The automobile industry loves it, because it reckons that switching to a green fuel will take the global-warming heat off cars. The oil industry loves it because the use of ethanol as a fuel additive means it is business as usual, at least for the time being. Politicians love it because by subsidizing it they can please all those constituencies."[1] Ethanol is unsatisfactory in many ways, and in the end may prove more of a hindrance than a help to the cause of energy system decarbonization, but the aggregation of disparate interests around specific technologies is something we'll continue to see in the emissions reduction arena, driving the incremental, and in some cases discontinuous, advance of decarbonizing technologies.

Now I want to say something about the difference between successful technologies and successful policies. In his classic paper "The Science of 'Muddling Through,'" Lindblom (1959) explained how public policies come about in a highly contested and uncertain political environment where neither cause-effect chains nor objectives can be agreed upon. In such cases, he observed, "the test [of good policy] is agreement on policy itself, which remains possible even when agreement on values is not" (p. 83). Lindblom goes on to say: "Agreement on policy [is] the only practicable test of the policy's correctness" (p. 84). The outcomes of a policy *cannot* be the proper judge of how good the policy is, because the system is usually too complex to specify how the policy is connected to the outcomes. Thus, the ability of the policy itself to organize competing political perspectives is the measure of how good the policy is.

A technology offers something a policy cannot: a reliable cause-effect chain that delivers a particular local outcome with great consistency. And even if different groups are drawn to this technology for different reasons, to advance different interests and worldviews, it is this consistency of *outcome* that brings them together.

The Progressive's Dilemma

So climate change is actually two fundamentally different types of problems: one that is going to be amenable to resolution through technological intervention and continued innovation, should we choose to invest and regulate appropriately, and one that is going to be much more challenging, and will have to be confronted much more indirectly, much more in a "muddling through" mode, where progress is halting and likely to be considerably less satisfactory than we would like. One thing I find particularly troubling about the hijacking of the more difficult problem (social vulnerability to climate impacts) to motivate the resolution of the easier problem (energy technology innovation) is that when we are on a much better path to decarbonizing the energy system, we will not be on a much better path to protecting vulnerable regions and people from climate impacts. Indeed, the inevitable increases in disaster losses and other negative impacts of climate in the coming

[1] "Castro was right," *Economist*, April 7–13, 2007, pp. 13–14.

decades will continue to be exploited by advocates of various stripes as a reason to continue to decarbonize, rather than as a reason to redress the social, economic, and environmental inequities that are at the root of climate vulnerability.

But another thing I find troubling is that by using the moral language of vulnerability to drive emissions reductions, rather than by framing it as a largely technical challenge, we have not moved nearly as decisively as we might toward emissions reductions because the focus has been on achieving conformity of worldview and behavior, rather than on accepting pluralism and understanding technology's capacity to harness such diversity through the reliability of technological outcomes.

Let me seek to generalize here by going back to the two unrelated problems I began with: immunizing children against diseases, and teaching children to read (for a more complete discussion, see Sarewitz and Nelson, 2008a, b). These display much the same attributes as emissions reductions and vulnerability reductions, only they are not usually linked politically or ethically (although they could be.)

The first thing to recognize is that the diverse group of actors, interests, and ways of knowing that have converged around childhood immunization is every bit as complexly pluralistic as the group that continues to battle, in the U.S. at least, over competing approaches to teaching reading. Both cases are also characterized by a very well-defined goal, and a shared desirability of achieving that goal – preventing childhood disease; creating children who are sufficiently literate to succeed in today's society. Progress toward those goals is easily measured, so the success or failure of alternative actions aimed at achieving the goal can be fairly clearly assessed.

The ability to make progress on one but not the other manifests in a number of ways, not all of which may be obvious. First, disputes over cause-effect relations are much more rampant for reading than for vaccines. Second, and as a result of this, scientific research is prescribed and carried out on teaching reading, with the aim of resolving the controversies about what ought to be done for the reading problem. This is important, because scientific research in areas of ongoing value dispute related to complex system behavior tends to make those disputes worse, not better (Sarewitz, 2004). This is one domain where the distinction between technology and science – so often over-asserted and over-reified – is actually profoundly important. The reliable essence of a technology (or effective routine) stands in stark contrast to the question-generating essence of scientific research on controversial problems. The reliability on the vaccine side also means that the diverse interests are arrayed around the technology in a complex and effective network that overcomes both political dispute and conflict about the technology to ensure its widespread delivery. The contested nature of competing approaches to reading means that the diverse interests remain in conflict, each making claims based on certain facts and experiences, to counter opposing claims supported in similar ways.

And even when the science is clear, if a reliable technological core of action is not available, the science does not necessarily provide a key to reliable practice. As Hurricanes Katrina and Mitch showed, even when the science is unequivocal from a descriptive or even predictive standpoint, if it cannot be applied to a reliable solution, its ability to drive convergence by diverse interests on appropriate measures to take is limited. Nor do neuroscientific insights into what the brain does during reading yet have much application to the realm of practice – though adherents of one pedagogical approach or another may cite such research to support their preferences.

A related difference is that context strongly affects outcomes in the reading case, and is almost irrelevant in the vaccine case. In some sense this just restates, but from the opposite direction, the absence of a reliable core of action in the reading case. But another way to think about it is that the context-dependence of the process of teaching reading creates all sorts of constituencies that consider themselves experts because they have developed tacit knowledge, judgment, and methods that may have proven effective for them, but which are not easily generalizable. They will seek to make the case that their proven approach works best, and are unlikely to be open to other proven approaches.

Now I want to insert emissions reductions back into the equation, because it seems to me to lie between vaccines on the one side and reading on the other, at least right now. It could well move more toward vaccines; I doubt it will move more toward reading, given historical and technological trends. Nevertheless, solving the emissions problem is a long-term challenge demanding appropriate public and private investments, sound policies, and effective politics. This transition will be driven by the decarbonizing capacity of individual technologies adopted in different ways and in different contexts – not by the effort to manage the global energy system for reduced emissions. A particular decarbonizing energy technology – say, biofuels or solar cells – are to the energy system as a particular vaccine is to the health system. Both systems may be unmanageably complex in terms of the ability to craft system-governing policies to achieve stipulated outcomes, yet individual technologies may nevertheless contribute effectively to particular high-level goals.

Is my whole argument here just an apology for the technological fix? Well, yes and no. Let me put it slightly differently: it would be a good thing if we could recognize difficult problems that were amenable to technological fixes, and distinguish them from difficult problems that are not so amenable. This would help us recognize where we can expect to see fairly rapid progress toward desired goals, and where such progress is likely to be much more difficult to achieve. It would also help us understand where focusing on technological solutions to a problem is unlikely to help, and might even hurt. There would still be room for Neo-Luddism.

My argument also offers some clarity about the meaning of expertise. Because where problems have the attributes of the reading challenge, the claims by experts that they know best about how to solve the problem should be recognized as inherently political. This is especially the case when the prescriptions offered are at the systems level – if we would just pay teachers more; if we could just go to all-year schooling; if we taught only phonics. Cause-effect chains are poorly enough specified at the system level that any scientifically based prescription is in fact policy entrepreneurship. In such domains, science is to be less trusted than technology.

I am not making a case for focusing investments and efforts on technological fixes at the expense of more difficult problems. In the case of adaptation to climate change, for example, we are hugely underinvested in both research and on-the-ground action that could reduce vulnerability to climate. But expectations for significant progress easily assessed over time scales even on the order of a decade or more should be modest. And I want to reiterate that the political failure in our overall approach to climate change has been on both ends of the continuum: we are acting as if vulnerability to climate can be solved by reducing emissions, which is wrong, and we are acting as if reducing emissions is essentially a problem of behavioral change in response to factual information, which is also wrong. These wrongs are compounded because they make the very hard problem – reducing vulnerability – seem easier than it is, and they have made the fairly straightforward (I won't say easy) problem – reducing emissions – harder than it needs to be.

My own reflections on these questions, and the discomfort that they have created for me, can be captured by what one might call the Progressive's Dilemma. If we perceive that a problem is caused by moral or ethical failure, then we want to solve that problem by correcting the failure, which means through behavioral change motivated by a clear understanding of both the moral and the factual elements of the problem. Solving a problem by addressing the underlying social causes seems both normatively and rationally more satisfactory than solving it by introducing a technology that gets us off the hook for our sins. Of course, such a position is a commitment to both moral and scientific absolutism, with all the irony that such a joint commitment entails. More practically, it is also a commitment to long, hard political work whose outcome may never be very satisfactory.

Technological fixes do not offer a route to moral or political redemption. So, for example, we might imagine a technology that can suck carbon dioxide out of the air and pump it into underground storage reservoirs. Such technologies are theoretically and even technically feasible; they may even someday become economically viable for scaling up to allow for direct management of atmospheric chemistry (e.g., Stolaroff et al., 2008; Pielke, 2009). This would rob us of an opportunity to struggle against the aspects of fossil fuel consumption that we might find obnoxious, but if we find ourselves regretting that opportunity then it tells us that our concern about global warming is not simply one of its projected impacts, but of its cultural causes.

I am told, by someone who understands the state of research on machine-neural interfaces, that scientists are perhaps 20 years from being able to directly intervene in the brain to enhance various higher-level cognitive capacities, for example, the capacity to read with facility. Whether or not this is the case, it provides a thought experiment for moving the teaching of reading from the realm of the political and scientific to the realm of the technological and efficacious. Were we to achieve the capacity to download reading ability into any brain, we might regret the lost opportunity that lack of progress in teaching gives us to fight for better salaries for teachers, better home environments for children, or greater equity in wealth distribution. But of course we haven't been making much progress in those battles in the U.S. in any case, and it's doubtful that having a technological fix for teaching children to read would do much damage to those good causes – it might even advance them.

But one thing is clear: Should such technological fixes appear, constituencies holding diverse interests, values, and ways of knowing would surely aggregate around them, because they would recognize a potent tool for acting in the world.

Groups that chose to not take advantage of the reliable effectiveness of the technology – because it offended their values, or contradicted their ways of knowing – would be marginalized and disempowered. This might be perfectly acceptable as a general principle to some groups – consider, for example, the Amish, or other tightly knit, technology-avoiding groups in the U.S. – by dint of the strength of their value systems and coherence of their communities. But as a general matter, taking a principled stance against the use of an effective technology to resolve something widely acknowledged to be a problem is an inherently disempowering action. You are ceding effectiveness to others, whose interests and ways of knowing are now bound up in the use of that technology.

There are plenty of reasons to distrust claims that certain technologies will solve complex problems that have resisted solution in the past. There are probably thousands of software packages that promise to revolutionize the teaching of reading skills, but they have failed to do so, and will continue to fail, because they cannot tame the contextual complexity of the education process. But technological capabilities can also impose a simplifying order on problems that are complex in the absence of such capabilities. For the past 40 years or so, progressive politics has been generally suspicious of technological fixes, perhaps as a reflection of the experience of the nuclear arms race, the growing appreciation of the damage we have done to our environment and the many conspicuous failures of international development aid. But I want to end with a suggestion that some notion of pragmatic technological progressivism needs to be resurrected as a part of any hopeful agenda for enhancing justice, equality, freedom, and even mutual understanding in the world.

I've pointed to four reasons why such a resurrection ought to be encouraged. First, the core reliability embodied in technologies is sometimes ideally suited for making progress on problems that are intractable when approached as political, behavioral, or moral problems. Second, in a world of finite attention and resources, we need to be smart in the way we choose to approach problems. We have not been particularly smart in the political approach to climate change. Third, effective technologies can act as political attractors, bringing together diverse and even conflicting constituencies who recognize a common interest in the outcomes that can be reliably achieved. Technology, that is, may be mobilized as a powerful tool for conflict resolution, for example if applied to disputes over limited natural resources. Finally – and flowing from the previous reasons – a decision to abjure a technology is a decision to abdicate the effectiveness and therefore the power to achieve one's aims that the technology confers. This seems to me like a very poor principle upon which to exercise political action.

Obviously the key is to discriminate: between problems that are amenable to technological fixes and those that are not; between technologies that are well-matched to the essence of a problem and those that are not; between claims of effectiveness that are well-supported and those that are not. Humans are an innovating species. The greatest source of reliable action in human affairs is not our institutions, cultures, or norms, but our inventions. Any approach to solving the many vexing challenges that face the world today needs to include this fundamental, if uncomfortable, reality of the human condition.

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The Political Economy of Technoscience

Astrid Schwarz and Alfred Nordmann

It would seem that the divine hand, both in its treatment of every human being and in its most grandiose workings, is bent on reminding us that the law of equilibrium is the fundamental law of the universe, for it rules everything that happens, all the plants that grow, every creature that breathes

(Marquis de Sade, 1800, 239).

Introduction: On Conservation and Innovation

The following reflections explore the political economies of science and technoscience, philosophically conceived. Accordingly, the intent is not to simply situate scientific activity within the political economy of a society.[1] Instead, we are referring to the management of matter and energy, space and place and the housekeeping principles of researchers when they, literally, account for physical processes. We propose to observe how researchers treat matter or energy and how they negotiate space, surface area, and place, either by accommodating themselves to limits or constraints or by seeking to overcome such limits. This investigation brings to light the underlying assumption in much contemporary research practice of an unlimited technoscientific world of abundance and excess that challenges received certitudes of a limited world that rests firmly and solidly on physical conservation laws and a conception of space as a radically limited resource.

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[1] This is how this term has been used before in Mirowski and Sent (2002), in Mirowski (2004), as well as in Rose and Rose (1976). These authors offered a critique of a pure science that is interested only in truth and trades only in recognition of contributions towards the achievement of truth. Another common approach is to investigate the role of science and technology in economic growth, the relations between science, technology, the state, and capital, and science and development (for instance in Woods, 2007; or also in Martin and Nightingale, 2000).

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In the context of a political economy of society this juxtaposition is far from being new: already in the first half of the twentieth century philosophers and sociologists like Werner Sombart and Georges Bataille contrasted two political economies around the notions of a limited world defined by conservation and an unlimited world that is defined by luxury, excess, abundance (Sombart, 1996; Bataille, 1991). In doing so, they recognized the central role of science and technology in modern economies. Indeed, Bataille goes as far as substantiating his economic conception by referring to scientific models, most importantly the concept of biosphere: "The terrestrial sphere (to be exact, the *biosphere**), which corresponds to the space available to life, is the only real limit" (Bataille, 1991, 29).[2] Because Earth is exposed to a permanent input of solar radiation, there is always an excess of energy. As long as living organisms grow and proliferate and solar energy is properly absorbed, excess is minimal. However, this situation changes once the limit of growth is achieved: "...life [...] enters into effervescence: Without exploding, its extreme exuberance pours out in a movement always bordering on explosion" (Bataille, 1991, 30).[3] In a fully realized biosphere "...there is generally no growth but only a luxurious squandering of energy in every form" (Bataille, 1991, 33). While humanity has successfully extended the limits to growth by investing in labor and technology, it also has immense power "to consume the excess of energy intensely and luxuriously" (Bataille, 1991, 64). These are the assumptions which Bataille develops further when he regards the role of wealth and excess. In his so-called general economy the "limits to growth" open a world of excess, which is an unavoidable aspect of all production and all transformations of matter. In contrast, a restricted or special economy takes "limits to growth" as a challenge to live productively within one's means and to gain surplus value from processing and reprocessing finite resources. But though he drew on scientific ideas to establish his conception of general economics, and though he distinguished the scientific, restricted economy of the economists from his avowedly non-scientific general economy, Bataille did not explicitly articulate a contrast between different ways of conceiving and exploring the world, between a general and a restricted political economy of science.

In the following we expand Bataille's conceptualization to develop the notion of a political economy of science. We then use this philosophical notion to contrast science and technoscience along the lines of Bataille's distinction between a restricted economics and a general economics. We identify the first with sciences that are constituted by conservation laws and therefore implicitly committed to an idea of limits to growth. In contrast, general economy can be identified with the technosciences that appear to adopt a principle of non-conservation, innovation, or infinite renewal – as exemplified, for instance, by the ambition to expand resources like "space" or

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[2] Bataille uses references sparingly, but here ("*"), Bataille refers directly to the author who first conceptualized the term: "*See Vernadsky (1929), where some of the considerations that follow are outlined (from a different viewpoint)."

[3] Translation modified by A.S./A.N.

"matter".[4] We begin by observing an acknowledgment of limits that appears constitutive in eighteenth to twentieth century conceptions of science, and in current attempts to exceed those limits. The classical conceptions of a conservative science appear across the disciplines in physics, chemistry, or ecology. Likewise the tendencies to exceed those limits characterize nano- as well as ecotechnologies.[5] The transition for physics, chemistry and ecology to nano- and ecotechnologies makes especially clear why we speak here of a *political* rather than moral or cognitive economy of the sciences and technosciences. Where one might first suspect that nanotechnologies and ecotechnologies are motivated by very different concerns, they prove to have in common that they defy the notion of a limited world. This commonality lies in their treatment of space and matter, which can be adequately described in terms of a political economy. What makes this a political, rather than a moral or cognitive economy[6] is the choice between adaptation to limits and a conquest of limits. This is eminently political when there is a promise of "green technologies", when sustainability is offered as a substitute to conservation, when there is a search for inherently benign technologies that are safe by design, and when technosciences share the idea of enhancing material nature, be it by using nanotechnology to turn dead matter into smart material, be it through the ecotechnological design of nature for instance in restoration biology or industrial ecology.[7]

By contrasting eco- and nanotechnological research programs with the housekeeping activities of traditionally constituted scientific disciplines, we therefore do not dwell on their obvious differences but highlight the political significance of different research practices regarding the management of matter or energy and space. These practices are not neutral. On the level of the norms of representation and ideals of production that govern scientific and technoscientific research they condition political choices. With respect to the restoration of nature or to global warming this includes the choice between mitigation or adaptation and expansion of capacity through geo-engineering.[8]

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[4] In ongoing debates about the limits of global resources, scientists have identified a "new scarcity" in resource use. They focus especially on "the big three" that are land use change (from cropland to industrial/urban land), emission of greenhouse gases, and extraction of materials (Bringezu, 2009). These "big three" are presented as a technological challenge rather than as a requirement to adapt.

[5] Here we focus on the latter. Ecotechnologies are disciplines like industrial ecology or restoration ecology and other research fields dealing with the modeling and management of resources. As to nanotechnologies see Nordmann (2010).

[6] On the notion of a moral economy of science see Daston (1995). Ernst Mach argued that concepts serve to economize the multiplicity of sensations (Mach, 1959).

[7] The choice between adaption to and conquest of limits is politically salient especially in current debates about the proper response to global warming where adaptionist proposals are countered by the hope that new technologies (including geoengineering) can sustain further economic growth.

[8] We are not claiming that a scientist who works within the narrow confines of conservation laws is thereby committed to the conservation of natural resources. We are claiming instead – though we cannot substantiate it here – that the conduct and principles of the sciences and technosciences condition deliberations about the resources and capacities of planet Earth.

Principles for Economic and Scientific Knowledge

Since there does not appear to be anywhere a scientific dispute about the validity or, indeed, necessity of conservation principles, it sounds strange, at first, that there should be a difference between the conservative sciences and non-conservative technosciences with their pursuit of innovation or infinite renewal.

Though conservation principles are as old as science itself, Antoine Lavoisier's formulation of the conservation of matter holds special place because it provides an obvious case of scientific reason acting as a law-giver to nature[9]: "in all the operations of art and nature, nothing is created; [...] the quality and quantity of the elements remain precisely the same and nothing takes place beyond changes and modifications in the combination of these elements" (Lavoisier, 1952, 41). Lavoisier continues by pointing out that this principle usefully serves as a standard for chemical experimentation and hypothesis-formation: Every experiment must submit to the housekeeping authority of the scale that demands a complete account of the entire quantity of matter before and after the experiment. Only such experiments are admitted and only such hypotheses, that meet this demand (Bensaude-Vincent, 1992).

This reliance of scientific knowledge on conservation principles was challenged by Georges Bataille. As noted above, the economists' restricted or special economy provides an account of supply and demand in a closed world. Just like the Lavoisian chemist, economic theorists represent the world by way of accountancy: Restricted or special economics from Adam Smith to Karl Marx to John Maynard Keynes considers how wealth becomes concentrated and distributed, it looks at the circulation of goods and currencies, at the balancing of cost and price, of demand and supply. The creation of wealth and even of "surplus value" is accounted for in terms of extraction of material and human resources. In contrast, Bataille's general and unrestricted economics celebrates excess and waste and interprets them as gifts of the sun.[10] This creative aspect of excess and waste appears in the thought of economist Joseph Schumpeter who explicitly refers to Sombart (and Nietzsche) when he proposes that "creative destruction" is a basic economic process. But Bataille did not simply propose a different theory but a different and explicitly non-scientific form of knowledge-production. Since he understood the role and function of restrictive conservation principles as conditions for scientific knowledge, he points out that with

[9] The implicit reference to Kant is meant to underscore that conservation principles create conditions for the possibility of representation; we therefore refer to them also as norms of representation. This is not the place to provide a systematic account of how these principles are constitutive of science – where science is taken to aim for theoretical representations of features of the world. For present purposes it is enough if their central, often unquestioned status is acknowledged.

[10] These characterizations do not do justice to the current state of economics as a science and technoscience. Bataille's caricature of restricted economics agrees with classical economics, especially in so far as it aims to become properly scientific by producing general testable models of economic exchange (indeed, the very notion of exchange – as opposed to that of the gift – is based on conservation rather than excess).

restricted and general economics also come two kinds of knowledge. The scientific knowledge of special economics is born from an anxious concern for particular facts and is characterized somewhat stereotypically by coldness and calculation as everything needs to be accounted for. According to Bataille, it "merely generalizes isolated situations" and "does not take into consideration a play of energy that is not limited by any particular end", and it thus does not consider "the play of living matter in general, involved in the movement of light of which it is the result" (Bataille, 1991, 22 f.).[11] This play of energy is excessive in that it exceeds the accountants' balance and produces a surplus. It is therefore not the subject of conventional scientific knowledge and Bataille hints accordingly that he wants to add to the wealth of knowledge even as he must fail at being scientific (Bataille, 1991, 10 f.): A general economics that takes as its model the sun's gift of energy to the earth does not account for the creation of wealth or of knowledge as a mere redistribution or representation of available resources but views all wealth-production, including that of knowledge, as a sign of abundance, excess, and general surplus – as something that must be squandered and cannot be earned. Thus, when Bataille pursues his general economics he follows the movement of energy from geophysics through biology into society not in terms of income and matching expenditure but in terms of excess and destruction. On Bataille's terms then, scientific knowledge like that of Lavoisier depends on the counterfactual construction of special and limited principles of conservation within the more general movement of unlimited energy: Within the thermodynamically open system "Earth", chemical laboratories are established as closed systems for the sake of the scientific presentation and representation of isolated facts. Since the excessive movement of a general economy of nature and society tends to undermine the creation of isolated closed systems and thereby human interests in intellectual mastery and technical control, it also undermines the special sciences that satisfy those interests for better or worse.

It is now possible to see in which sense Bataille's general economy is "non-conservative", namely in the sense in which there are non-Euclidean geometries that include Euclidean geometry as a special case, or in the sense in which Gaston Bachelard speaks of non-Lavoisian chemistry (Bachelard, 1968): Special economics appears as a limited case of a general economics that focuses on the way in which a special economy is constructed, just like non-Lavoisian chemistry follows the material processes of purification and experimental isolation that yield the kinds of substances and representations which then allow us to see nothing but recombinations of elements (Bensaude-Vincent, 1992; Holmes, 1989). From the point of view of general economics or non-Lavoisian chemistry, conservation principles ensure a constancy of nature as a necessary prerequisite for scientific inference and representation. With this conception of conservation principles in mind, one will start seeing them in all efforts to scientifically represent the features and causal processes of the world: Ex nihilo nihil and natura non fecit saltus have dignified Latin names, telling us that nothing can come from nothing and that nature makes no leaps. So, aside from conservation of mass or energy, of charge or angular momentum, there is

[11] Again, we slightly altered the wording of the translation.

uniformitarianism or actualism, there is Newton's first law or axiom of motion, there is the principle of sufficient reason, and there are the so-called inductive principles which posit that the future will be like the past or that nature does not change. All of these speak of the world as a limited whole in which nothing is created and where all change is a redistribution of what is already available. All of these notions are introduced as prerequisites for scientific representation; they are representational norms that structure a domain of phenomena such that objective knowledge about it becomes possible.[12]

The technosciences surrender this supposition of a limited and balanced world – they acknowledge limits only to discover a world of excess and technical possibility within and beyond them. However, if technoscience is to science somewhat as Bataille's unrestricted economics of excess is to a special economics of limits, this does not amount to a scientific revolution or paradigm shift in science where the new paradigm is non-conservative. If the argument so far is correct, there can be no such thing as a Kuhnian paradigm that is not constituted by one conservation principle or another. For the same reason, this shift does not involve a dispute about the standing of conservation laws: By definition, these are pretty much beyond dispute. Whenever technoscientists turn to the business of representation or explanation, they will be careful not to violate the conservation principles that serve as the representational norms in their community. The claim that technosciences are non-conservative does not refer to agreements or disagreements about principles but about the idea of novelty, creativity, perhaps transcendence that is implied in the making and building of things, in the acquisition of capabilities for the control of phenomena, in shaping or disclosing a new world. Instead of a paradigm shift, technoscience stands for an embrace of the technological or constructive character of science – and Lavoisier's principle that nothing is created in art and nature has always perhaps meant one thing for a science of nature which is enabled by its housekeeping practices and something quite different for art or technology that see this principle as a constraint and challenge to probe or even transgress the implied limit to creativity and novelty.

So, while there are various ways in which the technosciences are dedicated to a transgression of limits, we here pursue just one of these ways, namely the pleasureful transgression of a limited or restricted economy of science that assumes finite resources, towards an unlimited or general economy that celebrates the production and consumption of excess.

The Blue Planet – an Ambivalent Icon

The very first photographs of the planet Earth were produced in 1968 during the Apollo 8 mission. These photographs quickly became icons for our notion of Earth as a limited whole, our blue planet as a jewel in the skies, of astounding beauty

[12] See Note 9 above.

and vulnerability, a precious object of care. The rather small spaceship Apollo 8, a carefully crafted ecological cabin in its own right, here encountered spaceship Earth with its precious cargo and limited carrying capacity. This icon assumed a powerful role in the environmental movement and still figures prominently in a discourse of limits – limited resources and limits to growth, limits of space for exploding populations and limits of stability of fragile systems.[13] And yet, this image signifies not only the planet as a self-contained system and bounded space but also as a cipher of exuberance and boundless possibility.

A first indication of this ambivalence is the simple fact that this first photographic representation of the whole planet depended on space travel, and space travel may well be a paradigmatic technoscientific research activity. It encompasses travel into outer space along with travel into biospheres, nanospace or cyberspace where the latter includes the spatial reorganization of our workplaces, recreational spaces or homes through ambient web or ubiquitous computing technologies. All these are highly knowledge-intensive research activities but they do not advance claims to truth or to represent some constant feature of the world. Instead these research activities produce knowledge of basic capabilities of visualization, manipulation, and control. These knowledge claims consist in statements of the sort: look what we have done, where we have gone, how we visualized or modeled something, or what we built. Instead of representing within the limited framework of a zero-sum game and instead of merely transforming some configuration of matter and energy into another (Latour, 1990),[14] this research comes with the promise of genuine novelty, potentiality, and transcendence – if we can do this, then maybe we could also do this, and if we succeed, there will be more than there was before and maybe enough even for everyone to share and to create a win-win situation that knows no risks and no losers. Such promises attend any kind of space travel since it is supposed to disclose new opportunities and to make room for everyone. The first visual encounter, then, with the blue planet as a limited whole took place during a technological endeavor to surpass this limited whole.

What Does Earth Do with the Energy It Receives?

As we explore this ambivalence, it is worth noting that the startling radiance of planet earth came to the fore within a discourse on limits long before the environmental movement and that the encounter with the blue planet did not require the photographic opportunities that came with the Apollo mission. In 1885, physi-

[13] For a comprehensive history of “spaceship earth” as an icon of the environmental age, see e.g. Höhler (2008).

[14] When Latour points out that science is no zero-sum game, he does so to dissolve the conceit that science serves to represent a given world. According to Latour, all science turns out to be technoscience precisely in that humans and nature come together in the laboratory to create something new.

cist Heinrich Hertz delivered his inaugural lecture at Karlsruhe university where he asked the question “what [. . .] does the Earth do with the energy it receives” from the sun?

> First, some of it is reflected back as light of unchanged form. One may doubt whether this part should really be considered as part of the energy resources of the Earth. But since our understanding of the total balancing process, which is similar to a budget, represents no more than a general picture, we can therefore say that this reflected energy must be considered part of the energy income utilized by the earth for illumination. This energy enables the Earth to circle the Sun not as a dark, invisible mass, but to stand out as a bright star among the other planets; from them Earth can be observed just as well as the other planets are visible from Earth. It represents, so to speak, the astronomical upkeep allowance of the Earth. [. . .] Since this is a large amount, we may conclude for this reason that the illumination of the Earth is a brilliant one (Hertz, 1997, 40).

Heinrich Hertz is here considering a “total balancing process” which balances income against expenditure. As a physicist he is openly engaged in economic thinking and acknowledges this by likening his work to drawing up a budget. Indeed, the German title of the lecture refers to the “*Energiehaushalt der Erde”* which refers to the housekeeping that lies at the root of the very notion of “economy” and which makes evident that the “energy balance of the earth” results from a balancing of books and the equality of total income and total expenditure. As if to make sure that this is understood to be far more than a metaphor, Hertz follows the logic of budgeting to account for that part of the income of the sun that is reflected by the Earth’s atmosphere and he refers to this as the cost of illuminating the Earth in a particularly brilliant way. The English translation speaks here of an “upkeep allowance” which does not quite capture that Hertz alludes to an excessive expenditure that reflects and represents the enormous wealth of the Earth – the German expression for this expenditure is “*Repräsentationskosten der Erde*”, that is, an upkeep allowance for a royal court which powerfully represents itself by way of luxury and conspicuous waste.

Significantly, Hertz does not leave the framework of strict accountancy even as he exhibits an abundant wastefulness where the Earth writes off a third of its energy supply in order to keep up a good appearance. Indeed, Hertz expresses here only more clearly a political economy that manages matter and energy strictly in terms of conservation – in a limited world with limited resources nothing is created and nothing destroyed but everything becomes redistributed such that income and expenditure even out. Life within his world is a zero-sum game that revolves around trade-offs, benefits at a cost, winning at the expense of the losers. This is the general picture that Hertz is talking about and which forces him to account even for frivolous expenditures. However, though Hertz is forced by his bookkeeping method to account for all the energy and matter in the system under consideration, it is he who determined the boundaries of that system – he is looking neither at the earth, nor at the solar system, and he is also not drawing a box around the sun and the earth, but instead accounts for that part of the sun’s energy that is “intended for the earth” as well as all the energy that is already stored up in the earth (Hertz, 1997, 41; Kind, 2005; Pelkowski, 2008).

Quite in agreement with Wise and Smith's work on energy and empire (Wise and Smith, 1989), Hertz acknowledges that his housekeeping principles derive from considerations of the steam-engine as a technical system. Indeed, he refers to the steam-engine many times and likens the entire atmosphere, the whole earth, but also the sun to giant steam-engines (Hertz, 1997, 41f., 43). One decisive characteristic of a steam-engine is that it is more or less efficient but that there is always a loss in the conversion of heat into work. Accordingly, he finds for the energy balance of the earth as a whole, for any specific machine, for a biological organism or a human life that incoming energy is converted into useful work but that energy will dissipate and become useless without getting lost over the course of this and further transformations. So even aside from the energy used to illuminate the earth, actual conversion processes always begin and end with luxury and waste: They begin with the largesse of the sun that generously squanders so much of its energy and ends with the dissipation of now-useless energy. But along the way the conversion of energy keeps the earth going and our steam-engines running, and as one moves between these scales, one becomes aware of the "insignificance of men in this economy" (Hertz, 1997, 44).

Even as he considers technical systems, Hertz's political economy and impersonal system of housekeeping is orientated to the epistemic demands of science, or, more particularly to practices of representation. That Hertz himself was quite aware of these demands is testified throughout the work of this philosophically astute physicist who had reflected in an earlier lecture-series on the constitution of matter and the status of conservation principles like that of the conservation of mass (Hertz, 1999). In this book, Lavoisier appears as the founding father of modern science. Like Lavoisier, Hertz recognizes conservation principles as constitutive of scientific practice. He would have agreed with Larry Holmes and Bernadette Bensaude-Vincent who showed that Lavoisier structured the modern chemical laboratory through his proposals to institutionalize the conservation of mass in his apparatus and the associated employment of the scale: Lavoisier founded a political economy which establishes a specific manner of book-keeping, of evaluating the exchange of matter. Accordingly, Heinrich Hertz pursued the question whether this conservation law is a law of nature that is true for all things at all times, or whether it is an a priori principle or representational device that underwrites practice and must not be abandoned even where it would appear that there has been an increase or loss of total mass (Hertz, 1999, 115–116). Without it, at any rate, a certain kind of scientific knowledge would not be possible.

Following Hertz, this conservative picture of the world as a precondition of scientific knowledge was epitomized in Ludwig Wittgenstein's *Tractatus logico-philosophicus* and it figures centrally in Emile Meyerson's *Identity and Reality* (Wittgenstein, 1922; Meyerson, 1962). Though none of these scientists and philosophers were motivated by a concern for nature conservation or the fragility of ecosystems, it is easy to see that their way of thinking about limits that are constitutive of nature as an object of science gives rise to the injunction to live within our means and to accommodate ourselves to limited resources. Technology, on this account, is above all an ingenious way to achieve more with limited means, and science might discover, to quote Hertz again, “roundabout ways, in which we can so direct the general flow of energy that [our machines] correspond to our established goals” (Hertz, 1997, 39).

From “Obedience” to “Transgression”

On the account presented so far, controlling and managing the flow of energy and matter is a concern that is virtually at the center of science and technology in the modern world. For the scientific enterprise, the conservation principles ensure the constancy of nature and thus enable scientific inference and representation. And it is assumed that only this constancy and lawfulness of nature underwrites the technological enterprise. This conception of technology as applied science implies that technological ambitions and experimental creativity will always be constrained by a scientific world-view. The prohibition of a perpetual motion machine is only the most evident example of this. Lavoisier’s verdict about the limits on art and engineering and his view of experimentation emphasize that the scientist must literally surrender to the verdict of the experiment. Nature is invited into the laboratory as a witness who provides answers to our questions. Scientists, artists, engineers thus learn from nature not how to do or build things but which of their ideas are in accord with it.

But the supposed impossibility to create genuine novelty also produced ambivalence which found expression in formulations like these: “The victory over nature can only be achieved by way of obedience towards it” (Cassirer, 1985, 60). If nature can be used for scientific and technological purposes precisely because of its lawfulness, do we therefore have to surrender to the world as it is or can we still overcome nature and liberate ourselves from our natural condition – just as the conquest of outer space was seen as an attempt to leave behind our earth-bound existence (Arendt, 1998, 1)? Bacon’s conception of the experiment as a new style of innovative practice reflects this ambivalence. It expressed an experimental spirit that demonstrated its power not only of determining what is and what is not in accord with nature. At the same time, the experiment appeared as a technology for innovation, as a tool for transgressing given natural limits.

In the classical or conservative idiom of “science and technology” the scientific assumption of a limited world sets limits for technology and experimental practice. Despite the noted ambivalence, it took a long time until the inverse relation received recognition: Only in the fairly recent idiom of “technoscience” the unbounded creative potential of technology sets the expectation that the world, too, is unlimited.[15] Accordingly, the idea of overcoming nature that is associated with creative experimental interventions comes to the fore. Instead of Lavoisier, it is now Francis Bacon who is claimed as a founding figure and becomes idealized in a one-sided way: Today’s accounts of the Baconian experiment emphasize a spirit of creative experimentation that by and by conquered modern societies as

[15] For a strong claim regarding this inversion see Forman (2007).

it was adopted by artists, engineers, instrument makers, or social reformers. All of them share a creative desire in designing machinery, creating artwork, exploring the globe, or changing society. And indeed, even the idea of a perpetual motion machine makes its reappearance here and there (Dietzel et al., 2008).

The Power in the Earth

We will now take a closer look at this shift from the scientific conception of lawfulness and constancy as a limit on engineering to engineering practice as a model for the ability to exceed limits, including those that appear natural. This inversion is particularly evident in respect to the notion that there is a limit of space on Earth that constrains human civilization, including technology.

Economist Thomas Robert Malthus was convinced that man cannot transgress the absolute limit given by nature and in saying this he referred not only to limited space on earth but also to other resources. In his famous "Essay on the Principle of Population, as it affects the future improvement of society" (Malthus, 1798) he provides a clear and seemingly inescapable account that has not lost its power until today. Until at least the 1970s and probably beyond that, Malthus' population model fed into scientific ecology. It also influenced ecotechnologies like the "cabin ecology" of the 1950s and 1960s and the development of the biosphere in the 1980s. Today, it serves as a basic assumption of ecological economics – an exemplar, to be sure, of what Bataille called special or restricted economics (Becker et al., 2007, 275–299). So, what does the so-called Malthusian law say and what makes it so particularly seductive for ecological concerns? It draws a causal connection between the growth of population, space as a limited resource and the availability of food production. In Malthus' logic, the scarcity of food resources is the absolute limit for societies, a scarcity that is implied by a lawful nature.

> I say, that the power of population is indefinitely greater than the power in the earth to produce subsistence for man. Population, when unchecked, increases in a geometrical ratio. Subsistence increases only in an arithmetical ratio. [. . .] By that law of our nature which makes food necessary to the life of man, the effects of these two unequal powers must be kept equal. [. . .] Nature has scattered the seeds of life abroad with the most profuse and liberal hand. She has been comparatively sparing in the room, and the nourishment necessary to rear them. The germs of existence contained in this spot of earth, with ample food, and ample room to expand in, would fill millions of worlds in the course of a few thousand years. Necessity, that imperious all pervading law of nature, restrains them within the prescribed bounds. [. . .] And the race of man cannot, by any efforts of reason, escape from it (Malthus, 1798, 13–15).

This law of nature, according to Malthus, is the immutable condition for the economy and governs the relation between humanity and nature.

> It accords with the most liberal spirit of philosophy, to suppose that no stone can fall, or a plant rise, without the immediate agency of divine power. But we know from experience, that these operations of what we call nature have been conducted almost invariably according to fixed laws. And since the world began, the causes of population and depopulation have probably been as constant as any of the laws of nature with which we are acquainted (Malthus, 1798, 127–128).

Moreover, the scope of action for accommodating the "great machine" society to nature is rather small because the lawful order of society is ultimately based on the force of human "self-love" which is again given by nature. The Malthusian society cannot change the actual relations between the rich and the poor, it is not even disposed to imagine societal change.

> [...] [A] society constituted according to the most beautiful form that imagination can conceive, with benevolence for its moving principle, instead of self-love [...] would, from the inevitable laws of nature [...] degenerate into a society, constructed upon a plan not essentially different from that which prevails in every known state at present; I mean, a society divided into a class of proprietors, and a class of laborers, and with self-love for the main-spring of the great machine (Malthus, 1798, 207).

These in a sense anti-social elements of Malthus' philosophy might disguise the originality of his economic model, namely the identification of nature as the resource of society which still informs economic and ecological thinking today. The ground for this persistence is that nature is imagined as a given and unchangeable source that stands in opposition to societal actors in society, including the trader and economist. Societies have to accommodate to this nature and the limits of technology are imagined accordingly – nature cannot possibly be conceived in a technological manner. Malthusian nature, characterized by an unavoidable logic of power and balance, shares its fundamental assumptions with other conservation principles. Both, the relation between economy and nature, but above all the economy of nature itself is construed as an inescapable necessity. An order is projected onto nature and this order corresponds to the form of scientific laws and of economic processes alike.

It is therefore hardly astonishing that the first and second laws of thermodynamics have always played an important role in scientific ecology.[16] "The basic process [...] is the transfer of energy from one part of the ecosystem to another" wrote aquatic ecologist Raymond L. Lindeman, when he first described a lake as an energetically open ecosystem consisting of biotic and abiotic components (Lindeman, 1942, 400). Energy from the sun is accumulated in organisms, so-called producers, by means of photosynthesis. A portion of this energy is transferred via consumption to the next levels, but most of the energy is lost either by respiration or decomposition. This first description of the transfer mechanisms in an ecological system in terms of the laws of thermodynamics was further developed in ecosystem theory and thus became an important conceptual reference in ecological economics as a scientifically certified description of nature.

Lindeman's model was by no means the first one to conceptualize natural systems outside the laboratory as quantitatively recordable entities. In 1926 already, Vladimir Ivanovich Vernadsky published a paper "on gaseous exchange of the earth's crust" in which he treated geochemistry as a natural history of terrestrial

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[16] This paper is not the place for presenting the argument in detail. Georgescu-Roegen is one of the best known scholars who relied on the work of systems biologist Bertalanffy. Bertalanffy, in turn, began his career in the 1930s thinking about systems biology by adopting the two laws of thermodynamics to biology and transforming them into principles of Gestalt.

chemical elements. This geochemical approach turned the whole globe into a scientific object, and from this Vernadsky's concept of the biosphere derived its heuristic power.[17] The idea of control and balance is everywhere in play, since the objective of quantitatively describing the transfer of substances through a system is pursued by conceptualizing a biologically controlled flow of atoms in a specific geological site.[18] "All points oscillate around a certain fixed mean" was one of Vernadsky's central statements that clearly expresses conservation principles (Vernadsky, 1997, 225–227).

Regulatory feedback mechanisms played an important role in Vernadsky's model; they structured conceptualizations of cyclical processes. In the 1940s this geochemical approach found its way into more general efforts of systems analysis in the context of the famous Macy conferences (1946–1953). The inaugural meeting of the group was called "feedback mechanisms and circular causal systems in biological and social systems."[19] The very idea of a cyclical process and self-regulating feedback mechanisms constitutes a variant of conservation thinking – it serves as a norm of representation that constitutes scientific practice and also constitutes a specific scientific object, namely a kind of system, including the ecosystem. Just like Hertz's steam engines or the post-Malthusian systems of agriculture, these systems can be more or less efficient in that they use the available space or the available energy more intensively. This intensification takes place strictly within the circulation of matter and energy. Malthus was "proven wrong" only because he underestimated what intensification could do, but on this account he is still considered right, in principle: There is a limit to intensification and this limit brings us up against Malthus' unyielding, unforgiving nature. Some of the participants of the Macy conferences testify to this as they went on to develop what was later called General Systems Theory (GST).

Aside from neurophysiologist Ralph W. Gerard and ecologist George Evelyn Hutchinson who was also an important supporter of Lindeman's thermodynamic ecosystem concept,[20] other major contributors to GST were biologist Ludwig von Bertalanffy, economist Kenneth E. Boulding who advocated in the 1960s the concept "carrying capacity," and biomathematician Anatol Rapoport whose thermodynamic models influenced ecological economics in the 1970s. Hutchinson's seminal paper on "Circular Causal Systems in Ecology" shows clearly that his

[17] The concept had already been invented by geographer Eduard Suess, but it was only Vernadsky who conceptualized the biosphere as it was taken up by Bataille and as we know it today.

[18] For a more detailed looking at Russian ecology through the biosphere theory, see Levit (2010, ch. III.4.6).

[19] The Macy Conferences (with participants such as Norbert Wiener, John von Neumann, Warren McCulloch, Margaret Mead, or Heinz von Foerster) contributed decisively towards the dissemination of cybernetic approaches beyond primarily technological applications into areas such as psychology, ecology, and in general the human and life sciences. For more detail see Pias (2003).

[20] The formation of the GST is described in Gray and Rizzo (1973).

"systems" are scientific objects that are to be studied and represented by scientific ecology. He developed an ecological theory using cybernetic terms of feedback mechanisms and circular causality, arguing that, within certain boundaries, ecosystems are "self-correcting" by means of "circular causal paths". The assumption of those regulating feedback systems as ecological theories forms the basis of both his biogeochemical and biodemographic approach.[21] Abiotic and biotic factors alike are looked at from the point of view of the extent to which their effect is to stabilize the equilibrium (Hutchinson, 1948, 221–246).[22] The carbon cycle for instance can be described as being adjusted by the regulating effects of the oceans and the biological cycle.

By means of these powerful theoretical tools, ecology had become the authorized science to describe and explain not only the environment of a single organism, of populations or communities but also geographically larger systems, including earth as a whole. Thus, ecological theories seemed to provide the ideal tool box to manage any sort of environments, just as cybernetics and general systems theory provided a tool box to understand, manage, perhaps optimize the behavior of machines and other technical systems. In this transition from understanding to managing systems, however, "system" became an ambivalent term with a scientific as well as technical dimension. On the one hand, "system" served as a general representational device for describing and explaining nature and technology as self-contained, conservative, cyclical and self-regulatory processes. On the other hand, if nature shares with technological systems that it operates in a certain way, this leads to a technical notion of functional systems with performance parameters that can be managed, adjusted, optimized. The powerful promise of GST thus involved a shift from theoretical ecology, based on mathematical modeling, to issues of controlling and managing systems that contain living organisms – a shift from scientific ecology to ecotechnologies such as "space biology" or "cabin ecology."[23]

Exceeding the Limits to Growth

We have seen that feedback-cycles and self-regulation played an important role in the development of systems theories. The notion of "self-organization" is sometimes identified with this and sometimes implies an added dimension of emergence and

[21] Astrid Schwarz offers a closer look at the beginnings of systems theory in biology and ecology with a special emphasis on the concept of Gestalt (1996, 35–45). A detailed story of systems theory in early ecology is given in Voigt (2010, ch. III.3.1).

[22] George Evelyn Hutchinson participated in a number of Macy conferences and published in 1948 the paper "Circular causal systems in Ecology."

[23] On the subject of "space biology" see, for example, Hanrahan and Bushnell (1960), as well as a host of magazine articles in, among others, *Missiles and Rockets*, *Astronautics*, *American Biology Teacher* or in the *British Interplanetary Society Journal*. On "cabin ecology" see especially Calloway (1965) and Calloway (1967).

creativity. In contrast to the conservative general systems of the cyberneticists, self-organizing systems are said to create genuine, often surprising novelty – they take the system to a new level and move beyond intensification to innovation.[24] On the one hand, then, self-organization harks back to the model of a well-balanced and rather conservative nature that accommodates itself within given limits. But on the other hand self-organization opens the door to an image of nature that appears to be emergent and creative. The corresponding model is based on a political economy of technoscience that takes the seemingly unbounded technological creation of genuine novelty as a paradigm of nature. Technoscience does not accommodate itself to a limited world but seeks to expand those limits by disclosing new space and new resources.

Space travel like the Apollo program serves to disclose new space and new resources, and it does so by way of conspicuous consumption and – some would argue – an orgy of excess: The resources invested in the Apollo program cannot be accounted for; perhaps they are wasted or perhaps they bring infinite gain, and in the meantime they might be written off as a kind of national fireworks that deliver glorious pictures of the galaxies and the blue planet earth. On the level of research, this program was taken up by cabin ecology and biosphere design. Technoscientifically, the disclosure of new space and new resources corresponds to the construction, literally, of space-ship cabins that enable the discovery of new worlds beyond the biosphere. The idea behind "exceeding containment" was to construct a closed space that would be suitable for the maintenance of life and thus help to escape earthly confinement. What was to be created, then, was a perfectly controlled space at the limits of intensification – self-sustaining without loss as nearly as possible. This exercise in total control served to minimize reliance on the special conditions of life on Earth and to go beyond the absolute limit of space that was set by the biosphere.

All this can be seen in the story of the emergence of cabin ecology as a field of research with legions of technicians and scientists working on the technical and conceptual implementation of water, nutrient and gas cycles. This serious scientific-technological research program began in the 1950s with the dream of developing outer space as an unlimited spatial resource by establishing human settlements in Earth's orbit or even colonizing Mars.[25] The technical conception of constructed ecosystems for space travel took on added significance when in the 1960s the entire

[24] To do justice to this claim, one would need to take a close look at the role of conservation principles in the argument e.g., of Prigogine and Stengers (1984). To be sure, in order to scientifically represent and explain self-organizing systems, such principles will have to be evoked. And yet, these systems signify that nature can grow beyond itself and the emergence of order thus recalls nineteenth century arguments about a dynamics of nature that eludes the mechanics of representation.

[25] See, for instance, Clarke (1951). The program still has strong technological as well as imaginary potential. It plays a role in recent space experiments as well as trend-setting "eco-design" prototypes. A good example for the first case is the ongoing research project to develop "aquatic modules for biogenerative life support systems: Developmental aspects based on the space flight results of the C.E.B.A.S. mini-module" (Blüm, 2003). For an eco-design product see the air purifier "Bel-Air" (2007), developed by Matthieu Lehanneur and David Edwards (Harvard University

planet became visible as a spaceship that needs to maintain conditions of life for a human population. “Spaceship Earth” was no longer associated with space travel but increasingly with the emerging environmental discourse. The 1968 Apollo image of the blue planet brought into view not only the Earth as an enclosed and, above all, limited space but along with that the various scientific parameters for describing space (closed-loop cycles, stability, “carrying capacity”, and so on). Thus the “spaceship” became the rational model for the global management of Earth, but one in which humans could suddenly turn into an irritant by producing too much CO_2 or waste. Humans became a form of “pollution” on Earth, spreading like a disease and putting Gaia in mortal danger – as ecologist James Lovelock put it (Lovelock, 1996). With economist K. E. Boulding the “spaceship” underwent a transformation. The actual, technical model of space-travel for astronauts was now projected onto the planet as an object of management. Boulding turned the cabin or spaceship into a macroeconomic model in which carrying capacity played a major role and the limitation of space became identified with all other resource-limitations: “the earth has become a single spaceship, without unlimited reservoirs of anything, either for extraction or for pollution, and in which, therefore, man must find his place in a cyclical ecological system which is capable of continuous reproduction of material form even though it cannot escape having inputs of energy” (Boulding, 1966, 34).[26]

This “economy of the spaceship earth” came to underpin the concerns expressed in the Club of Rome report on the “Limits to Growth.” And as with cabin ecology, in particular, the envisioned control by a few parameters of spaceship earth and of planet earth as a total world model implies a form of excess. Travel into outer space, the current conquest of nanospace, and this project of managing the blue planet share the idea that space itself can be used to exert technical control. Within the conservative framework of an absolutely limited Malthusian earth, the notion of “carrying capacity” equated available surface area with available space. For example, alarmist images of how much standing room is taken up by all the inhabitants of the Earth translated into political calls for population control underlined by scientific models. The use of space for technical control came into its own when available surface area became divorced from available space with the notion of the “ecological footprint.” This notion also serves to send alarmist messages about the land use required to sustain a single citizen of the US or of India. The measure of the ecological footprint signals that we live far beyond our means. At the same time, somewhat paradoxically, it also signals that we can live far beyond our means: The sum of ecological footprints already exceeds the available surface area on Earth by a factor of 1.4 – and it is simultaneously the worry of limits-to-growth environmentalists and the hope of technoscientific researchers that this factor will become bigger in years to come. One way of doing so is to productively exploit the fact that at the nanoscale surface area is immensely large in relation to bulk. Ever since Richard Feynman’s call in 1959 to enter a new field of technological possibility by

and Le Laboratoire Paris). It is based on a technology that was originally developed by NASA to improve the air quality on board space shuttles (Barbera and Cozzo, 2009, 56).

[26] See also Höhler and Luks (2006).

discovering “plenty of room” at the bottom, this nanotechnological project is not viewed as a more intensive exploitation of an available resource but as the discovery of an entirely new space of action that permits a form of engineering which draws on the creative processes of nature.

While excess in molecular biology or in nanotechnology involves shaping the world atom by atom or molecule by molecule, ecotechnology produces excess through manipulation and enhancement of the cybernetic world machine. Today, scientific expertise about the limits to growth serves as a starting point and technological challenge to the so-called sustainability sciences and related technological fields which are primarily concerned with the control, discovery, and constant renewal of resources. The declaration of the recently founded World Resource Forum is a good example for this kind of agenda: “Traditional environmental technologies are no longer enough [. . .]. We call for a new global strategy for governing the use of natural resources [. . .]. By combining efficiency and resource productivity targets with sufficiency norms evolved through participative mechanisms, it should be possible to avoid the traditional type of growth.”[27] This is a conceptualization of limits that already points at its transgression and therefore exhibits a similar ambivalence as the notion of the self-regulating system. The World Resource Forum asserts that the acknowledgment of limits of resources creates possibilities for escaping these limits by means of efficiency in the sense of enhanced systems performance. This kind of efficiency is to result not primarily from conservation and the avoidance of waste but from technological as well as societal innovation (“participative mechanisms”).[28] This program corresponds to a new environmental movement that embraces technological innovation and that refers for this, in particular, to the luxurious gifts of energy from the sun: “We should see in hubris not solely what is negative and destructive but also what is positive and creative: the aspiration to imagine new realities, create new values, and reach new heights of human possibility.”[29]

Conclusion

Are we confined to Venadsky’s conservative biosphere or does the generous gift of the sun produce an abundance and concentration of wealth that needs to be released in the form of excess, waste, and creative destruction – such that the technological problem of sustainable development is the control of how this release takes place:

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[27] For more detail see www.worldresourcesforum.org/wrf_declaration (15 June, 2010). The WRF was founded in Davos, Switzerland, in September 2009.

[28] A vivid illustration of this was provided in a large exhibit curated by the German Max Planck Society for basic research. It presented as a point of departure a reminder of resource limits. From then on, however, it featured the power of the technosciences to go beyond these limits: “we must grow beyond ourselves” (Max Planck Gesellschaft, 2009, 181 and 187).

[29] Comment by Richard Florida (author of *Rise of the Creative Class)* on Nordhaus and Shellenberger (2006). For this and more such statements of praise see www.thebreakthrough.org/pressrev.shtml (15 June, 2010).

by way of exuberantly rising ocean levels, by grandiose geoengineering schemes, or by ever more “sustainable” production and consumption? Do we accommodate ideas of technological possibility within the framework of knowledge production in the special, restricted, “limited” sciences, or do we view technoscientific research as a productive, creative, “liberating” force of wealth-production? These questions return us to Georges Bataille’s reflections on restricted and general economics: How can we conceptualize the transformation from a limited world of scarcity to a world of excess. And can we control the transformation from a special economics of zero-sum games and of supply balancing demand, to a general economics of luxurious abundance and abject waste?

This essay on some of the transformations undergone by the “blue planet” and “Spaceship Earth” allowed us to simultaneously consider ecotechnologies and nanotechnologies as technosciences that do not accommodate to limits. In both cases we are dealing with space travel and the control of space as a technical resource (Nordmann, 2004; Schwarz, 2009). Ecotechnologies and nanotechnologies accept and incorporate arguments about limited growth and in response develop strategies of control that open up a boundless space – literally and metaphorically – of technical possibilities, for example by discovering vast new surface areas (nanomaterials research), by developing new forms of energy (hydrogen economy), by harnessing morphological and organismic potential (synthetic biology), or by designing the renewal of nature (restoration ecology).

There are various ways in which the technosciences seek a transgression of limits, for example, through the production of hybrids. Here we were interested in just one of these ways, namely the transgression of a limited or restricted economy of science that assumes finite resources and finite energy, towards an unlimited or general economy that celebrates the production and consumption of excess. This may have led us to the origin of technoscientific hype and hubris. More importantly, however, it led to a condition where the norms of representation that orient the sciences no longer shape our ideas of a constant and limited world. Instead, the explorative aspects of experimentation and the creative dimension of art and engineering provide an image of boundless technical innovation which suggests that the world itself is constantly renewable and an unlimited source of novelty.

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Science, the Public and the Media – Views from Everywhere

Peter Weingart

Relationships Between Science and the Public

The poster, which shows the old Albert Einstein with his violin case standing in front of Marilyn Monroe seated on a park bench in the pale moonlight, triggers an involuntary reaction in our minds, almost like a picture puzzle. Are we amused or irritated by the discrepancy between the intellectual crankiness of the professor and the "sex appeal" of the movie star? Are we reluctant to imagine Einstein becoming romantically involved with a sex-bomb? The relationship of science towards pop- or mass-culture and thus to the public in media democracies (represented here by Marilyn) is awkward (although Einstein himself was – exceptional for scientists – a media icon). It does not comply with the relationship of science in the feudal society of the seventeenth and eighteenth centuries which was characterized by irreverent submissiveness and the courting for the attention of the ruling class. Nor does it comply with the relationship of science in the bourgeois society of the nineteenth and early twentieth centuries in which the bourgeoisie, anxious for knowledge, was able to participate in the progresses of research with the help of their popularizers. The decisive change in the relationship between science and the public began when modern science could be considered as fully differentiated, i.e., since scientific communication was closed to the outside and became self-referential. Science was financially and institutionally dependent on state and society from the beginning, but this dependency has changed in its character. On the one hand, the contents of science are no longer derived from everyday experience but constituted in the disciplinary communication processes in highly specialized languages no longer understandable to the lay public. On the other hand, modern societies have developed into mass-democracies in which the addressees of scientific knowledge and appeals to fund research are no longer merely the educated, but the entire electorate for which the politicians have to legitimize their policies.

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The electorate, i.e., the general public, does not necessarily have a genuine interest in education and enlightenment, but first of all a pragmatic interest in the results of research with regard to its practical needs. Science's promise for progress has created expectations and demands, and the world is permeated by science and technology in a way that it is impossible to go through life without using scientific knowledge.

The public of mass-democracies is almost exclusively represented and continually updated by the media. Everything we know about the world, and thus about science, we know through the media (Luhmann). Due to the central standing of the media in the public discourse and in determining the political agenda, the primary interest in this essay is the role of the media in the communication with science. Since the relationship between science and the public is one of mutual dependency, it is necessary to look at the respective perceptions. How do science and the public perceive each other, what kind of expectations do they have of each other? How do they attempt to realize these expectations and what are the consequences of those attempts? The repercussions of the mutual perceptions on science proper, the attempted adaptations as far as science is concerned, are called "medialization".

In the following I will concentrate (1) on some examples of perceptions of science held by the public, (2) on particular examples of perceptions and attempts by science to influence the public, and, finally, on some aspects of the effects these attempts (may) have on science itself.

Science in the Perception of the Public

One source of perceptions of science held by the public are opinion polls. Various surveys have all come to the conclusion that trust in institutions in a given population is generally decreasing, but that of all institutions science is regarded as the most trust-worthy. This also holds for international comparison, for example between the USA and Germany (Peters et al., 2007) or the EU countries. The general finding, however, is of little significance as further inquiries quickly show. For example, it can be observed that the interest in science and technology increases with age and length of education. On the other hand only a third of the population feel that they are informed with regard to science. The highest interest is in medicine and environment because these fields are of direct concern to people. Optimism regarding the role of science can thus be found especially in the healing of diseases and relief of every day life (80.5%; 70.7%). The consideration of advantages and disadvantages of science, however, leads to a slightly positive result (50.4%). In the regularly EU-conducted survey it is striking that the answers are dependent on the level of education but also differ considerably between individual countries. Here, it is assumed that there is a relationship between the predominant values and the attitudes towards science. The originally assumed simple relationship between state of knowledge and positive attitude towards science is not as simple as it was thought to be. The analogous relationship between level of industrialization of a country and positive attitude does not fit either. It is rather the case that the positive attitudes and expectations regarding science can especially be found in countries that are in an early stage of industrialization (for example, in the East European countries). In contrast, scepticism, critique and lack of interest are more prominent in highly industrialized countries. Even in this pattern there are unexplained differences since, for example, the Danes are much more optimistic than the Germans even though the degree of industrialization is practically the same in both countries. In addition, the attitude is dependent on concrete occasions, i.e., on the kind of question asked. Thus, 88% of Cypriots consider gene manipulated food as dangerous, but only 30% in the Netherlands do (EU-Kommission, 2006). Meanwhile it is doubted that there is a widespread anti-science and anti-technology attitude in Germany. Rather, this notion is presumed to be a construct of the political discussion of the 1980s (vgl. z.B.Kistler, 2005; Renn, 2005).

Observers agree that the perception of science in the public is difficult to grasp by these kinds of surveys. However, as a general conclusion it can be said that the attitudes are the more ambivalent, based on experience and interests, the more concrete the occasions or themes are, not least because abstract science does not interest the majority of the public.

A similar picture emerges when the questions are aimed at the perception of scientists. Here the ambivalence towards the institution of science complies with the stereotypization of its protagonists. The "Draw-A-Scientist"-tests, first conducted in 1957 by Margaret Mead with high-school pupils in America, revealed aside from the widely shared descriptions (the scientist is a man, wearing a white garment, glasses, having a beard or is unshaven etc.) an ambivalent perception of negative as well as positive images. In contrast to the negative images, which also have a stronger presence, the positive images are without any relation to the career dreams of the children (Mead and Metraux, 1957). The DAST-research has shown the amazing stability of the stereotype which is already developed in elementary school. Later, it changes towards a more positive one only if higher levels of education are attained (NSB, 2002, Chapter 7). More recent research on stereotypes of scientists came to the same conclusion. They are still perceived as an elitist group obsessed with their work, as older men who do not have a family, are intelligent and of cool rationality whose work is often dangerous and bound to fail (Vilchez-Gonzalez and Palacios, 2006: 241; Schibeci, 1986).

A second source for the perception of science in the public are the popular entertainment media, in particular motion pictures and comics. It can be assumed that they reproduce and strengthen the clichés and biases held by the public. In fact motion pictures represent stereotypes which can be traced to the myths of antiquity, such as the legend of Prometheus, and which have been handed down by literature. The alchemist *Doctor Faustus* is the archetype, followed by literary figures such as *Dr. Frankenstein*, the first of the mad scientists, as well as *Dr. Jekyll* and *Mr. Hyde*, *Dr. Moreau*, *Dr. Caligari* and others. They all have the strongest of all myths in common: the creation of artificial life. "The achievement of the mechanical creation of human life – or even of life at all – looks like a culmination of the acquisition of knowledge and the power that this knowledge brings. Most societies have set definite limits to this extension of human knowledge; modern Western society has been distinguished in trying to obliterate this limit. But the old limits still exert their power and arouse a certain dread of what will be found beyond these limits" (Back, 1995, 328).

This ambivalence towards science and technology can also be traced to comic books. Even the funny/satirical animal stories (Donald Duck) describe, aside from the promises, the cases of failure of the engineer/technician who loses control over his inventions. The often unnecessary complicated inventions of Gyro Gearloose are contrasted with the down-to-earth and nature-loving character of Grandma Duck who finds natural sciences to be "unnatural" (Kagelmann, 1976, 125). The technical progress seems too complicated, the visions of a future over-technologized world are ambivalent, if not negative (Weingart, 2008).

The representation of science in the popular entertainment media, thus, indeed shows the same stereotypes and ambivalences that were revealed by the DAS-tests. In the surveys they are indirectly mirrored with the different attitudes toward science as an abstract institution (or the scientist as a job) and vis à vis concrete research or techniques.

A different form of perception is revealed when observing the reports on science, in particular reports on special fields of research or techniques, by the mass media. With the emergence of professional science journalism the presentation of science has developed to specialized editorial departments within the mass media.

The media do not primarily report about science for reasons of enlightenment as the popularizers had done. The public they address is also no longer comprised of the "educated of all classes seeking the truth" but an audience the media envisions solely from viewer and reader analyses. The media, i.e., concretely the editors and journalists, construct an audience for themselves according to the conceptions available to them. For this audience they only report about science if the contents under consideration have news value in the sense of the media's selection criteria. The only goal is to achieve maximum attention as this determines the income from advertisements on which the media depend as commercial enterprises. This, in turn, has an effect on the representation and ultimately on the perception of science in the public: Representations of science by the media stick to the predominant dramaturgical formats with regard to narrative, temporal and visual design (Donges and Imhof, 2001, 123).

This is exactly the basis of the tensions between science and the media and contributes to the recurrent conflicts between scientists and their mediators. The media representation with its tendency to dramatize and even sensationalize contradicts the scientists' self-perception of integrity which is the source of scientific credibility. In media reporting science, despite its ubiquity, appears to the public as a strange world about which it is easy to create clichés and myths.

The mass media perceive their environment highly selectively. This selectivity is not accidental but systematically structured by the so-called news values: actuality, controversies and conflicts, experience through local connection, every-day experience and others are such news values. Having this in mind, science is a very awkward topic for media coverage.

Despite differences in the media coverage of different cases, there are also important similarities. This concerns first and foremost the patterns of reception, i.e., the way how scientific themes become news and what kind of attention they receive from the media. Medical themes dominate and are followed by themes of natural sciences and technology (Stamm, 1995; Stuber, 2005). Gene technology and space technology are of interest while nanotechnology rather seems to be a passing trend theme (Piel, 2004). Relevance for every day life and a local/regional reference are also news values in the media coverage of science. This also holds for catastrophes, which receive the most attention, as was the case in the 1980s and 1990s with the accident at Tschernobyl and the Challenger explosion (vgl. Agazzi, 1995; Beste, 1989; Guha, 1989).

For a long time themes from the classic natural sciences such as chemistry and physics, which did not have a connection to every day life, were regarded as being of minor importance. This has changed in the past years. There is a recent boom regarding media coverage of science documented by the emergence of popular "knowledge" or "science" journals in the print media and related formats on television. This renewed interest of the media correlates with an intensified research on patterns of media coverage motivated by its significance for the legitimation of science. A study on science journals in 1997 and 1998 revealed that natural sciences (43%) were treated before medicine (25%), technology (13.2%), humanities (7.1%), and social sciences (3.8%) (Hömberg and Yankers, 2000; Scholz and Göpfert, 1998). Most of the journals reported themes which had entertainment value. Statements by the editors on the goals and principles for production of TV-magazines are especially revealing. The criteria for choosing themes: "...fascinating phenomena of nature" (volcanoes, tsunamis)...newest discoveries of science and technology (nanotechnology)...also every day themes such as water and coffee which we "often observe from surprising perspectives" (Grebe, Quarks & Co); "...themes have to be entertaining and fit into our program", i.e., even the false answers have to come across optically well, too (Klophaus, "Clever" SAT 1). Regarding the concepts of the shows it is stated: "To present science competently and entertaining without being too serious. Here, we always seek the most exciting way to present the topic" (Grebe, Quarks & Co); "'Clever' is 'science comedy' and purposefully not a common science show...an entertainment show...without the seriousness of schools...We want to make people have fun with knowledge without doing any overkill" (Klophaus, Clever, SAT 1).[1] The connecting of scientific content with entertainment, also called infotainment or edutainment, is a fairly recent development in science journalism and seems to be a result of the inevitable rejection of intellectual efforts by the broad audience. Many professional observers find this acceptable and only few point out that these shows do not contribute to increasing the capability of using critique, also with regard to science. The potentially progressive reports, which focus on the workings of science and its protagonists and could

[1] All quotations in *attempto* 19, 2005.

give insight into the production of knowledge, make use of the more accessible modus of biographies of scientists with a tendency towards hagiography.

As expected, the stereotypes presented in the surveys and popular entertainment media can also be found in the news media. Marcelle LaFollette has condensed the stereotypes in her extensive study on images the US public has of science: the *magician*, the rational and efficient *expert*, the *creator* and *destroyer* (from 1930 onward particularly associated with the physicist who is assumed to be responsible for positive and negative effects), as well as the *hero* who combines optimism in the future with an insatiable thirst for discovery (LaFollette, 1990, Chapter 6). Thus, the media reproduce the same stereotypes which can already be found in the literature of the eighteenth and nineteenth centuries and the creations of the pop culture of the twentieth century. They share the "image of difference" (LaFollette, 1990, 76). The "myth of being different" creates admiration, respect, trust, and fear at the same time and supports the social distance of science regarding societal responsibility.

One important function of the media is to produce discourses which deal with controversial research or techniques that can, during the discourse, be "embedded" into society (Weingart et al., 2007a). Topics differ in the degree to which they are controversial. These differences can be traced to the implications which they have for the dominant interests and values just like the different levels of attention for various disciplines or research areas. The grids of perception are only more differentiated when dealing with specific research topics. They can be classified as scientific, political, economic and ethical or legal interpretations (Schäfer, 2007, 79ff; Schäfer, 2008, 212). The discussion about stem cells has because of its ethical and legal implications received an unusually extensive and polarized media attention involving many actors. Human genome research, in contrast, was received in an almost equally extensive and pluralistic but largely uncontroversial discussion. Neutrino research, finally, has received only scant attention which was constrained to the science pages and completely uncontroversial (Schäfer, 2008). The examples for all three cases could be multiplied. Often it can be foretold already in the initial phase if a research area will trigger a public controversy or not. If such a controversy begins it can be predicted that the media will stage-manage it. Thus, science enjoys media attention not least because of its irritating effects (again one aspect of ambivalence!). Not only controversial discussions in the media public are being represented but also the controversies within science itself. The inner scientific discussions about open questions and uncertainties which are completely normal in the research process are interpreted by the media as conflict which reflects the inability and lack of sound judgement on the part of the scientists. They typically do not differentiate between scientific "mainstream" and marginal groups of "dissenters". In the case of anthropogenic climate change, for example, the media weigh all positions represented in science as equal. "The more intensive reporting about anthropogenic climate change, the more unequivocal the warnings of a catastrophe, the more interesting the "sceptical positions" represented by the media become. This pattern of media reporting is consistent with the theory about news value. It is irrelevant to the media if the weights between the scientists who believe climate change to be proven and the sceptics who doubt it are unequal. For the media dissent as such is worth reporting. Presenting the internal discussion is in accordance with the news value of polarization" (Weingart et al., 2007a, 18).

To the outside, in public perception, an image of helplessness and strife is portrayed while inside science the research process takes its evolutionary course. But a specific pattern of media reporting is associated with this form of perception. The media take on the role of a distanced observer who regards the discussion among the scientists from a supposedly neutral perspective. Communication scientists speak of "frames" in which the discussions are interpreted. In the concrete case the uncertainties of climate research are emphasized, in addition an ironical distance to the semantics of catastrophes (including that of the media themselves!) is taken, and the constellation of interests behind the climate change hysteria is revealed. The disturbing consequence of this perception of the scientific discussion is that science as an institution is attributed a self-interest in dramatizing research results. An exemplary commentary in a German newspaper read: "An alliance of 'concerned scientists', media representatives, special interest groups and politicians fuels fears about the implications of the greenhouse effect. They all seem to believe in the benefits of such fears. The concerned scientists finally come out of their boring laboratories and bathe in the sun of nationwide attention. The media love exciting horror stories because they fascinate the public and promise attention and success. Politicians use the attention thus created, find voters and solidify their positions" (Die Welt, 05-11-1993).

The Public in the Perception of Science

Meanwhile scientists, science administrators and science policymakers are no longer unaffected by the ways how the media and the public perceive them. They adapt to the permanent observation by the media and, following the logic of the increasingly important presentation on the "front stage", try to influence it to their own benefit, to anticipate controversies and resistances, and to pursue "public relations" in the traditional sense. With this we are on the side of science perceiving the public. Just like the media construct their publics and their image of science the protagonists of science construct an image of the public to which they want to present themselves.

In order to see how science perceives the public one only has to look at how scientists and politicians of science articulate their fear of losing approval of the public and how they try to regain lost support. Scientists' constructions of the public have changed significantly in the past three to four decades. In particular, natural scientists and engineers in the 1960s and 1970s still had a strong elitist image of their own role towards the public. In this image there was no space for the public as having a democratic legitimation to participate in decisions on the implementation of riskful technologies. Since the controversies about nuclear power, the scientists and politicians of science involved had to learn that such demands could be iterated with regard to all new branches of research and the introduction of new technologies as soon as they give reason for assuming risk. It is then unimportant whether these assumptions are justified from the perspective of the scientists. Today it is almost unthinkable that a scientist, even if he has that conviction, characterizes the people as "irrational." This form of distance, if not disdain of the public on the part of scientists can be traced fairly exactly to the time since the development of quantum mechanics until the early 1970s. It is explained by the ultimate abstraction of physics in conjunction with its commanding lead role in the development of nuclear key technologies during this period (Bensaude-Vincent, 2001).

Since then the political context has changed fundamentally, and at the same time the life sciences have assumed the lead function in technological development. The process of science discovering a democratic mass public has taken a long time and has still not been completed. When American politicians and their western allies were shocked by the launch of the Soviet satellite "Sputnik," they called for an educational program which was aimed at improving the scientific knowledge of their respective populations and thereby increase the probability of similar achievements by their own, still to be trained, scientists. The problem was, in fact, the relatively small number of students in the natural and technological sciences. The program had the immediate objective to increase the "scientific literacy" (the scientific education) in order to raise student enrolment in these fields and gain public approval for generous funding of space research. Only later did it occur to the instigators of the program that it made the "core curriculum" of the sciences the only referent with no regard to the everyday interests of the public. The program "Public Understanding of Science“, which was first started in England and the USA, shares the same philosophy: that the addressed public should "understand" the contents of science, which science views as relevant and communicable.

The propagandists of science have only realized the paternalist implications of these programs recently which led to a change in PR-strategies and to a new construction of the public. Thus, PUS in England and the USA became "public engagement in science and technology," and in Germany "Wissenschaft im Dialog" (Science in Dialogue). With this rhetorical shift the new character of the public was recognized, though without being mentioned in more detail. It is a democratic public which has its own interests and values and is not to be told by scientists which innovations it should approve of. This change of mind is, however, still half-hearted. Larger programs such as the German "Jahre der Wissenschaft" are, as respective evaluations have emphasized repeatedly, aimed at an unspecific public and have unspecific goals. Ostensibly the goal is to arouse the public's interest in science, lastly with the hope to gain general consent to growing science budgets. Alongside there is also the motive to close the widening gap of scientific-technical student enrolment (Weingart et al., 2007b).

One of the reasons for the wasteful character of these campaigns is that these programs are not conceptualized by scientists alone but by advertising agencies. This means that the public constructed by scientists is now replaced by a public of science constructed by PR-specialists. Consequently, the methods and instruments that characterize the programs are those of the advertising industry. It is the public of "events" and marketing, the success of the programs is measured in numbers of people who have been "reached," anywhere from 5,000 to half a million. They are indicators of success for the advertisement of goods of mass consumption. It remains unclear what the lasting effects will be with regard to changes of attitude and behaviour, as for example the choice of studies by youths. The longer term effects of the much smaller programs of cooperation between scientists and teachers, which are aimed at pupils, are rather sobering. The suggested involvement of the public by terms such as “engagement” and “dialogue” is thus first and foremost rhetorical. Visitors of “space centers” and “open days of research” or the audience of science shows are, of course, not really involved in a dialogue on funding particular research programs. Even participants of “round tables” or consensus conferences, who literally converse with scientists, do not have anything to do with the political decisions regarding the research. Their involvement is merely as a voter, and thus indirect.

Will Science Be Medialized?

The described development of the communication between science and the public shows that science, as an institution, has adapted to the public of mass (media) democracies. There are good reasons why science did and still does this, albeit reluctantly. The apparent elitism of individual researchers is only the appearance of a societal characteristic: science is a differentiated social system. This means that, first of all, scientists communicate with each other. They have to do so if they want to successfully produce new knowledge and be recognized for it by their colleagues. This internal communication is so efficient only because it is highly specialized and has created languages for each discipline and even individual fields of research. It is as such the mechanism of the distribution of the crucial reward in science, i.e., reputation. By attributing competent collegial recognition to certain discoveries and truth claims their authors are allowed to accumulate social capital and rise in the internal hierarchy thus created by this communication. The communication with the public, on the other hand, is not only obstructive in terms of time spent but also involves the “wrong” (lay) audience. Because of lack of specialized training it is incompetent to participate in the evaluation of disciplinary knowledge claims. It is therefore disreputable in science, even though it is not uncommon, to address the broad public, for example the media, when dealing with solving controversies within science. Questions of truth cannot be solved by taking a vote.[2]

The conditions of a science secluded from the public, however, do not hold anymore. On the one hand, science has become a topic for the media. Like other societal themes, science is under the media’s constant observation because new discoveries such as the human genome, dramatic scenarios such as anthropogenic climate change, or scandals such as various cases of fraud, all have a high news value. On the other hand, it has become a mantra of science policy to ask scientists to report about their work in the media, and thus make science in general as well

[2] On cases of scientists turning to the public cf. Bucchi (1996).

as their respective disciplines and institutions more attractive. In some institutions such an "outreach"-activity has already become a criterion for evaluations. In addition, universities meanwhile have created their own PR-departments. They produce increasingly expensive journals and send them to each other and the media as advertising material, demanding scientists to provide material. The outer appearances already share the jargon of the industry. The "science of the public" is therefore, in contrast to its predecessors, characterized by a smaller distance to its public. It constitutes its audience by following the laws of media communication while, to say it with exaggeration, contents merely play a secondary role.

This development can be called the medialization of science. Medialization is supposed to mean that a particular system (here: science) orients itself to the operational logic of the media. Here it is useful to differentiate between the representation and the production of knowledge (Rödder, 2008). A comparably innocuous consequence is that the representation of science is (to some growing share) carried out in the same media forms as all other media communications. Thus, it is subject to the same conditions, i.e., the competition for attention as well as its rapid decline, and it risks to be viewed as "interested" communications that cannot claim higher credibility. A less innocuous consequence of medialization would be that the presentation of science has effects on the production of scientific knowledge. This is the case when research priorities are determined – against the better judgement of the scientists – by the popularity value communicated into politics because politicians expect a higher approval from the electorate.[3] It is also the case when teaching positions at the university are awarded because of media fame gained through a television show instead of achievements recognized within the scientific community (Weingart and Pansegrau, 1999). In this still hypothetical case, scientific and media communication would compete with each other, and science's monopoly of truth would be crowded out by the media's monopoly of attention. This, then, would be tantamount to the replacement of scientific reputation, instrumental for the guidance of research, by prominence in the media. The forms of representing science have undoubtedly become medialized, but the effects on the production of knowledge are still unclear. First empirical studies show that the differentiation of science is not reversed, as suggested by the radical medialization thesis suggests. Rather, scientists' views of themselves are differentiated with regard to the representation in the public. Aside from the classic type of scientist who is only focused on his work and avoids any kind of contact with the public, one can find scientists who instrumentalize the public in different ways. They may do this for the interest of science in general, for their own convictions, or for carrying out certain directions of research (Rödder, 2008). Furthermore, medialization is restricted to certain fields of research and to discoveries of research that are of particular interest to the media (Schäfer, 2008). The media communicate the interests of politics and the economy and constitute the framework of conditions under which science has to operate. This framework has

[3] This is, of course, not to say that the public does not have a legitimate claim to determine priorities of publicly funded research, although its involvement will depend to some extent on expert advice.

become narrower and, thus, the necessary measures of adaptation have become more complex. What this ultimately means for the achievements of science, its reliability and our trust in science is not yet foreseeable.

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Part V
Science, Values and Society: Freedom of Research and Social Accountability

Conditions of Science: The Three-Way Tension of Freedom, Accountability and Utility

Torsten Wilholt and Hans Glimell

Getting the Best Out of Scientific Research: An Argumentative Map

In the long-standing dispute about the conditions of science in society, many grand outlines have been suggested. Their proponents have often attempted to promote one or more general conclusions about science (e.g., that research must be autonomous, or that science's social accountability must be reinforced) and, understandably, have striven to identify a coherent argumentative strategy that best supports their claims. Arguments that point into other directions are often seen as obstacles that must be rebutted or dismissed. In this paper, we will try to describe the main layout of these arguments from a more ecumenical perspective. In this first section, we will start by portraying six core arguments that have each played a role in science policy after World War II as well as more generally in the debate over the contemporary conditions of science. Their common characteristic is that each of them puts on view some connection between science and one or more of the things we value. The relevant items of concern include wealth and other evident aspects of our quality of life, knowledge (obviously), but also safety, control, social justice, and even democracy itself.

Naturally, the arguments described will be ideal types, distilled from lines of reasoning that have been occurring in actual discourse in many different varieties and guises. Despite the inevitable loss of detail and historical precision this entails, we think that this approach can provide helpful orientation within the variform debates about the conditions of science. The six arguments we identify can be classified into three categories, following the rough directions to which they point:

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arguments for freedom of research, arguments for accountability, and arguments for targeted research. These three argumentative directions can thus be seen as creating a three-way tension that defines the main lines of controversy about science.

In the subsequent section we will expand our emerging framework by identifying a number of strategies and priorities that have actually been adopted in science policy after World War II. The three-way tension is recognizably present in the succession of policy constructs that have been implemented in western countries. In parallel to our exposition of the policy constructs, we will therefore revisit their locations on our argumentative map and ask ourselves whether new approaches in science policy can make the three-way tension manageable. How should they deal with the conflicting arguments? Being abstracted from real-world claims, each of the arguments we describe has its strengths and limitations – none of them is applicable to each and every instance of scientific research. Our tentative take on their clashing conclusions is that one should not seek a universal, "correct" resolution of the conflict of the arguments (e.g., a proof to the effect that one of the arguments defeats all the others). Rather, the consideration of each problem in science policy requires a specific weighing up of the arguments as they apply to the particular case. In the third and concluding section of the paper, we will follow up on this idea and in particular reflect on the ongoing efforts to introduce deliberative procedures into science policy. We will discuss their potential of providing a framework in which the why-issues concerning science, i.e., the differing ways in which scientific research is related to things we value, can be brought to bear on the science policy discourse.

Arguments for Freedom of Research

The Argument from Social Epistemology. It is a venerable epistemological position that free inquiry is simply the most effective way of organizing a collective epistemic effort. The rationale behind this claim is that free inquiry will lead to a diversity of different approaches. This raises the chances of collective success in the search for knowledge, both because each individual approach is fallible, and because ideas can be improved by mutual criticism. (Mill, 1859, Chapter 2, is usually credited with this argument, though he had important forerunners, e.g., Milton, 1644.)

Modern defenses of freedom of research sometimes run along the lines of an updated version of this argument for free inquiry: All prior judgments about the fruitfulness of research projects are fallible. It can not be precluded that projects which are at present not recommendable according to widespread standards will turn out to be groundbreaking. Therefore, scientists should choose their approaches and projects freely, such that – it is claimed – a wide variety of approaches ends up being pursued. Some of them will prevail and lead to new knowledge, but it is impossible at any time to predict which ones these will be.

Because the principle of freedom of research does not seem to carry any restrictions or implications with regard to the kind of knowledge that will be produced, and in particular with regard to whether that knowledge will be in the public interest, some thinkers have been prepared to restrict or sacrifice the principle (Bernal, 1939, 277–278; Feyerabend, 1980, 167–168; Kitcher, 2004, 56) and instead to pursue specific targeting of research with a view to particularly desired pieces of knowledge (see the "arguments for targeted research" below). This has usually been countered by the argument that scientific innovation is essentially unforeseeable and that it is therefore impossible to plan the production of substantial scientific knowledge for particular purposes (Polanyi, 1942; 1962, cf. Bush, 1945, Chapter 3).

Evidently, the argument from social epistemology provides at best an instrumental rationale for the freedom of research: *If* we want a maximally efficient production of knowledge, *then* we should ideally guarantee a research environment in which (groups of) scientists can freely pursue their chosen projects. Note also that the implicit presuppositions of this argument are considerable (cf. Wilholt, 2006, 2008). Most notably, it rests on the assumption that freedom of research does serve to generate a productive diversity of approaches within the research community. This is only plausible if the argument is understood as an argument for individual freedom within the sciences. It can hardly be adapted to support the collective right of a scientific discipline to plod on without "external" interference that is also sometimes defended with the rhetoric of scientific freedom.

The Argument from Democracy. It is a classic idea of democratic theory that democracy requires knowledge on the part of the citizen in order to assess the consequences of legislative and executive decisions and to develop informed preferences (cf. Page and Shapiro, 1992, Chapter 10). The corollary that science as a major producer of knowledge must enjoy a certain degree of independence from government was already formulated by enlightenment thinkers (cf. Condorcet, 1792). A modernized version of the argument could proceed along the following lines.

While the legitimacy of a democratic government of course includes legitimate control over public funds, this legitimacy is itself dependent on a functioning democratic process. Free speech and free inquiry belong to the preconditions of the democratic process (cf. Dahl, 1985, 21–22; Brown and Guston, 2009). The knowledge required for an informed view on the affairs of modern societies by far surpasses whatever knowledge might be produced by private research efforts. For example, private inquiries of citizens into the causes of climate change would have little hope of attaining any helpful insight into the matter. A public science whose task it is to provide information to the citizens in as impartial and reliable a manner as possible, is thus indispensable. While the government is therefore obliged to provide funding for public science, any attempt to execute further control over science and determine the kind of knowledge it produces runs the risk of undermining the democratic process and thereby the government's own legitimacy.

The analogy to publicly-financed media is befitting. Attempts of governments to control the contents of, for example, public television programs, are rightly condemned. This is not because the legitimate power of democratically-legitimized governments over what is being done with public funds is somehow limited, but because control over the contents provided by such a core instrument of public knowledge transmission and opinion generation would undermine the democratic process itself by which the government's legitimacy is generated.

The argument from democracy promises to provide strong grounds for freedom of research, insofar as protecting the preconditions of the democratic process deserves high priority. It might be limited in its scope, though. While the argument can reasonably support the case for publicly funded free research in, for example, climatology, one might argue that government meddling with the research agenda of category theory or palaeoentomology would leave the democratic process unaffected. The argument from democracy might therefore be taken to contribute to the justification of a principle of freedom of research only insofar as research with discernable political implications is concerned.

As with the argument from social epistemology, fine distinctions also matter in the case of the argument from democracy. Note especially that the argument cannot be employed to fence off every effort to introduce democratic elements or public participation into the processes that determine the scientific agenda. It can only be used to show that these efforts should not take the form of immediate government involvement. What the argument from democracy calls for is a *separation of powers* between science and the other political powers. Again, this idea is not to be confused with a de-politicization of science. Instead, one can think of the argument as demanding a buffer between science and the legislative and executive powers of the state.

The connection between scientific autonomy and concerns for democracy has only recently gained a renewed topicality in the context of the lively US-American discussion about the politicization of science under the Bush administration (cf. UCS, 2004).

Arguments for Accountability

The Argument from Inseparability. The knowledge produced by the sciences has an *impact* on the conditions under which we all will have to live in the future; therefore, the research decisions that have this tremendous impact cannot be made under the protective bell jar of a scientific autonomy that goes so far as to exclude accountability. It has long been argued that the impact of technoscientific change has reached a new and unforeseen dimension in the twentieth century (in terms of numbers of people affected, irreversibility of consequences, etc.) (Jonas, 1979). Under the rubric of impact, one should also consider the consequences that knowledge itself has (independently of its technological implementation), e.g., on our self-perception. Impacts of this kind have been described as a co-production of scientific knowledge and social order (Jasanoff, 2004).

Scientists themselves would perhaps not be held accountable for the impact of their research results if their realm of knowledge production was so neatly *separated* from the realm of application and impact as some of them have tried to argue at some time or other. The widespread and multi-pronged critique of this separability (that is, in effect, the separability of science and technology) therefore adds up to an argument for accountability. The case for inseparability includes the following important lines of reasoning:

– Advanced experimental science has itself become so technological that scientific research and the development and improvement of technologies are now inextricably interwoven. Nano-science is a powerful contemporary illustration of this practical intertwinement of science and technology that has been increasing for many decades.

– Powerful innovation, once made possible, can't be stopped at the lab door. The atomic bomb remains the emblematic example of this insight. A technological potential emerging inside the lab will at least sometimes have such a strong influence on the political and societal developments outside that this effect alone negates the separation of the idea from its application.
– All kinds of choices deeply embedded in the research process affect the ultimate social outcomes – "research decisions" and "decisions of application" can't be disentangled from one another (Douglas, 2003). Decisions about experimental design, about data interpretation, and even about publication and dissemination of results are inevitably made with a view to the consequences of potential errors. A separation of such consequences into "scientific" and "extra-scientific" is by and large not feasible.
– The co-production of science and social order begins with the earliest stages of research, not only with its application. As a present-day illustration of this, consider the reconfiguration of our idea of (intellectual) property with respect to biological systems occasioned by genome research (see e.g., Hilgartner, 2004). The distinction between knowledge production and application can therefore not mark a significant boundary of accountability.

The Argument from Interests within Science. Scientists have specific interests, both of an individual nature and such as are determined by the dynamics of the scientific discipline they belong to. They want to advance their careers, work at the perceived frontiers of research rather than in the backwaters, cultivate progress in their own discipline etc. While it was for a long time assumed that these could be ignored, or left to the self-regulatory mechanisms of science, or assumed to be in the long run convergent with the public interest in scientific knowledge production, this kind of trust has been slowly eroding. (Academically, science studies have devoted much effort to identifying interests within science in such works as Latour and Woolgar (1979), Pickering (1984), Shapin and Schaffer (1985) and several others. For a description of the political erosion of trust, see Sarewitz (1996, Chapter 4), and Guston (2000).) In debates over the environmental sciences, over the funding of large-scale research investments, over gender bias and also over cases of fraud in scientific research, the received picture of the disinterested scientist has been called into question. Instead, science policy scholars have started to regard scientific research as a *task* that is *delegated* by the public or the government to the scientific community, and thus to view the relationship between government (respectively the public) and science as one between contractors, where the principal (i.e., the public) must somehow make sure that the agent (science) pursues the delegated task rather than his own interests (cf. Guston, 2000, and the many works referenced there in fn. 1 on p. 166).

The demand for accountability on the part of the sciences is on the one hand strengthened by the fact that neither the public nor administrators or politicians usually have the required knowledge to estimate the significance of a scientific program or research project, or the credibility of a scientific claim. Science policy is thus shaped by an asymmetry of information (Guston, 2000), which is hard to overcome due to large transaction costs, and scientists may therefore be regarded as having an obligation to fulfill in bridging this knowledge-gap. On the other hand, this asymmetry may at the same time limit the practical feasibility of demands for accountability.

Arguments for Targeted Research

The Argument from Non-Linearity. It has gradually been recognized that the many social benefits that science is expected to contribute to – e.g., better health care, better protection of the environment, increased quality of life in general, as well as new industries and new and better jobs – will not spill automatically from the kind of scientific research conducted by scientists who simply follow the inner dynamics of their discipline (see e.g., Rosenberg, 1994, Chapter 8; Wise, 1985; Sarewitz, 1996, Chapter 2; Guston, 2000, Chapter 3). Instead, research programs have to be strategically designed in order to cater to these public needs. This argument converges with the critique of the so-called "linear model" or "cascade model" of techno-scientific innovation. Deriving practical benefits from the results of basic research is not a matter of straightforward "application", as the linear model (Bush, 1945) had suggested. Rather, it requires considerable research efforts itself and maybe even a genuine kind of strategic research, in order to achieve the "finalization" of scientific knowledge (cf. Schäfer, 1983). This kind of research will not normally coincide with the research questions dictated by the disciplinary dynamics of the sciences and has therefore at times been described as "transdisciplinary". Even if the epistemological differences between "transdisciplinary", "mode II" (Gibbons et al., 1994) or "post-normal" science (Funtowicz and Ravetz, 1993) on one side, and academic, disciplinary research on the other, may have been overemphasized to a certain extent (cf. Adam et al., 2006), the linear model is lastingly tarnished, both by the philosophical critique and by the economic experiences of the 1980s.

Incidentally, an early and radical version of the argument from non-linearity can be seen in the movement for planned science instigated in the 1930s by J. D. Bernal. His own argument for a science directed to social benefit is embedded in an alternative reading of the history of science that bears resemblance to the later criticism of the linear model. In Bernal's view, it is not so much the abstract pursuit of true theories that is the driving force behind scientific progress and yields practical utility as a by-product. To the contrary, science owes its tremendous success to the fact that it is ultimately a practical enterprise to begin with; it is built on craft knowledge and is driven forward by the inventions of artisans and engineers throughout its history (Bernal, 1939, Chapter 2). The view of applications as a spin-off or spillover from "pure science" is rejected in favor of an image of science immediately geared towards concrete, practical aims. In that sense, Bernalism is a special (and very radical) case of the argument under discussion.

Today, the well-supported doubts about the linear model will guarantee continuing political demand for strategic research (stripped, of course, from Bernal's socialist underpinnings). However, as an argument for specific targeting, this line of reasoning can only be as strong as the practical means of constraining the directions of research in accordance with strategic considerations are effective. In the past, such attempts have had mixed success. If the envisioned targets do not happen to coincide with research frontiers arising from the inner dynamics of the discipline, scientists are likely to exhibit resistance to targeting (cf. van den Daele et al., 1979). Known forms of resistance are difficulties in recruiting qualified researchers, lack of response to targeted funding programs, and maybe most notoriously the covert substitution of research aims (re-labeling). In cases where such problems cannot be overcome, they may limit the ultimate force of the argument.

The Argument from National Economy. As regards one specific kind of benefit that is expected from techno-scientific progress, economic growth, it is often argued that public resources have to be strategically invested into research that holds promise of boosting the national (private) economy in the long term. The classic rationale behind this is ultimately the idea that private enterprise economy fails to provide adequate incentives for knowledge production (Arrow, 1962; Nelson, 1959), for a variety of reasons: The costs, the time spans, and the uncertainty involved in knowledge production are all too big to be born by private enterprise. And even once relevant knowledge is achieved, it is difficult to appropriate. Even if some private firms *do* in fact perform scientific research and even if reasons can be identified why that might make sense for them economically (Rosenberg, 1990), these examples have always been exceptional in that they all occurred in a small number of industrial sectors and a handful of large firms. What's more, they seem to have become rarer and rarer over the last 15 years or so.

Therefore, public science has to step in and engage in research that is geared towards providing the specific kinds of knowledge needed to fuel the innovation process in private industries. Politically, this aim has been pursued in the form of the many instruments of technology transfer policy, which can on a very general level be described as an intentional mixing of public and private. (Cf. Guston, 2000, Chapter 5, esp. 137. Though Guston's observations are limited to the USA, similar trends abound in Europe; witness the German "Verwertungsoffensive", BMBF, 2004, 366.)

The persuasive force of the argument from national economy may be limited by considerations of distributive justice. As a public investment, scientific research should bring about a public good that benefits the whole of society. But arguably, at present only few actors are able to derive benefits from the results of research. The very idea that science produces a public good has been called into question (Callon, 1994), and the mixing of public and private by means of technology transfer policy has been criticized (e.g., Nelkin, 1984; Slaughter and Leslie, 1997; Slaughter and Rhoades, 2004).

Dealing with the Three-Way Tension: Strategies in Science Policy

As we announced at the outset, each of the six arguments establishes some connection between science and something we value. We now see that none of these connections hold unconditionally and indisputably for each and every instance of scientific research. Some of these connections depend on preconditions that apply only in some but not all cases, while others vary in strength with different fields of application. Nevertheless, the three different directions into which the arguments pull constitute a persistent three-way tension that can be seen as characteristic of the science policy arena. In the political sphere, the "things we value" figure as things valued by the public, and the conflicting conclusions of the arguments constitute a practical problem that calls for strategies of mediation.

In what follows, we will survey some of the strategies for dealing with the three-way tension that have dominated western science policy since after World War II. Obviously, the policy constructs we will describe are abstractions from much richer historical developments. Real science policy has almost never followed one clear-cut paradigm but is typically characterized by a patchwork of approaches. Nevertheless, some typical strategies for handling the three-way tension stand out because they have dominated policymakers' approach to scientific research at certain times. We will try to describe these in the following sections. We are not interested in the logical space of all potential strategies but rather only in the ones that have actually had a significant influence on policy.

First Strategy: The Policy of Non-Policy ("Blind Delegation")

The first strategy we have to consider is simple: Declare freedom of research as an indispensable core element of every adequate science policy, and accordingly embrace the respective arguments summarized in the beginning of the first section, while simultaneously prioritizing their import. A nearly perfect illustration of this can be found in Michael Polanyi's essay "The Republic of Science" (1962). The knowledge-seeking enterprise of science requires freedom, and this requirement is assumed to beat all arguments in favor of accountability or targeting: "Any attempt at guiding research towards a purpose other than its own is an attempt to deflect it from the advancement of science. . . . You can kill or mutilate the advance of science, but you cannot shape it" (Polanyi, 1962, 62).

This absolute commitment to non-interference and to science unbound or unfettered rests upon the scientific community's own version of Adam Smith's "invisible hand", which, according to Polanyi, instantiates the more general principle of "coordination by mutual adjustment of independent initiatives". Within science, this principle is assumed to operate by tacitly and gently guiding a set of independent initiatives to a maximum advancement of science. Where in a liberal market the price mechanism is trusted to infallibly mediate mutual adjustment, the equivalent regulative force within science is rendered possible by scientists actively taking note and appropriately responding to the published results of other scientists (Polanyi, 1962, 56).

The invisible hand is also meant to sustain coordination and quality of scientific research automatically, constituting a case of “sociological exceptionalism” (Bimber and Guston, 1995, 558). Specifically, it is supposed to produce the two outputs that society can demand of any “Republic of Science”: the integrity and the productivity of science. Any interference is seen as destined to damage or disrupt this mechanism.

In a certain sense, the state patronage of science established on a wide front after World War II, especially as portrayed in the White House’ famous commissioned report *Science – The Endless Frontier*, constitutes a challenge to this principle. In return for federal funding, society expected something back from the scientific community; “the free play of intellectuals” had to be made politically accountable (Bush, 1945, 12). The vision articulated in the report encompassed just about every imaginable socio-economic benefit and public utility: advanced military technology, new sources of energy, more abundant crops, new medical technologies, more jobs, shorter working hours (Guston and Keniston, 1994, 1–2; Van der Meulen, 1998, 397; Godin, 2006, 644).

A new order, often later referred to as a principal-agent contractual relation, was proposed and gradually began to be implemented. Its script introduced strategic interests and social accountability, challenging the view that the values of freedom and self-regulation automatically trump all other concerns. The evolving spectrum of “four estates” from truth to power (Price, 1965) – the scientists, the professions applying the findings of science, the administrators and the politicians – implied an alternative mode of governance: the twofold principle of freedom and responsibility: “the closer the scientific estate is to the end of the spectrum that is concerned solely with truth, the more it is entitled freedom and self-government; [. . .] the closer it gets to the exercise of power, the less it is permitted to organize itself as a corporate entity, and the more it is required to submit to the test of political responsibility” (ibid, 137).

In practice however, the governmental agencies and the new research councils administrating the new “social contract for science” by and large kept giving support to free research instead of implementing constraints. The terms-of-trade were hence for a long time “remarkably advantageous”, offering ample support with no strings attached (Wittrock, 1985, 27).

The actual relations between the “republic of science” and political purposes and values loomed in the distance as the twofold principle of freedom and responsibility was overwhelmed by a self-regulating mutual adjustment principle; prescribing and installing “blind delegation” (Braun, 2003) or *the policy of non-policy* as the first universal science policy regime.

Second Strategy: Interlaced Self-Regulation

A strategy giving absolute priority to arguments for *targeted research* has never been imposed in Western democracies. However, a gradual decline of confidence in the linear model and the self-regulation mechanisms of the post-war models has given rise to policy constructs that grant considerable weight to these arguments – enough to allow constraints on scientific freedom. Nevertheless, the policies in question preserve large elements of the self-government of science, e.g., by means of combining targeted government programs with less constrained institutional funding and project funding through science-oriented agencies. (However, incentives were and are often set in such a way as to make clear that researchers and institutions that do not use their "freedom" to become ever more market- and application-oriented – in short, "targeted" – will in the long term spell their own doom.)

Policies pushing for piecemeal targeted research at certain points in the process, while maintaining freedom of research and self-regulation as the fall-back position, have come in different colours. We summarize them under the heading "interlaced self-regulation". An early example of this strategy was the two-part strategy of soft science policing that was tentatively implemented in the US after doubts about the social contract for science had gained a foothold during the 1970s, as compellingly described by David Guston. One strove to uphold the leading principle of the contract, while at the same time interweaving into it elements foreign to the species. Around 1980 this had begun to institute a new modus operandi: *collaborative assurance* (Guston, 2000, 144–146).

Principal-agent contractual relations (so far only sparsely institutionalized) were now furnished with certain so-called boundary organizations, i.e., "institutions that straddle the apparent politics/science boundary and, in doing so, internalize the provisional and ambiguous character of that boundary" (ibid., 30; see also Guston 2001, 399–402). These allowed the political and scientific communities to stabilize contested border areas and render them manageable. By translating and negotiating between different styles of reasoning, they seek to create a sustainable level of mutual trust, pursuing the motto "Trust – but verify!" (different from Polanyi's "Trust!"; or even "Trust, and glorify!"). The Office of Research Integrity (dealing with contested allegations of research misconduct) and the Office of Technology Transfer (dealing with demands for tangible market linkages), both at the NIH, exemplify boundary organizations which in this way tampered with the principle of self-regulation by setting up collaborative assurance arrangements to secure the integrity and the productivity of science respectively.

While this institutional innovation was essentially a US response to a diminishing efficiency of the prevailing policies, parallel European developments differ in their mode and level of regulation. In response to framings and recommendations put forward in two high-level OECD reports (OECD, 1963; OECD, 1971; see Elzinga and Jamison, 1995, 584–588), new policies promoting the inclusion of pro-active selectivity and targeting procedures were outlined, sometimes referred to as an "Enlarged Science Policy" (Geiger, 1985, 65). As a supplement to established science-centered "policy-for-science" practices, they launched a new "science-for-policy" discourse anchored in state-centered top-down planning to improve economic competitiveness as well as enhance the social relevance of science. Numerous mission-oriented intermediary (or sector research) agencies were founded to articulate distinctive public research demands, and channel the transfer of basic scientific knowledge for applications in these specific political fields (Braun, 1993, 142).

Many policy analysts concluded that the eroded trust in the self-regulation of science, the dissemination of values of commercial culture into the inner realms of science, and the increasing number of mission-oriented agencies devoted to forming a consensus between representatives from the academic, industrial and governmental cultures (Elzinga and Jamison, 1995, 591–592) in selected areas of science and technology, amounted to an epochal shift in science policy. As the workings of science became systematically associated with particular contexts of application, the state patronage of science evolved into a more a pro-active role. The social contract for science was thereby superseded in the early 1980s (Guston, 2000, 144). Institutions closely affiliated with this policy soon had to follow suit. For example, the research councils, which had become "parliaments of science" in line with the idea of blind delegation, now had to recast themselves, the new motto being "become entrepreneurial – or become obsolete" (Rip, 1994, 3).

Certainly the developments succeeding the innocence of the first decades of the contract seem to disturb Polanyi's market-style mechanism of "price signals" mediated by publications. Now also policy-makers issue signals to attract and guide the scientists by offering extra funding for prioritized fields. In practice, however, this interventionist mode of governance, referred to as a *delegation by incentives* (Braun, 2003, 312–313), seems to have encountered crucial problems from the very outset. Being merely an addition to a social and career system still clearly favoring undirected research, it presented a costly option for many scientists, and therefore was accompanied by a temptation to minimize their efforts – "the incentive mode of funding raises decision-making costs, monitoring costs and increases the danger of moral hazard" (ibid, 313).

Today, scholars of science policy look into arrangements designed to overcome the shortcomings of delegation by incentives. According to Braun, three of these are austerity (budget reduction), contracts, and networks. Whether unintentionally implemented due to budgetary deficits, or deliberately applied as a delegation strategy, austerity pushes scientists to look for financial compensation elsewhere. It has been suggested that this has indeed had a tangible impact on a shift from mode-1 to mode-2 science. But austerity suffers from exactly the same dysfunctions as the incentives policy. The modes of delegation by contract or by networks are claimed to be different. They (the latter in particular) are expected to be more capable of transforming the deeper "institutional embeddedness" of science, of keeping in check the moral hazard of scientists "shirking" responsibilities (e.g., by means of covert replacement of research targets) and of increasing their social responsiveness (*ibid.*, 314–318).

Our own stance will be to adopt a moderate assessment of the overall significance of newer tools and institutions of science regulation. For the time being, this appears to be better substantiated than endorsing more dramatic epochal shift claims. While acknowledging that policy-makers today have a wide range of options with which they can strongly affect the conditions of science, we also bear in mind that scientists demonstrate great skills in domesticating agendas imposed on them, adjusting funding terms and instructions to concur with their internal priorities. A case in point concerns the wave of neo-liberal Schumpeterian research policies perceived as almost unstoppable some 15 years ago. In Sweden, once seen as a model for pro-active patronage policies (a precursor of Mode 2), these policies were rebutted in less than 5 years and radically reconfigured into a neo-*classical* research policy, practically amounting to a reinstatement of the Republic of Science (Elam and Glimell, 2004). Self-regulation may have to make room for other institutional logics here and there and yet remain the dominant principle.

Third Strategy: Science Legislation ("Blunt Regulation")

The conflict between the policy of non-policy and the policies of interlaced self-regulation are troubled by the two-way tension between arguments for freedom of research and arguments for targeted research. But it is not as if *accountability* comes into play only by being built into some of the boundary organizations of interlaced self-regulation. One must not forget about an additional element that has served to arbitrate this three-way tension and that has coexisted with all other solutions we have discussed: legislation.

We certainly will not try to list all kinds of legislation that concern science. Instead we will proceed by suggesting a couple of general characteristics of this mode of policy and illustrating them with examples. First: science legislation typically means *bringing the implicit values that tie a community together to the fore, often by making them subject to controversy.* Legislation is by definition an imperative, non-negotiable mode of directing people and their collective efforts. Instead of weighing or balancing the pros and the cons it dictates a fixed yes or no. As shown in our earlier review of arguments over the conditions of science, this is quite the opposite of what many consider the very hallmark of science as a human endeavour. Not only should curiosity and free inquiry be safeguarded as indispensable for it, it has also been held in high esteem as a vital check-and-balance for an unrestrained exercise of political power. Therefore, once legislation vis-à-vis science is brought forward, typically also other values of similar significance are involved. It is then bound to evoke contestation and controversy, and lay bare the divides between deep-rooted cultural, ethnic and religious values that are normally concealed; it can help us identifying these.

This is more or less the opposite of how the other policy constructs work. While these tend to conceal value disagreements, the strategy of blunt regulation tends to expose them for all to see. Consider the case of stem cells. When in 2001 President George W. Bush announced in a televised address to the American nation that funding for embryonic stem-cell research was to be stopped except from those cell lines already in existence, this spurred a lively world-wide debate over legislation. Almost overnight, numerous individuals, non-governmental organizations and private associations which one would think were quite remote from debates over science policies, had one of their own. Moral and existential issues lay open, but when then 1 or 2 years later research councils in many countries responded by launching their home-brewed ethical guidelines for stem-cell research, this soon seemed to have put much of these public deliberations off to the side.

Secondly, science legislation, while often concerned with more or less long-established values, has a *short expiration date since it legislates a moving target*.

Consider here the case of information technology in the 1970s. For a few years during the explosive development of this technology, many people sensed a close connection between the then dominant outward show of computers – namely bulky machines, wrapped in several protective covers, isolated in big separate buildings with restricted access – and the literature genre of dystopian Big Brother imageries, quite in vogue at the time. In several countries, the concerns this raised led to specific computer legislation, primarily affecting the use of information and communication technology (ICT), but to some extent also regulating science in the area.

Only 5 or 10 years later, however, ugly computers had been transformed into handy little gadgets, being almost like toys which children (and fathers of course) could play with. The scary connotations had vanished, and ICT legislation largely lay fallow, only shortly after its inception. (It might perhaps return in the future, as a backlash to the immoderation of the current post-9/11 security discourse which today spurs a new wave of ICT research and applications.)

Thirdly, we suggest that *some workings of current science policies call forth a science legislation that regulates practically non-existing phenomena*. Some areas of contemporary technoscience carry a potential that has prompted the demand to address their wider ethical and societal implications "upstream". At the same time, some of the actors involved repeatedly heated the relevant discussions by (co)producing long lists of just incredible achievements and benefits begging to become fulfilled. This was perhaps a response provoked in part by dystopian nano science fiction. But nanotechnology *is* breathtaking due to both its opportunities and its risks – a predicament of the late modern "risk society" that few would dispute. The predicament is further amplified by a kind of discursive production, as was the case when K. Eric Drexler, determined to learn from the public mistrust around genetically modified organisms, pointed out already in the 1980s that for every major opportunity one attaches to nanotechnology there is always an equally high risk one has to deal with. To be sure, the nano case represents an extreme, and the only legislation activities linked to nanotechnology so far are some minor amendments to existing laws concerning the possible toxicity of certain nanoparticles. But if the strong claims around nanotechnology continue and if they continue to evoke major worries, then we might yet hear calls for a legislation regulating scientific things which are as yet little more than fiction.

Deliberating on the Tensions

For a decade or two, the conditions of science have primarily been put under pressure by two economically-informed policy currents. The first one, entrepreneurial (or Schumpeterian) in character, has been concerned with the "what" and the "when" dimension of research funding (picking winners with the right timing), driven by a strong belief in the possibility to identify and derive macro-economic benefits from recurrent technological shifts. A second current, often in connection with the first, has focused on a "how" issue, namely how non-scientists (i.e., civil servants) can get scientists to embrace aims and goals of the public. By transplanting notions from the new institutionalism of economics – principal-agent relationships, transaction costs, information asymmetry, moral hazard and "shirking" (Arrow, 1985; Moe, 1984; Powell and DiMaggio, 1991) – it searches for the right micro mechanisms for the practical implementation of science policy.

Whereas these developments have brought into focus the productive and managerial aspects (science policy as innovation governance and management by delegation) respectively, our own primary concern instead highlights the why-issue. By this we mean the diverse ways in which scientific research is connected with things we value and which we have tried to trace out by our six prototypical arguments. "Why-issue" is a shorthand for all the principal *reasons* that are (or should be) relevant in an argued justification of how the priorities are set amongst scientific freedom, accountability and strategic targeting in each respective case. We believe that the arguments we have identified are the predominant ones that would need to be addressed in order to assess these reasons and thereby to tackle the why-issues.

We wonder whether such arguments and the values to which they speak have so far received a fair share of attention in the workings of science policy, dominated as it is has been by the questions "what", "how much" and "how", and by strategies that tend to conceal rather then reveal the question "why". Within the regime of "blind delegation", why-issues were evaded by regarding the arguments for freedom of research as trumping all other concerns. This was typically complemented by a particular way of applying the linear model of innovation which helped to avoid all antagonistic tensions by neatly telling them apart temporally. According to this interpretation of the model, arguments for freedom of research are predominantly relevant for the early ("upstream") phases of knowledge production, whereas other concerns – accountability and utility – become relevant only in later ("downstream") phases along an envisioned linear axis. In contrast, the "blunt regulation" indeed does highlight competing or incongruous connections between science and things we value, but usually only for a short while, after which controversy and negotiation officially ends in a legislative decision.

Unlike both of these strategies, interlaced self-regulation per definition acknowledges the simultaneous operation of several arguments and the tensions generated thereby. After all, it evolved as a response to the shortcomings of the first regime, and consequently came out as distinctly different from it (Guston, 2000, 140–145). But built into the policy practices of interlaced self-regulation (as well as into the accompanying theoretical principal-agent framework) is the assumption that the tensions can be regarded as an antagonism of two parties. Our analysis shows, however, that the why-issues that define the field of science policy pull into more than two different directions. Accordingly, regulative approaches within the paradigm of interlaced self-regulation have largely concentrated on appropriate administrative procedures to manage the relation between science and the government. The resulting technical measures are often more likely to mask than to exhibit or elucidate the disputed connections to values and benefits underlying them. They have therefore not brought about a course of policy that openly integrates the "why"-questions.

Our ideal of such a course is based on the view that no single argument can be asserted to hold sway for each and every instance of scientific research, and none of them can claim universal priority. This situation sets the demand for ways of arbitrating the three-way tension which can deal with the conflicting arguments on a case-by-case basis. In the remainder of this paper, we will reflect on the prospects of solving this problem by means of *deliberation*.

There is currently, in particular in Europe, a lively debate about deliberative governance modes. Two British commentators have claimed that the many initiatives one way or another advocating for or testing in practice to introduce deliberative procedures into science policy amount to a new "Politics of Talk" (Irwin, 2006), indeed also propelling "the New Scientific Governance" or "Participatory turn" in science policy (*ibid*.; Rayner, 2007).

Evidence suggests that this is not merely a passing policy fancy, but a more long-standing development around the phenomenon of "risk". Remember that 10 or even 20 years ago, several scholars claimed that an obsession with risk was becoming a defining characteristic of late modernity. It is sometimes argued that this has escalated since then and that a "rise of risk" can be observed in the contemporary public debate, culminating in "the Risk Analysis of Everything" (Rayner, 2007; Power, 2007). Deliberative procedures introduced in order to grant non-experts a role in high level decision making on science and technology, as they have recently been promoted by such prominent policy institutions as the European Commission and the National Science Foundation, are said to be part of this trend.

> Social scientists have responded to the rise of risk, triumph of technique and attendant electoral decline by advocating and designing increasingly sophisticated techniques of their own to re-establish a role for non-experts in scientific, environmental, and technological decision making (e.g., Irwin, 1995; Renn et al., 1995). These include focus groups, citizen juries, community advisory boards, consenus conferences, and participatory integrated assessment. (Rayner, 2007, 169)

This deliberative or public participation paradigm is also intimately linked to a new conception of the citizen, one who fulfills the virtues of being socially embedded in a community, locally knowledgeable, reflexive about society and nature, committed to a common good, and to inclusionary deliberation as the way of revealing good political solutions. These ideas and the deliberative techniques accompanying them make frequent appearances in the ongoing process of putting flesh on the rhetorical bones of an emerging European knowledge society, notably including the far-reaching idea that they could and should not only "widen the circles of innovation", as was the motto of one commissioned report (Nordmann, 2004), but indeed also put an end to the die-hard linear model (or "regime of economics of technoscientific promises") and replace that with the "regime of collective experimentation" as the predominant force of innovation policy, as was the message of another (Felt et al., 2007).

Recently, concerns have been raised that these endeavors threaten to lead to a narrow and technical focus, as "[l]arger questions of the character and direction of scientific and technological change are effectively ignored whilst "risk" comes to be defined in narrow, technically measurable terms" (Irwin, 2006, 302). As a result of reducing everything to monetary loss or mortality rates, "a discourse of values is pushed aside by a discourse of valuation" (Rayner, 2007, 168). How this tendency can be avoided is just one of several unresolved questions with regard to current practices of deliberative science governance. Others concern the uptake of results of participatory procedures (how to make sure that they make a difference), their inter-relationships with other modes of science governance, their concrete aims (e.g., consensus or compromise) and the question of how scientific advice can be integrated into the process (cf. Hagendijk et al., 2005).

These are important concerns and they remind us that the practice of deliberative procedures may still have a long way to go before they can really fulfill the high expectations set in them. It is useful to remember that deliberative conceptions have been proposed by democratic theorists motivated by the idea that a "well-functioning system of democracy rests not on preferences but on reasons" and that democracy must therefore "offer a system in which reasons are exchanged and evaluated" (Sunstein, 1997, 94). To realize such systems in the area of science policy is obviously not an easy task, but may be well worth the effort.

We openly admit that our analysis does not offer any insights on how the practical problems of deliberative science governance can be overcome. Neither will we try to develop answers to the possibly even trickier questions that will be raised by any attempt to grant deliberation a more dominant role in vital areas of science policy such as the funding system. (e.g., at what level[s] and at which stage[s] of the process should deliberation step in?) The level of analysis we have chosen does unfortunately not permit such detailed conclusions.

Instead, what we think our analysis offers is an additional reason why the experiment should be undertaken despite the obstacles. Over and above the *general* aim of transforming the democratic process from a process of public bargaining into a process of public reasoning, deliberative science governance is desirable also on account of the *specific* logic of the tensions that govern the area of science policy. Taking up the multiple challenges involved in implementing deliberative science governance may thus be worthwhile because deliberative procedures could for the first time offer a framework in which the full plurality of reasonable concerns and arguments related to science can be acknowledged and negotiated on a case-by-case basis rather than subsumed under some schematism.

Within our framework, the argument from democracy may be expected to bear special significance for deliberative procedures. Recall that we have interpreted it as calling for a buffer that averts immediate government influence on the procedures of science, so that one might ask whether deliberative science governance can provide such a buffer. We are optimistic that it can, provided that its instruments are carefully applied. In particular, the buffering idea implies that it should not be government agencies – or at least not government agencies alone – who define the exact problems to be discussed and the exact modes of discussion within a given deliberative procedure. Very similar conclusions have been drawn by the authors of a large empirical study of participatory procedures in European science governance, who criticize "[t]he current tendency . . . [of] government to impose a framework on deliberation which suits its own short term policy needs rather than engaging with public problem definitions and concerns" (Hagendijk et al., 2005, 27). Their recommendation is that "public groups should participate in the initial stage of problem definition (ie in deciding what needs to be discussed and how) rather than being forced into a sometimes-problematic framework" (*ibid,* 99). If these recommendations are taken seriously, deliberation may provide just the kind of buffering that the often highly politicized contexts of contemporary science policy demand.

We acknowledge of course, as many have pointed out, that there is no guarantee that deliberations will ease or eliminate tensions. But participative procedures need not result in consensus in order to serve important purposes. Deliberation helps to bring to light and clarify the nature of conflicts that would otherwise seethe away covertly (Gutmann and Thompson, 1996, 43). It also compels conflicting parties to cast their proposals in relation to *shared* values and *common* interests, instead of just presenting their group-interested standpoints, and thereby serves to focus debate on the public good (Cohen, 1989, 68–69, 76–77). Thus, even if a deliberative effort should result "only" in cooperation or compromise rather than consensus, we believe that this compromise will typically be a more justifiable one than the many compromises that are hidden in conventional modes of science governance.

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Integrating the Ethical into Scientific Rationality

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If we try to characterize in a general way what it is we philosophers of science do, we typically come up short. We frequently find we have more in common with colleagues in the various sciences, or the history of science, or the sociology of science, or cognitive science, or social epistemology, or the history of philosophy, or some other area instead, than we have with each other. Interdisciplinarity seems to be our strength. But our interdisciplinarity has its limits. Few of us have much in common, in terms of academic pursuits, with those who regularly analyze science from a moral or political point of view, still less with those who, from a moral or political point of view, actively endorse some approaches in science and not others. How research funds should be allocated, priorities set, research organized, results communicated and applied, and accountability maintained – how all of this should (morally or politically should) be done – is seldom what professionally engages our attention, even though policy decisions about all of this have profound effects on what does concern us. It is as if Rudolf Carnap's description of the dominant attitude in philosophy of science nearly a century ago was a description of us:

> All of us in the [Vienna] Circle were strongly interested in social and political progress. . .. But we liked to keep our philosophical work separated from our political aims. In our view, logic, including applied logic, and the theory of knowledge, the analysis of language, and the methodology of science, are, like science itself, neutral with respect to practical aims, whether they are moral aims for the individual, or political aims for a society. (Carnap, 1963, 23)

But much has changed since the early twentieth century. Research in the history, philosophy, and sociology of science has shown that science itself is shot through with values; very little of it is morally and politically neutral. So a moral/political approach toward it has been rendered much more natural. What's more, recent events disclose a potent mix of moral and religious and political and economic interests shaping science more vigorously than at almost any time in the recent past:

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witness the current furor over the "politicization" and "commercialization" of science. So a moral/political approach toward science has now been rendered much more urgent. Though we have, for the most part, failed to take up this approach, the time is right to do so. But how? How can our role as philosophers of science be thought to encompass such an approach?

A Glance at the Past

It was the middle of the twentieth century when Carnap wrote his reminiscence of the Vienna Circle, and by then philosophy of science had come into its own as a professional discipline. Indeed, by then new academic departments and privately endowed centers had formed that were devoted exclusively to philosophy of science, new philosophy of science journals and conference series and book series were launched, and government support for research was expanding through such sources as the National Science Foundation (for more details see Howard, 2003). In addition, philosophy of science by then enjoyed increasing prestige within traditional philosophy departments and a preeminent place among the various science studies fields. All this prominence achieved by philosophy of science was doubtless connected, at least in part, to the distinctive goal philosophy of science had adopted. Unlike other areas of philosophy such as ethics or epistemology or metaphysics, philosophy of science sought to engage with and contribute to *science*, then (and probably still) considered the most impressive, most progressive, most demanding of human endeavors. And unlike other science studies fields such as history of science and sociology of science – which also engaged with science – philosophy of science sought not simply to describe science but to articulate and even improve upon what lay at the very heart of its success, scientific rationality itself.

As impressive as the goal of philosophy of science was, however, the mode of its pursuit left something to be desired. Indeed, at mid-century, as is well known, only the logical aspects of science were thought relevant to scientific rationality – only those logical aspects, in fact, related to Hans Reichenbach's context of justification. Articulating and improving upon scientific rationality meant reconstructing science using the modes of conceptualization provided by formal logic and empiricist epistemology – reconstructing science in such a way as to maximize the virtues considered essential to those modes of conceptualization. This was, of course, the understanding of philosophy of science's goal associated most famously not only with Hans Reichenbach but also with Rudolf Carnap and Carl Hempel. Thus, scientific theories were represented as axiom systems partially interpreted by an observational language itself interpreted on the basis of observation. Explanations invoking such theories were represented as the logical derivation of the statements to be explained (the "explananda") from the theories and statements of initial conditions (the "explanans"). The assessment of these theories was represented as the logical derivation of observation statements ("predictions") (from the theories in conjunction with statements of initial conditions) and the comparison of those predictions with statements describing the results of observation or experiment. And so on. But, as Thomas Kuhn, Paul Feyerabend, Imre Lakatos, Stephen Toulmin, and a host of others made especially clear, those logical aspects of science, though they were surely relevant to an understanding of scientific rationality, were far from the whole story. Indeed, the critics suggested that those logical aspects provided very little of the story – that what philosophy of science was offering as an account of scientific rationality was of surprisingly little relevance to actual science.

No matter. By the end of the twentieth century the mid-century critics and those they influenced had more than compensated for the first lean offerings of professional philosophy of science. No longer was scientific rationality thought to be confined merely to Reichenbach's context of justification, it was now understood to encompass as well the abductive and other reasoning processes that populated his context of discovery. Nor was scientific rationality confined any longer to *logic*, whether of discovery or justification. Indeed, social factors such as competition and cooperation among scientists, and particular patterns of consensus and dissensus, were found to contribute to scientific success and it became possible to speak of the rationality of various modes of community organization and community practice as well as the rationality of various modes of individual behavior. Even the more material aspects of science such as scientific instrumentation and scientific modeling were found to relate to scientific rationality since they embedded within them scientific knowledge and thereby contributed to the development of new knowledge. And the old questions of theory structure and theory validation were treated in new ways, ways that were informed by historical accounts of the temporally extended research programs that generated such theories and determined the conditions of their acceptance or rejection. By the end of the twentieth century, in short, articulating and improving upon scientific rationality was found to require involvement with a great many aspects of science, historical and social and material as well as logical, and the resources from a variety of fields – including the history of science, the sociology of science, cognitive science, social epistemology, and the history of technology – were required to do this well. But by century's end, articulating and improving upon scientific rationality was not found to require involvement with the *ethical* aspects of science and, hence, no resources from such fields as ethics or political philosophy or public policy were required to do philosophy of science well. True, feminist philosophers of science were speaking a great deal about the ethical aspects of science and how they relate to scientific rationality, and philosophers of biology were occasionally taking up such topics as the ethical implications of the genome project or the status of creation science or the political as well as conceptual and empirical problems associated with sociobiology. But for the rest, to quote from a 1996 essay by Phillip Kitcher and Nancy Cartwright, the ethics of science was "virtually unexplored territory" (Kitcher and Cartwright, 1996, 149).

Meanwhile, in the sciences – that which philosophy of science was supposed to be about – new or newly revised ethical codes were proliferating by century's end. On the American scene, the American Physical Society adopted its "Guidelines for Professional Conduct" in 1991 (updated and expanded in 2002), the American Chemical Society adopted "The Chemist's Code of Conduct" in 1994, the Society for American Archaeology adopted the "Principles of Archaeological Ethics" in

1996, the American Sociological Association approved its revised "Code of Ethics and Policies and Procedures" in 1997, the American Society for Biochemistry and Molecular Biology approved its "Code of Ethics" in 1998, and the American Psychological Association adopted the most recent version of its "Ethical Principles of Psychologists and Code of Conduct" in 2002 – to cite just a few examples. On the international scene there were, for example, the "Uppsala Code of Ethics for Scientists," published in 1984 and considered a basis for later international guidelines, "The Toronto Resolution" of 1991, whose purpose was to create a common moral framework worldwide for the conduct of research, and the "Code of Conduct for Scientists," called for by the 1999 World Conference on Science organized by UNESCO and the International Council for Science, to be prepared by 2007. All these codes acknowledged scientists' multiple responsibilities – for example, to their individual disciplines and to science in general, to society and to the environment, to their employers, employees, coworkers, and students, and to their human, animate, and even inanimate (e.g., archaeological) subjects of investigation. All these codes thereby acknowledged, whether explicitly or implicitly, potential conflicts arising from scientists' multiple responsibilities – conflicts between scientists' epistemic responsibility to advance science and their ethical responsibility to serve the public good, for example, or conflicts between scientists' epistemic responsibility to obtain and disseminate particular kinds of information and their ethical responsibility to protect the subjects of their investigations from harm, or conflicts between scientists' epistemic as well as ethical responsibility to share information with other scientists and their ethical responsibility to safeguard the proprietary information of their employers. All these codes, as a result, illustrated the entanglements in science of the ethical and the epistemic. For scientists at century's end, in short, unlike for philosophers of science, articulating and improving upon scientific rationality did obviously require involvement with the ethical aspects of science.

Challenges of the Present

If the ethical codes proliferating at century's end pointed toward a fuller understanding of scientific rationality, they were also hampered by a variety of weaknesses, most notably vagueness and incompleteness. Consider, for example, the issue of fraud. A growing concern over fraud in science was surely one of the factors that motivated at least many of the American codes of ethics that appeared at century's end. There was, after all, a succession of well-publicized cases of "scientific misconduct" in prominent U.S. research institutions. It started in the 1970s with the case of William Summerlin, chief of transplantation immunology at Sloan-Kettering, who claimed he could transplant, even across species, corneas, glands, and skin that would normally be rejected; he was discovered only after 3 years of this when a lab assistant noticed that the black "skin graphs" were drawn on with a marker (Judson, 2004). The publicized cases of misconduct even included the so-called "Baltimore Affair" in which a paper, coauthored by Nobel Prize winner and soon-to-be President of Rockefeller University David Baltimore, was suspected of suppressing negative evidence and even making use of fabricated evidence; the case took 10 years to settle, involved investigations by the National Institutes of Health and two universities as well as Congress, and is still controversial (see, e.g., Kevles, 1998 as well as Judson, 2004). And there was a succession of studies suggesting that the varieties and extent of the misconduct went far beyond the public cases. *Betrayers of the Truth: Fraud and Deceit in the Halls of Science*, for example, written by *Science* news reporters William Broad and Nicholas Wade (1982), provided the first substantive overview of the extent of scientific misconduct. *Stealing into Print: Fraud, Plagiarism, and Misconduct in Scientific Publishing,* by science policy analyst Marcel LaFollette (1992), analyzed scientific publication practices, including peer review and journal editorial policies, and the ways in which they allowed for scientific misconduct. And *Impure Science: Fraud, Compromise, and Political Influence in Scientific Research* by economist Robert Bell (1992) drew on case studies from the National Institutes of Health, the National Science Foundation, and the Department of Defense to argue that contemporary U.S. science was being corrupted by money and politics, that is, by its "patronage" system. There were also other kinds of studies – for example, the Acadia Institute Project on Professional Values and Ethical Issues in the Graduate Education of Scientists and Engineers (see Swazey et al., 1993), funded by the National Science Foundation, which focused on the academic research environment itself and found that, of the 2,000 doctoral candidates and 2,000 faculty surveyed in 99 of the largest graduate departments in chemistry, civil engineering, microbiology, and sociology in the U.S., 50% of the faculty and 43% of the students reported direct knowledge of at least two types of misconduct in their laboratories ranging from faking research results to withholding results from competitors, and 53% of the students and 26% of the faculty revealed that they were unlikely to report such misconduct for fear of reprisals. All this scientific misconduct played an important role in motivating the proliferation of ethics codes in the sciences at century's end – codes at least one of whose purposes was to prevent the misconduct.

But the mode of prevention was frequently very flimsy indeed. Take, for example, the American Chemical Society's "Chemist's Code of Conduct" (American Chemical Society, 1994). It maintains that chemists have responsibilities to nine entities – the public, the science of chemistry, the profession, the environment, employers, employees, students, associates, and clients, and regarding the profession in particular it maintains that "conflicts of interest and scientific misconduct, such as fabrication, falsification, and plagiarism, are incompatible with this Code." But it leaves completely unexplained exactly what "fabrication, falsification, and plagiarism" are. For example, is the common practice of "gift authorship," in which the names of senior researchers are included on papers that they had no part in producing just because the work was done in their labs, funded by their research grants – is this practice *plagiarism* – the senior researchers taking credit for work that is not really theirs? Or is it rather *exploitation* of the students or associates whose work it is – which is incompatible with other parts of the "Chemist's Code" than the part quoted, the parts that specify a chemist's responsibilities to students and associates rather than the profession? Or is the practice of gift authorship, instead, something laudable, part of promoting the "professional development" of students, helping them to get ahead by associating their names with the names of established scientists – something *supported* by the Code? Is gift authorship, in short, "intellectual corruption" (Richard Lewontin's, 2004 description), part of the "culture of fraud" that is science today (Horace Judson's, 2004 description)? Or is it, as many other scientists claim, just "standard practice," harmless and acceptable (see LaFollette, 1992, 100).

Or consider another part of the "Chemist's Code of Conduct" and another motivation for at least some of the new ethics codes at century's end. Concern over global problems – concern over ever mounting threats to the environment, ever mounting threats to public health, ever mounting threats to world peace – concern over these problems also played a role in the appearance of some of the ethics codes. The expectation had always been that science would solve such problems but by the end of the twentieth century there was a growing sense that science had failed to solve them and may even have made them worse.[1] As one of the background papers for the 1999 World Conference on Science, the conference that was to produce a new international code of ethics for scientists, proclaimed:

> Today . . . science suffers from a serious image problem. In large parts of the world, people no longer conceive of science as being essentially a benefactor of humanity, nor do they readily associate science with the classical quest to develop a more enlightened civilization. Trust in the ethical integrity and responsibility of scientists is declining partly to be replaced by suspicion and fear of abuses of various kinds. . . .
>
> . . .The present state of affairs calls for a powerful statement about the ethical responsibilities of science towards society and present or future generations, and towards the environment. (International Council for Science's Standing Committee on Responsibility and Ethics in Science, 1999)

But all that the "Chemist's Code of Conduct" (American Chemical Society, 1994) says about the ethical responsibilities of chemistry to society and present and future generations is: "Chemists have a professional responsibility to serve the public interest and welfare and to further knowledge of science. Chemists should actively be concerned with the health and welfare of co-workers, consumers and the community. Public comments on scientific matters should be made with care and precision, without unsubstantiated, exaggerated, or premature statements." And all that the "Chemist's Code of Conduct" says about the ethical responsibilities of chemistry to the environment is: "Chemists should understand and anticipate the environmental consequences of their work. Chemists have responsibility to avoid pollution and to protect the environment." But the Code leaves completely undefined what all these responsibilities amount to, these responsibilities to serve the public interest and welfare, avoid pollution, and protect the environment, and it is important to remember that the "Chemist's Code" also covers the ethical responsibilities of

[1] Global warming formed a particularly painful example. During the last decade of the twentieth century and the first few years of the twenty-first the U.S. spent upwards of $25 billion on global climate system research as a basis for creating appropriate climate policy, but it has yet to take any meaningful action on such policy (Sarewitz, 2006). The problem, meanwhile, keeps getting worse.

chemists to their employers – who of course include, among others, manufacturers of petrochemicals and the pharmaceutical industry. These responsibilities to employers read: "Chemists should promote and protect the legitimate interests of their employers, perform work honestly and competently, fulfill obligations, and safeguard proprietary information."

Other ethical codes within the sciences have comparable problems. The American Physical Society's (2002) "Guidelines for Professional Conduct," for example, though far more precise in dealing with the issues of gift authorship and other kinds of scientific misconduct, never even mention physicists' responsibilities to society or future generations or the environment or even physicists' responsibilities to their employers, many of whom, of course, are involved in the development and deployment of military weaponry. In fact, the only responsibility of physicists noted in their code is their responsibility to *science*, physics in particular:

> The Constitution of the American Physical Society states that the objective of the Society shall be the advancement and diffusion of the knowledge of physics. It is the purpose of this statement to advance that objective by presenting ethical guidelines for Society members.
>
> Each physicist is a citizen of the community of science. Each shares responsibility for the welfare of this community. Science is best advanced when there is mutual trust, based upon honest behavior, throughout the community. Acts of deception, or any other acts that deliberately compromise the advancement of science, are unacceptable...

The "Code of Ethics" of the American Society for Biochemistry and Molecular Biology (1998), on the other hand, *does* acknowledge other responsibilities besides the responsibility to science – for example, the responsibility to the public "to promote and follow practices that enhance the public interest or well-being" and the responsibility to trainees "to create and maintain a working environment that encourages cultural diversity." Of course, these responsibilities are, again, unhelpfully vague, but the inclusion of the responsibility to encourage cultural diversity is still noteworthy. Or rather, its *absence* from *other* ethics codes is noteworthy given the prominence accorded the lack of diversity in the sciences at century's end. The examples go on and on.

Hopes for the Future

The needs that have motivated the ethics codes in the sciences are not being met. If anything, they are getting more pressing. But scientists tend to be reluctant to be policed by outsiders and outsiders, for their part, tend to be reluctant to police science (for examples of this see Goodstein, 1995). The ethics codes, by contrast, have been constructed by scientists and are enforced by scientists. What's more, they – some of them – have been revised when inadequacies have been uncovered,[2] and

[2]For example, the American Psychological Association's "Ethical Principles of Psychologists and Code of Conduct" has been revised nine times since 1953. See American Psychological Association (2002).

they can be revised again. So the ethics codes ultimately represent scientists' policing themselves. But the ethics codes tend to be inadequate. Helping to make them adequate thus becomes an important project, not only because the ethics codes are scientists' own responses to their own serious problems but also because adequate ethics codes define the goals to which all other responses are directed. For example, sociological research into the structural conditions (e.g., funding, publication, and science journalism practices) that encourage scientific misconduct and resulting policy recommendations for changing those structural conditions to discourage such misconduct all depend on an understanding of what constitutes appropriate and inappropriate scientific conduct and it is this that adequate versions of ethics codes are intended to supply.[3] They also provide a basis for funding decisions and serve important pedagogical functions for both scientists and the public at large.

Helping to make the ethics codes in the sciences adequate is thus an important project – an important *normative* project, one that looks deeply into not only the goals and attendant responsibilities that scientists do set for themselves, both individually and collectively, but also the goals and responsibilities scientists ought to set for themselves. It is, moreover, an important epistemic as well as ethical project. Indeed, helping to make the ethics codes adequate responds to needs that are *both* epistemic and ethical. Scientific fraud, for example, is not only unethical but also a serious threat to the validity of accepted scientific knowledge. Exclusionary practices within the scientific community – practices that exclude women and minority men from equal opportunities with white men, for example – are not only unethical – unjust – but are also epistemically damaging since they decrease the pool of available talent and, as the work of women scientists during the last few decades amply demonstrates, also tend to leave in place the biases associated with the group that excludes. The failure of scientists to respond to the legitimate needs of society is not only unethical – an unfair recompense for the support society has lavished on science – but is also a threat to society's continued support of science and thereby to science's continued epistemic success. And so on. And, of course, looking deeply into the goals that scientists ought to set for themselves involves epistemic

[3] Against the charge – cf., e.g., Franzen et al. (2007) – that everyone already knows what constitutes appropriate and inappropriate scientific conduct it must be noted that everyone is learning this neither from the generally vague ethics codes now available nor from professors and mentors and coworkers who, according to the Acadia study previously mentioned (Swazey et al., 1993) as well as more recent studies (e.g., Martinson et al., 2005), are frequently poor models of appropriate scientific conduct. Note, also, that the category systems of misconduct used by these studies are themselves contested (see, e.g., Wadman, 2005). And when we remember that "appropriate scientific conduct" covers much more than non-fraudulent scientific conduct – covers, e.g., research activities genuinely beneficial to society – the need for adequate ethics codes to counteract what everyone learns from prevailing practices becomes even more apparent. Small wonder that "The Toronto Resolution" (see Faucett, 1993) calls for adequate ethics codes to be "widely disseminated through the school and university curricula, to educate rising generations, as well as practicing scientists and scholars, about their emerging responsibilities."

considerations (such as the feasibility of certain kinds of endeavors[4]) as well as ethical considerations (such as the social value of those endeavors). The upshot: Helping to make the ethics codes in the sciences adequate connects to traditional normative and epistemic concerns of philosophy of science as much as it departs from those concerns – or better, discloses a whole new set of connections to those traditional normative and epistemic concerns, ethical connections. If it is time philosophers of science integrate the ethical into our conception of scientific rationality, exploring how to make the ethics codes in the sciences adequate is an excellent place to start.

Of course, many have warned that history discloses hazards to such interventions in science, the most notorious being what happened in the Soviet Union under Stalin and Germany under Hitler. At least in those cases, it is said, determining from outside of science (or "helping" to determine) the goals and responsibilities of scientists yielded disaster. Are there, then, dangers in this new project of philosophers of science visible in such historical cases, dangers with which philosophers of science need now to be apprised?

Lessons from Lysenko

Start with the case of the Soviet Union and the infamous Trofim Lysenko. The historical details are uncontroversial (see, e.g., Graham, 1987; Lewontin and Levins, 1976; Roll-Hansen, 2005 for what follows). By the end of the 1920s – it was then that the short-lived Russian eugenics movement had come to a close under heavy pressure from the political authorities – the attempt to explain human behaviour in terms of innate characteristics was considered illegitimate in the Soviet Union. Instead, social environment was billed as the most important influence on human behaviour and stress was laid on the possibility of moulding the personalities and talents of children by constructing a suitable social environment. Crime, alcoholism, prostitution, and other social ills were expected to vanish under the influence of the right educational and political and economic conditions. This was, at any rate, the Marxist social/scientific goal – human perfectibility, social equality, the "new Soviet man" – and it contrasted sharply with what was widely understood to be the elitist message of classical genetics: that genes determine traits and abilities, that genes are (relatively) immutable, and hence that the social hierarchy traits and abilities determine is also (relatively) immutable. (Note that it had been distinguished geneticists such as Hermann Muller, Nikolai Koltsov, Nikolai Vavilov, and Aleksandr Serebrovskii who favored Soviet eugenics programs – e.g., Serebrovskii, who proposed large-scale artificial insemination of Soviet women with the sperm of outstanding men.)

Lysenko's agricultural research program, concerned with the directed transformation of biological varieties (interpreted as the directed transformation of heredity)

[4]The realism/anti-realism controversy in philosophy of science illustrates such epistemic considerations. See, for example, van Fraassen (1980) and Kourany (2000).

by means of environmental manipulation, fit right in with the new state-sanctioned social/scientific goal although neither he nor his followers typically applied his results to the human case.[5] Lysenko's research program also fit in with that goal's methodological underpinnings – the Marxist unity of theory and practice, the view that scientific research should have a clear social purpose by being tied to the needs of society and should be evaluated thereby (the "practice" criterion of scientific truth). Geneticists such as Muller, Koltsov, Vavilov, and Serebrovskii had been members of the intellectual middle classes of pre-revolutionary Russia and the rift these individuals took for granted between their frequently highly theoretical research and agricultural practice was, as one observer of the time described it, "a capitalist remnant in the mind of the individual scientific workers," which made them "lock themselves up in their laboratories and not move further than their greenhouses" (Aleksandr Muralov, quoted by Roll-Hansen, 2005, 101). By contrast, Lysenko, who came from peasant origins and received the bulk of his technical training after the revolution, aimed to and did use the state and collective farms as his laboratory, directly involving the peasants and their farming experience with his research and not only with its results. For him, large-scale practical experience was superior to "pure" scientific experiments in deciding the truth of theories. "In every aspect the conflict in agriculture was a revolutionary conflict, posing the detached, elite, theoretical, pure scientific, educated values of the old middle classes against the engaged, enthusiastic, practical, applied, self-taught values of the new holders of power" (Lewontin and Levins, 1976, 51).

What led to the downfall of Lysenko's program, and with it Soviet agricultural science, was none of this, however. What led to the downfall of Lysenko's program was its epistemic failings. To begin with, the concepts Lysenko relied on were extremely vague. *Vernalization*, for example, one of Lysenko's key concepts, covered almost anything that was done to seeds or tubers before planting, *nutrient* seemed to include everything from chemical elements in the soil or organic food or gases present in the atmosphere to environmental conditions such as sunlight, temperature, and humidity, and his Theory of Phasic Development of Plants never clearly differentiated or coherently described the different plant phases he was proposing. Lysenko's experimental failings were more worrisome than his conceptual failings, however. His claim to have converted the winter wheat Kooperatorka into a spring wheat, for example, was based on a year-and-a-half-long experiment with a single plant and its offspring (the experiment started with two plants but one perished because of "pests gnawing its roots" [quoted by Roll-Hansen, 2005,

[5] The human case cropped up, however. Thus, for example, when Hermann Muller called attention to "the fascist race and class implications of Lamarckism, since if true it would imply the genetic inferiority, at present, of peoples and classes that had lived under conditions giving less opportunity for mental and physical development," Yakovlev replied that "the genes of man had been changed by the environment of civilization and therefore primitive races existing today have inferior genes. But . . . about three generations of socialism will so change the genes as to make all races equal. Just better the conditions and you better the genes" (Hermann Muller, in a letter to Julian Huxley, as quoted by Roll-Hansen, 2005, 203, 214).

201]) – an experiment whose results were never duplicated either in or out of the Soviet Union. Experiments involving larger samples had other problems: "in the conditions of Soviet farms, where there was often no electricity and no refrigerating equipment, it must have been nearly impossible to keep the seeds in uniform conditions over long periods of time" (Graham, 1987, 109), and experimental conditions frequently presented an ideal environment for the spread of fungi and plant diseases. What's more, the Russian plant varieties used in these experiments were of unknown (and doubtful) purity and there was an almost total absence of control plots (Lysenko frequently presented his evidence in terms of yields in a certain season with both treated and untreated plantings of the same crops, but the comparisons were not rigorous enough to serve as controlled samples). Finally, extremely inaccurate records were kept of trials and diverse ways to discount negative results were readily available (peasants' lack of cooperation with extremely labor-intensive procedures, impure plant varieties, plant diseases, variable weather conditions, and more). Risky predictions were not made in any case. For example,

> Vernalization was only very rarely used as an attempt to make possible the previously impossible – growing crops that had never been grown before in the region because of the climate. Rather, it was usually directed toward making traditional crops ripen earlier or growing a grain that because of the length of its growing season could only occasionally be successfully harvested by traditional methods in a certain region before frost. These are the kinds of experiments in which the evidence can be manipulated very easily, or where sloppiness in record-keeping can conceal results from even an honest researcher. (Graham, 1987, 110)[6]

And then there were the Stalinist tactics used to ensure the survival of Lysenkoism and the suppression of its critics – the elaborate system of formal censorship, the informal self-censorship created by constant threats of terror and political reprisals, the loss of professional positions, the imprisonments, the executions – tactics that compounded Lysenko's epistemic failings. These tactics had as little to do with the state-sanctioned Marxist social/scientific goal as Lysenko's epistemic failings. But it was these tactics and these epistemic failings rather than the Marxist goal that led to the downfall of Soviet agricultural science.

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[6]Even Roll-Hansen, 2005, who takes Graham as well as other historians to task for at times insufficiently recognizing "the strength of valid scientific support for parts of Lysenko's work" (293) and aims, instead, to give a more balanced perspective on Lysenko, clearly acknowledges in the end Lysenko's "unscientific methods of experimenting and arguing" (295). Lewontin and Levins also provide an especially open-minded and sympathetic portrayal of Lysenko though they still conclude that

> In the end, the Lysenkoist revolution was a failure. It did not result in a radical breakthrough in agricultural productivity. Far from overthrowing traditional genetics and creating a new science, it cut short the pioneering work of Soviet genetics and set it back a generation. Its own contribution to contemporary biology was negligible. (Lewontin and Levins, 1976, 33)

Nazi Science

The case of the Nazis was fundamentally different (see for what follows Deichmann, 1996; Jewish Virtual Library, 2007; Proctor, 1988, 1999, 2000). True, it was said in Germany under Hitler as it was in the Soviet Union under Stalin that the goal of science was to serve the people. But in Germany "serving the people" was understood in a very peculiar way. Indeed, in Germany it was biology, not social environment, that was held to be the most important determinant of human character and human institutions and hence, if problems were found in the latter (human character or institutions), the causes and cures would have to be found in the former (human biology). In particular, the economic and social problems facing Germany after World War I were traced to such factors as the "degeneration" of the "German racial stock" or the ill health of the "German genetic streams" and the solution to these problems was said to lie in "racial hygiene" or "racial cleansing" – isolating and removing the causes of the degeneration. Thus, there were the miscegenation laws banning marriage between Germans (Aryans) and Jews and between "healthy" Germans and Germans with afflictions such as venereal disease or feeble-mindedness; there was the sterilization of alcoholics and those with hereditary diseases such as schizophrenia; there was the euthanasia of retarded and handicapped children and adult psychiatric patients; there was the expulsion of Jews from professional life and their segregation in ghettos; and finally there was the extermination of Jews and gypsies and homosexuals, communists, the handicapped, prostitutes, drug addicts, the homeless, the tubercular, and anyone else stigmatized by German racial scientists as "degenerate" ("Lebensunwertes Leben" or "life unworthy of life") – the "final solution."

In Germany under Hitler, in short, serving the people meant serving – perfecting – the "race," that is, serving some of the people while ignoring the good of, devaluing, subjugating, and finally murdering the rest of the people. And so the science that was done to serve the people, understood in this way, included all sorts of racist genetic and biomedical and anthropological and psychological research – into the links between Jews and criminal behavior, Jews and mental infirmity, Jews and homosexuality, and Jews and the dangers of racial miscegenation, for example. It included, as well, all the science done in Nazi concentration camps – the experiments to investigate the effectiveness of various kinds of vaccines and other chemical substances on inmates who were deliberately infected with malaria, typhus, yellow fever, smallpox, cholera, diphtheria, or other diseases; the experiments to investigate the effectiveness of various treatments for hypothermia on inmates exposed for hours to freezing temperatures (which included investigating how long it took to lower the body temperature to death and at what temperature death occurred); the experiments to investigate the effectiveness of various treatments for wounds and burns previously inflicted on inmates and deliberately infected with bacteria such as streptococcus, gas gangrene, and tetanus (sometimes aggravated by forcing wood shavings and ground glass into the wounds); the experiments to investigate the effects of various poisons; to determine the easiest and quickest methods to sterilize millions of people; and so on. Of course, the science that was done to serve the people also included progressive public health-related research. For example, German cancer research, at the time the most advanced in the world, motivated the introduction of such health reforms as smoke-free public spaces, bans on carcinogenic food dyes, and new means of controlling occupational carcinogens. But this research was promoted and shaped by Nazi ideals of bodily purity and racial hygiene and it was suffused with Nazi rhetoric (as when the nascent tumor was characterized as "a new race of cells, distinct from the other cell races of the body," a pathological race that needed to be destroyed [Proctor, 1999, 47]). "The Nazi campaign against carcinogenic food dyes, the world-class asbestos and tobacco epidemiology, and much else as well, are all in some sense as fascist as the yellow stars and the death camps" (Proctor, 2000, 345). And as for the rest of German science during the Third Reich, it was characterized by widespread accommodation and cooperation with the Nazi authorities:

> When Hitler was preparing his seizure of power, he figured the German scientists into the equation as a *quantité négligeable*, and unfortunately he was right. I cannot shake the tormenting thought that it would have been possible to prevent much if, at the first moment Hitler attacked freedom and justice, a group of German scientists had protested." (geneticist and wartime Director of the Kaiser Wilhelm Institute for Biology Alfred Kuhn in *Wissenschaft und Freiheit* 1954, 269, quoted in Deichmann, 1996, 318)

The Moral

The Nazi scientific goal to serve the people, then, did lead to disaster – to epistemic as well as moral disaster (e.g., racist science) – while its Marxist counterpart in the Soviet Union, at least in the case of agricultural science, did not (though, of course, it did in genetics research, which was largely shut down). What conclusions can we then draw for philosophers of science seeking to help determine the goals shaping scientific research as well as other aspects of science relevant to making the ethics codes in the sciences adequate? Surely not that external intervention in science ought to be avoided. After all, compared to the Soviet Union, with its external imposition of a Marxist scientific goal on its "elite" scientists "of the old middle classes" – compared to the Soviet Union there was relatively little external intervention in science in Germany under National Socialism.[7] Indeed, scientists there were largely running the show:

[7] True, scientists were removed from their positions in both the Soviet Union and Nazi Germany, but they tended to be removed because of the nature of their scientific work in the Soviet Union whereas they were removed because of their "race," irrespective of their scientific work (which was, however, racially characterized – as "Jewish science," for example), in Nazi Germany. And true, many of the scientists in the Soviet Union were sympathetic to the political directions in which science was being taken by Stalin. But when these scientists became openly critical of the epistemic weaknesses of Lysenko's science they were imprisoned or executed nonetheless, despite their political sympathies.

> . . .Science (especially biomedical science) under the Nazis cannot simply be seen in terms of a fundamentally "passive" or "apolitical" scientific community responding to purely external political forces; on the contrary, there is strong evidence that scientists actively designed and administered central aspects of National Socialist racial policy. (Proctor, 1988, 6)

External intervention – good external intervention promoting good scientific goals – in this case could have helped, and its absence surely hurt. (Remember Alfred Kuhn's "tormenting thought" that "it would have been possible to prevent much if, at the first moment Hitler attacked freedom and justice, a group of German scientists" – and we can add here, philosophers of science – "had protested.") Even in the case of Lysenko's science external intervention promoting good epistemic values might have helped. What happened in the Soviet Union under Stalin and Germany under Hitler, in short, should give scant pause to philosophers of science and the project to integrate the ethical into scientific rationality.

Nearly four decades ago Paul Feyerabend wrote an essay entitled "Philosophy of Science: A Subject with a Great Past" in which he bemoaned the uselessness of the then-fashionable logical empiricism and went on to urge philosophers of science to pursue a very different kind of philosophy, one not only relevant to science but also fearless to criticize and even transform that science rather than conform to it. For this purpose Feyerabend suggested that philosophers engage in a detailed study of primary sources in the history of science, at least those primary sources in which philosophy was closely involved with the science that was done and helped to shape its development. "It is to be hoped that such a concrete study will return to [philosophy of science] the excitement and usefulness it once possessed" (Feyerabend, 1970). Now, decades later, there is a need once again to bemoan the uselessness of philosophy of science and urge philosophers of science to criticize and even transform science rather than conform to it. This time, however, the need is to be met by ethical study, not historical – by broadening our conception of scientific rationality to encompass the ethical aspects of science, by acknowledging the inextricable interconnections of the ethical and the epistemic. It is to be hoped that by broadening our conception of scientific rationality in this way we will indeed be able to return to philosophy of science the excitement and usefulness it once possessed – if not at the time of Feyerabend's Galileo then at least at the time of the Vienna Circle's Neurath and Frank.

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Part VI
Science, Values and Society: Historical Transformations

What Makes Computer Science a Science?

Michael S. Mahoney

Computer Science is no more about computers than astronomy is about telescopes.

E. W. Dijkstra

'Software engineering' is something of an oxymoron. It's very difficult to have real engineering before you have physics, and there isn't anything even close to physics for software.

L. Peter Deutsch[1]

What Is the Question? Paderborn 2000

Several years ago I gave the opening paper at a conference on "Mapping the History of Software" at the Heinz-Nixdorf Museum in Paderborn, titled "Software as Science – Science as Software" (Mahoney, 2002). Taking "software" to mean programs and the activity of writing them, I described the early formation of theoretical computer science as a mathematical discipline, focusing on automata, formal languages, and formal semantics, and then pointed to the ways in which concepts from that field were being applied to the sciences, especially the biological sciences, and making them increasingly computational in nature. Essentially I followed the narrative of the mathematization of the sciences, in which computation played the dual role of subject and agent. To my surprise, the ensuing discussion turned quite heated, revealing disagreements both with me and among the audience of computer people themselves over whether software (i.e., computer programs and programming)

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[1]Quoted by Rosenberg, *Dreaming in Code: Two Dozen Programmers, Three Years, 4,732 Bugs, and One Quest for Transcendent Software* (New York, Crown Publishers, 2007), 276. Deutsch had a distinguished career in the design and implementation of programming languages; see http://www.sigsoft.org/SEN/deutsch.html (accessed 15 July 2008)

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could be the subject of a science, whether that science was mathematics, and indeed whether mathematics could be considered a science at all. I addressed some of these points in an addendum to the published version of the paper, but I have been thinking about them since then. As historian I am, of course, agnostic on these questions. I have no stake in whether or not computing is a science or what kind of science it is. I am interested in what the historical actors thought about the issue, and both the literature and the reward structure of the field seem to make it clear that most of them considered it a science and a mathematical science at that. As Turing Award winner, C.A.R. Hoare put it in his inaugural lecture in the chair of computer science at Oxford in 1985, he held as self-evident (if unrealized) philosophical and moral principles that computers are mathematical machines, computer programs are mathematical expressions, a programming language is a mathematical theory, and programming is a mathematical activity (Hoare, 1989). If other practitioners have disagreed, as indeed they have, it makes the question all the more interesting and revealing of the dynamics of the field. In particular, their disagreements seem particularly pertinent to the issue of science in the context of application, and so I want to consider them in more detail in what follows.

There was lot of common wisdom in the room in Paderborn, expressed with all the certainty and confidence of common wisdom, for example, that mathematics is not a science but a creation of the human mind, independent of the physical world. Yet, that is not so much a principle as a philosophical puzzle, which Eugene Wigner so famously characterized as the "unreasonable effectiveness of mathematics in the natural sciences" (Steiner, 1998; Wigner, 1960). Why is the mathematics we create so uncannily true of the physical world? What is the relation of our mathematical ideas to our experience of the world? Whence arise the mathematical truths that we cannot (yet) prove, that is, what is the basis of our mathematical "intuition"? And what happens when mathematics provides our only access to realms beyond physical intuition, as in the case of quantum mechanics or string theory?

Or, to take another piece of common wisdom, "computers and the programs written for them are artifacts, not natural phenomena, and science is about natural phenomena." Here again the principle raises more issues than it settles, and they are directly pertinent to the topic of this volume. In the phrase "science in the context of application" the term "context" suggests – or even implies – a distinction between science and its application or, in older terms, between (pure) science and applied science. Behind the distinction lies a notion of a science as an autonomous, self-generating body of knowledge about nature, which runs the risk of distraction when it is addressed to or directed by problems external to it. Commerce, industry, and the military constitute threats to its integrity, misdirecting its goals and compromising its ideals. Science is about truth, not utility, and as institutions committed to the pursuit of scientific truth universities in particular should emphasize research as their mission and keep development at a distance.

Yet, recent work in the history, philosophy, and social study of science casts doubt on the validity of the distinction. The sciences are not about nature, but about our representations of nature, the models we construct through our interactions with it. Since the seventeenth century, machines have constituted the main medium of our interaction with nature and hence the basis of our representations of it. New machines have posed new questions and prompted new solutions, generating new sciences while reshaping the world they represent, in large part by presenting it in new ways. As clocks shaped celestial mechanics, so steam engines opened a world of thermodynamics, energy, and entropy; electrical machines, a world of electromagnetic fields; telecommunications, a world of information; and computers, a world of codes and computational processes. Astronomy looks quite different when the limits of the naked eye on the earth's surface are transcended by optical telescopes, radio telescopes, satellite laboratories, and space probes. The science has changed as new technical resources have expanded the universe beyond the visible spectrum. In that sense, pace Dijkstra, astronomy is very much about telescopes. The same may be said for all the sciences, which now depend almost entirely on artifactually generated phenomena, in most cases mediated by computer software.[2] The sciences thus produce the nature they study, and the test of their knowledge is their ability to produce it. That is not a new situation. One can date it back at least to the seventeenth century, when the categories of art and nature were joined and people began to construct natural phenomena in the laboratory, following Francis Bacon's admonitions that "Nature, to be commanded, must be obeyed" and that "Truth and utility are one and the same thing."

Hence, the artifactual nature of computers and of the programs running on them has nothing to do with the question of whether there is a science of software or of what kind of science it might be. So let me return to that question and to the discussion in Paderborn. The resistance among the audience to the notion of theoretical computer science as the science of software seems to have reflected two major sets of concerns born of experience in the field. First, the practitioners in the room felt that theory has in fact played little or no role in the development of the vast body of software produced in the last 50 years and hence that a knowledge of automata, formal languages, and formal semantics offers no real understanding of how software is actually created and how programs work. Second, a focus on the mathematical structure of computing takes no account of the human and social aspects of software production and of software in action. Whatever transpires in the machine, computer programs acquire meaning at their interface with the world and with humans, and a science of software grounded in the mathematical theory of computation affords no access to that meaning. On both counts, theoretical computer science divorces software from the context of application that is its *raison d'étre*. Computers hold little intrinsic interest. What makes them interesting is what one can do with them.

I'll return to the first point in a moment, but on the second several of my critics asserted that, in taking the science of software to be theoretical computer science, the mathematical science of computation, I had been misled by the English term "computer science", or rather by the "science" in the term. The German *Informatik*

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[2]On the artifactual basis of modern science, see Peter Galison, *Image and Logic: A Material Culture of Microphysics* (University of Chicago Press, 1997); Paul Humphreys, *Extending Ourselves* (Oxford University Press, 2002); and Davis Baird, *Thing Knowledge* (University of California Press, 2004).

and its European cognates, I was told, carried no such connotation and hence encouraged a more encompassing view of the field. At the time, I did not know enough to respond properly, except to note that a major portion of the magisterial two-volume *Handbook of Theoretical Computer Science* stemmed from European authors, many of whom were affiliated with institutions bearing the name "informatics" in some form. Only later did I come across several articles by Wolfgang Coy, in which he noted that in Germany in particular *Informatik* had developed in line with computer science in the American sense (Coy et al., 1992, 1997). As his sometime collaborator, Christiane Floyd, wrote at about the same time, computer science "views itself as a formal and an engineering science" and thus "... is firmly rooted in the established scientific paradigm, as is evidenced by its theoretical teachings as well as its professional practice" (Floyd, 1992). So, historically at least, I was on firm ground in equating *Informatik* with computer science.

Left out of that equation, and hence lost from historical view, were critical efforts to contest and change it. Some practitioners did have a more encompassing view, even if they had not succeeded in persuading the community as a whole. The scientific paradigm, Floyd noted, "emphasizes analytical thinking, experiments and proofs as basic elements of scientific methodology" and avoids questions of human values and needs. Coy felt the same concern. In the late 1980s, he and several colleagues in Berlin had undertaken a project to define a *Theorie der Informatik* distinct from and more broadly conceived than the theory of computation or computing. The volume *Sichtweisen der Informatik* (1992) emerged from their deliberations and was intended at the time as a first step toward their goal, indeed the first in a Vieweg series titled *Theorie der Informatik*. Dirk Siefkes continued the effort at the TU Berlin through the 1990s. (It is perhaps worth noting that much of this work has been carried out in German and remained untranslated and hence unknown to anglophones – despite the fact that Germans generally publish their computer science in English.) During the same period, Floyd had gathered several colleagues around the question of "Software Development and Reality Construction," aimed at bringing recent work in collaborative software design, phenomenology, and social studies of science to bear on an "epistemology of software development" that took account of the humans who created and used software. The effort to define a theory of informatics not centered on the computer wrestles with the question, if computer science is not a (the) mathematical science of computation (defined ultimately in terms of the Turing machine and its equivalents), what is it a science of and what kind of science is it, and what must a person know to pursue it?

Whatever the answers to those questions, to which I shall return below, it should be noted here that the broader view of *Informatik* would root the science firmly and inextricably in the context of application, which would become its very subject. It would be a theory of use and utility. It would thereby reinforce the peculiar perspective that computer science offers on the question of science in the context of application. As a name, "computer science" has an advantage over *Informatik:* it reminds us that there would be no such subject without computers, which handle information in special and specifiable ways. As a logical concept, the computer is a protean device: it does nothing on its own but can do anything for which one can provide the appropriate instructions. Computer science is about those instructions.

It began entirely in the context of application and has never separated itself from it. Even in its most abstract form, it is concerned with effective processes, that is, with doing something, and the ultimate test is a working program. Over time, however, research in the field has produced a science of broader scope that now, increasingly provides intellectual as well as instrumental access to the natural world. In providing models and tools for representing nature computationally, computer science has incorporated itself into the natural sciences and has thereby involved them in its context of application.[3] Indeed, it has undermined any distinction between science and application.

Yet, perhaps ironically, the felt need to define computer science as an autonomous discipline initially rested on that distinction, which took concrete form in a bifurcation between the academic subject and the commercial development of computing. For the first decade or so following the inauguration of ENIAC as a proof of concept of electronic digital computing, practitioners faced two basic problems: designing machines that worked well enough to solve problems people wanted solved and writing the detailed instructions that produced the solutions. At first, the people who wanted solutions were scientists and engineers whose computational needs had outstripped the mechanical and electromechanical resources they had been using. Beyond that immediately interested community of users, who were able and willing to learn enough about the new device to apply it to their activities, establishing a market for computers was a matter of persuading people that they wanted computers to solve problems they had already been solving by other means and of providing them with the necessary support. Thus the possibilities of the computer came to lie in the hands of communities of practitioners in a variety of domains, who sought to translate their knowledge and practices into effective procedures expressible as sequences of instructions. The task was neither straightforward nor easy, and the interest of the growing number of manufacturers of computers lay in facilitating it by providing faster and more capacious machines and by assisting in the writing of application programs and in making the task of programming easier. That was the strategy followed by IBM, once the company decided to build computers to replace their earlier electrical accounting machinery. In the late 1950s,"software" emerged as a product distinct from hardware, and by the mid-1960s programming had become a business in itself, sharing the interest of users and manufacturers in the development of tools and systems to enhance the productivity of programmers, who by that time were increasingly in short supply. "Wanted: 50,000 Programmers" announced the title of a 1967 article on the state of the field (Bylinsky, 1967).

The growing concern with programmers and their productivity in the 1960s reflects a peculiar aspect of computing as a technology. In a sense, programmers came into existence only with the spread of computers beyond the realm of science and engineering. At first, it was assumed that people wanting to use the computer for numerical calculation would be willing and able to learn enough to write their

[3] See for example the work of Ehud Shapiro et al. on computers made of biological molecules. For the philosophical and methodological implications of this development, see Paul Humphrey's contribution to this volume, as well as his *Extending Ourselves: Computational Science, Empiricism, and Scientific Method* (Oxford University Press, 2002).

own programs. The manual for the EDSAC at Cambridge was written for such a user (Wilkes et al., 1951). But the task was soon delegated to others, whose job it became to translate a suitably formulated symbolic solution into the operational codes of the machine. Users of EDSAC could take advantage of a library of common routines, which they could include in their programs using a common protocol. That division of labor followed the computer into business and industry, as "coders" attended to the details of schematic solutions, often in the form of flow-charts, laid out by "program analysts" following the lead of "systems analysts" (Ensmenger, 2001). Coders and program analysts soon coalesced into programmers, and as the market burgeoned it became clear that the job required no special qualifications, or at least no qualifications that could be specified. Anybody, it turned out, could learn to program, and anybody from any background could turn out to be good at it – or, for that matter, bad at it.[4] Despite the efforts of test designers, there seemed no way to measure aptitude for the task or to predict success at it. Programming became a craft skill, putting control over the pace and quality of work in the hands of the programmers, to the growing consternation of their managers. But at first it was a special kind of craft skill, dependent for its very existence on a large artifact affordable only to large organizations, which controlled access to it. The appearance of the personal computer at the turn of the 1980s opened access to individual owners, who again required little or no formal training to become skilled programmers and developers.

How Theoretical Computer Science Became a Mathematical Discipline

By the mid-1950s commercial computers began to arrive on college and university campuses beyond those, such as Harvard, MIT, and Princeton, where the earliest experimental models had been built. The IBM 650 led the way, all but donated to institutions who wished to have them. Depending on local circumstances, the machines were introduced under a variety of auspices and thus fell under the aegis of different departments, which used them in different ways. At first looked upon as a service, computers did not raise curricular issues until the mid-1960s, when both faculty and students began to push for their recognition as a subject of study. For faculty, establishing a place in the curriculum meant defining the subject as an autonomous discipline. In the years following the Association for Computing Machinery's (ACM) Curriculum '65, the question of the nature of computer science and its identity distinct from other disciplines became the subject of discussion and

[4] I can attest personally to this point, having been hired as a part-time programmer by an electronics firm in 1959 while a senior in college with no knowledge whatever of computers. My training consisted of being handed a copy of the operating manual for the computer and directed to Daniel McCracken's *Digital Computer Programming* (New York: Wiley, 1957), one of the first general texts on the subject.

debate. In a talk at a conference in 1967 on “Academic and Related Research in Computing Science”, John R. Pierce of Bell Telephone Laboratories professed a skeptical view:

> I don’t really understand the title, Computer Science. I guess I don’t understand science very well; I’m an engineer. . .. Computers are worth thinking about and talking about and doing about only because they are useful devices, which do something for somebody. If you are just interested in contemplating the abstract, I would strongly recommend the belly button, which would survive any war that man survives.[5]

Pierce’s colleague at Bell Labs, Richard Hamming, suggested in his Turing Award lecture of that year why something more than the abstract might be at stake, especially for academic researchers dependent on the largesse of the government:

> In the face of this difficulty [of defining “computer science”] many people, including myself at times, feel that we should ignore the discussion and get on with doing it. But as George Forsythe points out so well in a recent article, it does matter what people in Washington D.C. think computer science is. According to him, they tend to feel that it is a part of applied mathematics and therefore turn to the mathematicians for advice in the granting of funds. And it is not greatly different elsewhere; in both industry and the universities you can often still see traces of where computing first started, whether in electrical engineering, physics, mathematics, or even business. Evidently the picture which people have of a subject can significantly affect its subsequent development. Therefore, although we cannot hope to settle the question definitively, we need frequently to examine and to air our views on what our subject is and should become (Hamming, 1987).

The title of Forsythe’s article was “What to do until the computer scientist comes,” implying that such a person did not yet exist (Forsythe, 1968). That is, on campuses and in the research community, computer science had not yet achieved the authority to set its agenda that is the hallmark of an autonomous discipline. Peter Wegner, an astute observer of trends in the nascent field at the time, discerned “three computer cultures” working to define themselves (Wegner, 1970). “Computer technologists” interested in computers themselves, or hardware, sought to distinguish their knowledge and practices from the larger discipline of electrical and electronics engineering (Jesiek, 2006). The “computer mathematicians,” concerned with the theory of computation, aimed at placing their subject on its own foundation within mathematics, which tended to treat computing as a branch of applied mathematics and hence of little theoretical interest or importance. The third culture, the “computer scientists,” interested in systems software and tools for programming, faced the task of establishing a discipline independent of either engineering or mathematics.

As it turned out, computer mathematics was already assuming coherent form and laying the theoretical foundations for computer science. During the late 1950s

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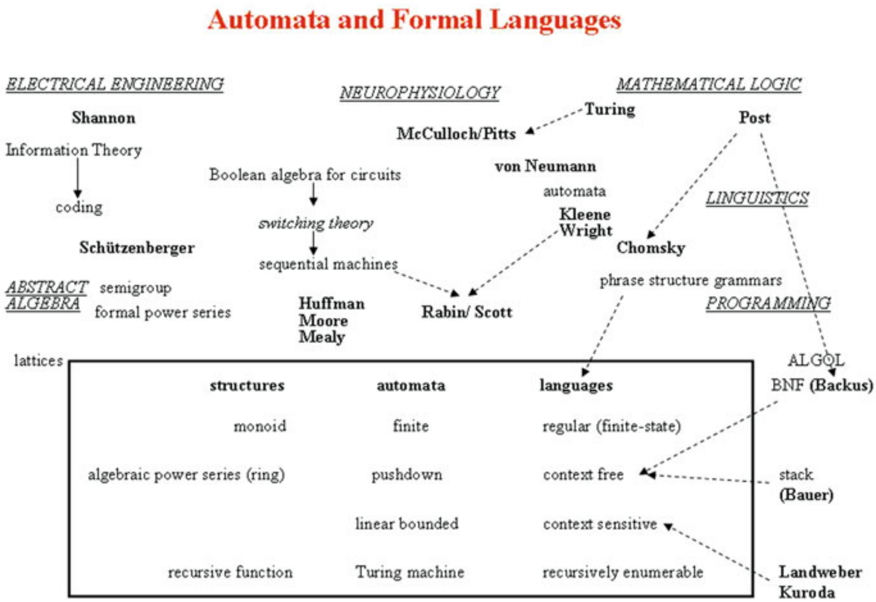
[5] Keynote Address, Conference on Academic and Related Research Programs in Computing Science, 5–8 June 1967; publ. in *University Education in Computing Science*, ed. Aaron Finerman (New York, 1968), 7. Renowned for his work in information theory, Pierce at the time was Executive Director of Research, Communications Sciences Division, Bell Telephone Laboratories. Ironically, as will become clear below, the seminal textbooks in the theory of computation would emerge over the next decade from the Computing Research division of the same organization.

and early 1960s initially independent investigations in electrical engineering, neurophysiology, cybernetics, linguistics, logic, and mathematics in both academia and industry began to converge on a common agenda that formed the theoretical core of what became computer science. Over a short period of time, the analysis and synthesis of switching circuits, the study of classes of finite automata and the patterns of symbols recognized by them, the phrase-structure grammars of Noam Chomsky's new mathematical linguistics, and work on mechanical theorem-proving assumed a shared mathematical form. By late 1960s, researchers in the new subjects of automata and formal languages, formal semantics, and computational complexity were pursuing their own research agenda, recognized in 1970 by *Mathematical Reviews* as a distinct branch of mathematics, based on modern abstract algebra and bringing to that field its demand for constructive solutions and the challenges of the finite but intractably large. The abstract symbolism might give way at times to descriptions of machines but behind the latter stood the deep structures of current algebraic theory (Mahoney, 2007). The following diagram provides an overview of the process for the field of automata and formal languages.



Much of this work proceeded hand-in-hand with the development of new programming languages and systems, increasingly conceptualized in terms of virtual machines, which served then as specifications for the physical devices. Instead of simply providing instructions to a machine, high-level programming languages became means of talking about algorithms, data structures, and computational processes. Drawing on the new theory of computation, compilers for these languages (in many cases written in them) verified the syntax and semantics of the program as they generated efficient code. In 1969, John Hopcroft and Jeffrey Ullman began their text, *Formal Languages and Their Relation to Automata*, by highlighting Chomsky's mathematical grammars, the context-free definition of ALGOL, syntax-directed compilation, and the concept of the compiler-compiler.

> Since then a considerable flurry of activity has taken place, the results of which have related formal languages and automata theory to such an extent that it is impossible to treat the areas separately. By now, no serious study of computer science would be complete without a knowledge of the techniques and results from language and automata theory.

The book was the first of several volumes by Hopcroft and Ullman, soon joined by Alfred Aho, that over the course of the 1970s essentially defined computer science as an academic subject: Aho's and Ullman's 1995 text, *The Foundations of Computer Science*, which reached its sixth printing in 2000, might be said to encapsulate it today.

The careers of these authors reflect a characteristic pattern of interaction between academic science and industrial application in computing. John Hopcroft came from Stanford to Princeton to teach automata theory in its earliest days. Aho and Ullman were among his students. They went on from Princeton to join the technical staff at Bell Labs, to which Hopcroft served as consultant. Ullman returned to Princeton as professor before completing the circle at Stanford. While continuing at the Labs until the early 1990s, Aho also served as chair of computer science at Steven Institute of Technology; he later went on to Columbia. During the 1970s, these men trained a cohort of students at Princeton, who then joined them at the Labs and played central roles in the development of Unix. From the mid-1960s through the mid-1980s there was continuing traffic back and forth between Princeton and Bell Telephone Laboratories, even without any formal relationship between the two institutions.[6] Princeton was no exception. From the outset, work in computer science blurred the boundaries between academic and industrial research. The ACM's highest honor, the Turing Award, first awarded in 1966, has emphasized theoretical work. A significant number of the winners have come from industry, and most of the academics among them have had some industrial experience.

That said, one must differentiate among sectors of the industry. As suggested above, the development of electronic data processing (EDP) took place almost entirely outside the academic world and for the most part independently of the theoretical work just described. It involved the creation, maintenance, and expansion of systems for storing and manipulating large bodies of data for business, industry, and government. Here IBM took the lead, providing the systems along with the computers leased to customers. Large competitors such as Remington-Rand followed suit, while smaller companies relied on independent software contractors to supply customers' needs. Customers themselves, in particular large organizations, established their own data-processing departments, which assumed responsibility for writing

[6] Indeed, in 1969 the Princeton faculty voted down a proposal for a formal cooperative program with Bell Labs. Among the most famous commuters at the time was John Tukey (inventor of the Fast Fourier Transform and creator of the terms "bit" and "software"), who was both a professor of mathematics at Princeton and a director of research at Bell Labs.

programs and maintaining systems. Except for users' groups such as SHARE, both the systems and the application software remained largely proprietary, reflecting the particular architecture of machines on the one hand and the data processing needs of particular companies on the other (Akera, 2001). The one major effort at standardization, the data-processing language COBOL (COmmon Business Oriented Language), quickly became a marriage between Remington-Rand's FLOW-MATIC and IBM's Commercial Translator, brokered by a Department of Defense eager to achieve some uniformity across its many platforms. Only toward the late 1970s did practitioners in the field begin to think about it in theoretical terms.

The four major languages developed by 1960 (and still in use in some form today) reflected the quite different goals and interests of their user communities. COBOL aimed at allowing programmers to order and manipulate large bodies of alphanumerical data in a language close to English; its purpose was to get the job done, not to reflect on it, and to make programs readable by managers. FORTRAN (FORmula TRANslator) was developed for numerical calculation and became the language of choice of scientists and engineers. LISP (LISt Processor) arose from research on symbolic mathematical computation and became the basis for a mathematical theory of computation, serving as both a tool and a touchstone for the theoretical developments of the 1960s. Algol (Algorithmic Language), designed by a European-American committee of computer scientists, retained the focus on numerical computation of FORTRAN, which it aimed to replace, while incorporating some of the theoretical features of LISP. Occupying a middle ground between theory and practice, it became the prototype language for systems programming and thereby for the emerging discipline of computer science. While Algol included the full capabilities of FORTRAN and could be extended to include those of LISP, it made no gesture toward COBOL and its domain of data processing.[7]

In both the United States and Europe, it was the overlapping LISP and Algol communities who created the discipline of computer science and designed a curriculum to go with it.[8] Although the Europeans among them might refer to their subject as "informatics", they shared with their American colleagues an agenda addressed to the analysis of algorithms and data structures and to the design of programming languages and operating systems with which to set mathematically verifiable specifications for computers and to write mathematically demonstrable programs.[9] They had little to say about data processing, except to deprecate the

[7] IBM's effort to bridge FORTRAN, COBOL, and Algol in PL/1 proved overwrought and short-lived.

[8] While Algol itself never caught on in the United States, it set the pattern for such languages as Pascal, C, and even C++, a combination of C and Simula, the latter an extension and modification of Algol to accommodate what is now called object-oriented programming.

[9] As Per Brinch-Hansen later recalled, it was in an old cosy villa that I defined the instruction set of the RC 4000 computer. It became a nice, uninspired copy of the IBM 360. However, one thing set the RC 4000 apart from other computers: its function was concisely defined in the programming language Algol 60 before it was built. It was no doubt the only computer in the world that made it possible for the user to predict the result, bit by bit, of dividing two non -normalized floating-point numbers. See "The Programmer as a Young Dog", in his *The Search for Simplicity: Essays in Parallel Programming* (Los Alamitos, CA: IEEE Computer Society Press, 1996), Chapter 10.

theoretical and esthetic failings of COBOL. Thus the main sector of the computing industry remained beyond the purview of computer science, despite the contributions of individual members of the industry to the field. Only IBM spanned the two communities, contributing to computer science through its research laboratories while keeping its focus on the business of large-scale data processing.[10] From the late 1970s, the spread of minicomputers and the growing preference for Unix both as an operating system and as a teaching environment only widened the gap between computer science and commercial data processing.

How Software Engineering Did Not Become an Engineering Discipline

The sense of separation between the two communities emerged with some force at the second of two conferences sponsored by the NATO Science Committee in 1968 and 1969 to consider the possibility of a discipline of software engineering which would "[base] software manufacture... on the types of theoretical foundations and practical disciplines that are traditional in the established branches of engineering".[11] The effort was prompted by a growing and (apparently) widespread sense of a "crisis" in the production of large-scale software systems: project after project found itself behind schedule, over budget, and unable to meet specifications. Participants at the first gathering in Garmisch in October 1968, evenly divided between academic and corporate affiliations and between Americans and Europeans, seemed to reach consensus about the seriousness of the problem and the need for some sort of systematic response.[12] However, in Rome a year later it became painfully evident among a similarly composed group of participants that computer scientists ("theory") and corporate software developers and managers

[10]For example, the work of IBM's Vienna Laboratory on the formal semantics of PL/1 placed it in the forefront of that highly mathematic al subject. Particularly revealing of the tension between the two commitments was IBM 's initially tepid response to E.F. Codd's path-breaking work on the relational data model, carried out around the same time at the San Jose Laboratory. On the one hand Codd's approach promised to put data processing on a mathematical footing similar to that of programming languages, in particular freeing the design, maintenance, and use of databases from any concern with the architecture of particular machines and hence making database systems in principle portable from one machine to another. On the other hand, relational database systems threatened the obsolescence of IBM's established database systems, which were tied to IBM equipment and formed a major source of in come for the company.

[11]Software Engineering: Report on a Conference sponsored by the NATO Science Committee, Garmisch, Germany, 7–11 October 1968, P. Naur and B. Randell, eds., Scientific Affairs Division, NATO, 1969, 13. The report was republished, together with the report on the second conference in Rome the following year, in P. Naur, B. Randell, and J.N. Buxton, eds., Software Engineering: Concepts and Techniques. Proceedings of the NATO Conferences, Petrocelli (1976). Randell has made both reports available for download in pdf format at http://homepages.cs.ncl.ac.uk/brian.randell/NATO/.

[12]Indeed, speaking off the record in conversations, corporate representatives reported incidents that deepened the sense of "crisis." As one participant later recalled in an interview, bad software was causing system failures that resulted in injury and death; "people were getting killed!"

("practice") had quite different views about the nature of the problem and the form of a response to it. Christopher Strachey, a computer scientist with a foot in both camps, addressed the issue on the last day of the conference, pointing to the lack of communication between the two sides:

> It seems to me that one of the difficulties about computing science at the moment is that it can't demonstrate any of the things that it has in mind; it can't demonstrate to the software engineering people on a sufficiently large scale that what it is doing is of interest or importance to them.

Taking recursive methods as an example of a theoretically verified approach to programming that had made little headway in the software industry, he asked, "How can we convince people who are dealing with hundreds of programmers and millions of instructions that something as radical as changing the basic core of the way in which they program is a good thing to do?" (Buxton and Randell, 1970).

Essentially, the computer scientists viewed software engineering from the perspective of theoretically informed programming systems extended to encompass, and eventually automate, all phases of the development cycle from analysis of requirements to testing and maintenance. Given a computational model expressed in a high-level programming language, theory-based programming systems gave reasonable assurance that the resulting machine code would work as designed, that is, that the dynamic process would enact the model. Computer science was responsible for building the system right. The challenge to software engineering was building the right system, that is, designing a computational model that adequately and accurately captured the workings of the system of interest. Early studies suggested that the bulk of the errors uncovered in testing originated in the early stages of requirements analysis and specification, in making clear what the software was supposed to do. Much of the effort in software engineering during the 1970s and 1980s was directed toward extending programming languages to the level of design and specification. In one line of thought, structured programming would be preceded by structured analysis and design, carried out in a similarly formal language.

By contrast, the corporate developers thought of software engineering in terms of project management, with an emphasis on organizing people and keeping records. For them, high-level programming languages offered means of maintaining supervisory control over programmers and of structuring the division of labor. The earliest notion of a "software factory" rested on the existence of such languages and the programming systems underlying them, which constituted closed working environments. Denied access to the computer itself by hierarchically structured operating systems, programmers would have no choice but to program within its constraints.

Given the direction of management thinking at the time, the two sides were actually not all that far apart, since both aimed at industrialization of software development (or, as the NATO report referred to it, software manufacture) along the lines followed by machine-based industries earlier in the century. The assembly line became the touchstone, and behind it the thinking of F. W. Taylor and Henry

Ford.[13] Behind it too lay the model of engineering as applied science, in most cases mathematical science, and as a body of domain-independent methods for systems analysis and development.

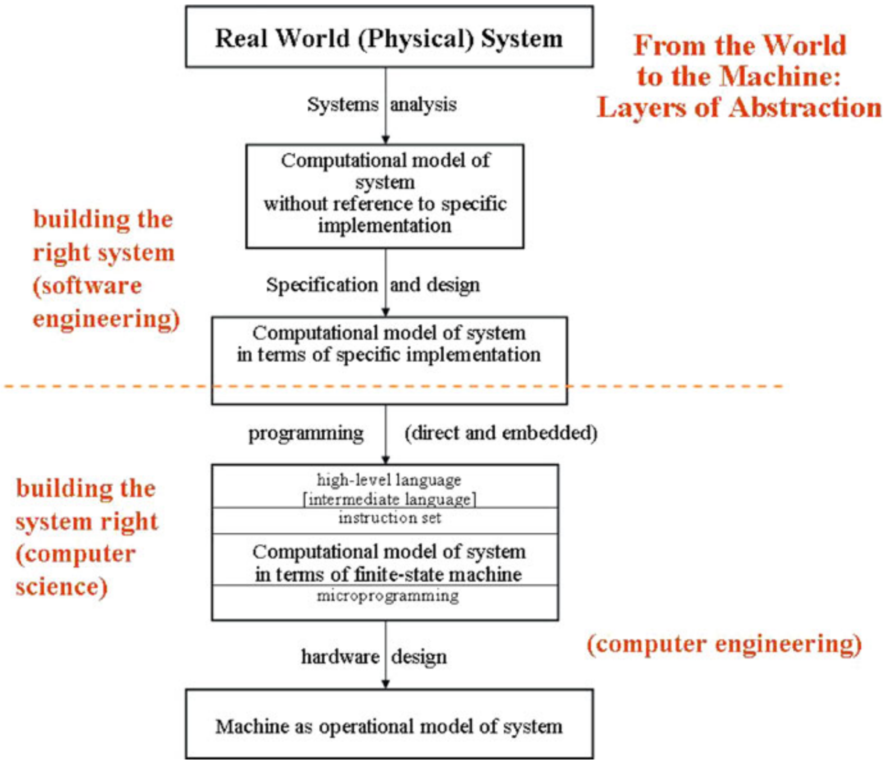
Thus, although the two sides might have disagreed over their relative roles in defining and articulating software engineering, the practitioners at first accepted the theoreticians' notion of what constituted a theory of computing. At issue was just how much of that theory was necessary to get the job done, which translated into the question of what role theory should play in the education of computer science students headed for jobs in the industry, particularly business data processing. What the industry needed was people who could program in a corporate environment as members of large teams, and it wanted the curriculum to reflect and meet that need. What the theoreticians feared was shifts in the curriculum that conveyed the notion that "computer science = programming" with an accompanying whiff of vocational training. That was a complaint lodged against the ACM's Curriculum '78 by two leading figures in the field, who claimed that it took no account of the "major advances in the theory of computation and in the utility of theoretical results in practical settings... [and the] real progress in developing principles and theories for the design and verification of algorithms and programs." (Austing et al., 1979; Ralston and Shaw, 1980) A few years later, an ACM Task Force on the Core of Computer Science charged in 1985 with defining "an intellectual framework for the discipline of computing" pointed to the same problem. In its final report in 1989, the task force admonished that the identification of computing with programming "... denies a coherent approach to making experimental and theoretical computer science integral and harmonious parts of a curriculum," and it laid out a tripartite scheme which emphasized mathematical theory, scientific modeling, and engineering design as the "paradigms" of the discipline (Denning et al., 1989). The physical sciences provided the model for the group, which pointed in particular to the Feynman Lectures in Physics as a "paradigm" (again) of the laboratory-based introductory course they had in mind. While the task force noted that "[m]any computing graduates wind up in business data processing, a domain in which most computing curricula do not seek to develop competence," they made no accommodation for it, observing rather that "whether computing departments or business departments should develop that competence is an old controversy." (*Ibid*)

The question of competence went well beyond business data processing. As Barry Boehm, a leading researcher on the management and economics of large software projects, in particular in real-time control systems, pointed out in 1976:

> Those scientific principles available to support software engineering address problems in an area we shall call *Area 1: detailed design and coding of systems software by experts* in a relatively economics-independent context. Unfortunately, the most pressing software development problems are in an area we shall call *Area 2: requirements analysis, design, test, and maintenance of applications software by technicians* in an *economics-driven* context. (Boehm 1976)

[13] For further discussion of these themes, see my "Finding a History for Software Engineering".

By the mid-1980s it was becoming evident that the applied science model of software engineering was not making much progress in Area 2. Despite advances in programming systems and considerable research on computer-based design environments, the systematic, top-down staging of projects did not match the realities of development in the field. What worked for operating systems and programming tools, the subjects of computer science, did not function as well for the real-world systems of concern to the computer industry and its corporate and government customers. Among other things, top-down structured design assumed that customers knew what they wanted their system to do and how they wanted to work with it, and that developers knew how to model the desired system. But that was not usually the case, either for customers or developers. Requirements, specifications, and design changed as projects gained experience of the emerging system, requiring returns to earlier stages in the development process. More important, determining what to build required knowledge of the domain of application. Software engineering as applied computer science could help to build the system right, but it offered little assistance in building the right system. At the top levels of abstraction, where computing encountered the real world, the problems were not about how computers worked but about how the world worked and how computers could be made to model its workings.



Put another way, computing could not generate within itself the questions that drove its development. The challenges came from outside the discipline, from the domains of application. Already the case for centralized systems, it proved even more so as networking distributed computing over a variety of systems. The context (or, better, contexts) of application set the agenda for computer science.

Reflections in Recent Discussions Among Software Engineers

Since 2001 a group of leading software engineers has been engaged in a series of historical case studies directed toward demonstrating the impact of research on practice in the field. The project arose in response to a widespread view that research was not leading to practical results; rather, the effective techniques and methodologies guiding current software development arose from practical experience. The case studies revealed subtle patterns of interaction between the two, in many cases a form of bootstrapping back and forth (Osterweil et al., 2008). As the project progressed, discussion turned from the case studies to the current state and future direction of the field. As historical consultant to the group, I have listened to the latter discussions with considerable interest, especially as I compare how current practitioners talk about their subject with the discourse at Garmisch and Rome 40 years earlier. Some of the concerns remain the same: large-scale projects still run behind schedule, over budget, and short of specifications. But the main challenges are new, born of the explosive transition from centralized mainframe computing to distributed computing over networks encompassing devices of all sizes and sorts. The systems of concern earlier have not only grown more complex in themselves but now function as interactive parts of larger, heterogeneous systems, indeed systems of systems, which constitute the infrastructure of business, industry, and government and form a pervasive presence in modern life. These systems add network effects to the complexity of the finite but intractably large spaces generated by discrete combinatorics. Software engineering faces a new array of questions: how to model complex, nonlinear, interactive systems; how to abstract from one level of a system to a higher one; how to manage complexity; how to scale.

With these new challenges has come a shift in discourse from the mechanical to the biological, from the formal to the experimental, and from the structural to the social (or sociological). Practitioners speak of “self-repair and maintenance,” of “self-organization and adaptation,” of “growing (or evolving) software, rather than coding it,” of “context and environment,” and of “emergent” (i.e., irreducible) system behavior. Here, to refer again to the title of my paper at Paderborn, software as science meets science as software. For what warrants the use of these biological terms for software development is the computational modeling of living systems, originally inspired by von Neumann’s cybernetic notion of automata as “artificial organisms,” capable in principle of homeostasis, self-replication, and even evolution (von Neumann, 1951). Research on cellular automata (or what von Neumann called “growing automata”) and genetic algorithms has subsumed natural and artificial organisms under the common heading of complex adaptive systems, which in the research agenda, for example, of Stephanie Forrest, Chair of Computer Science at the University of New Mexico and Research Professor at the Santa Fe Institute, includes: genetic algorithms, computational immunology, biological modeling, and computer security.[14] (Try to distinguish science from the context of application here.)

The biological approach to computing has its drawbacks when it comes to theory. At first it seemed that the computational theory of finite automata would extend to cellular automata in similarly fundamental ways. Stephen Wolfram proposed in 1984 that "Computation and formal language theory may in general be expected to play a role in the theory of non-equilibrium and self-organizing systems analogous to the role of information theory in conventional statistical mechanics."[15] Indeed, formal language theory provided some of the earliest models of plant growth (Lindenmayer systems). However, other systems accessible only through computational modeling pose a theoretical challenge brought out by Christopher Langton, a founder of Artificial Life:

> We need to separate the notion of a formal specification of a machine – that is, a specification of the logical structure of the machine – from the notion of a formal specification of a machine's behavior – that is, a specification of the sequence of transitions that the machine will undergo. In general, we cannot derive behaviours from structure, nor can we derive structure from behaviours. (Langton, 1996; Smith, 1996)

In the concluding chapter of *Hidden Order: How Adaptation Builds Complexity*, Holland makes clear what is lost thereby. Looking "Toward Theory" and "the general principles that will deepen our understanding of all complex adaptive systems [cas]," he insists as a point of departure that:

> Mathematics is our sine qua non on this part of the journey. Fortunately, we need not delve into the details to describe the form of the mathematics and what it can contribute; the details will probably change anyhow, as we close in on our destination. Mathematics has a critical role because it along enables us to formulate *rigorous* generalizations, or principles. Neither physical experiments nor computer- based experiments, on their own, can provide such generalizations. Physical experiments usually are limited to supplying input and constraints for rigorous models, because the experiments themselves are rarely described in a language that permits deductive exploration. Computer-based experiments have rigorous descriptions, but they deal only in specifics. A well-designed mathematical model, on the other hand, generalizes the particulars revealed by physical experiments, computer-based models, and interdisciplinary comparisons. Furthermore, the tools of mathematics provide rigorous derivations and predictions applicable to all *cas*. Only mathematics can take us the full distance. (Holland, 1995)

[14] Stephanie Forrest homepage, http://www.cs.unm.edu/~forrest/, accessed 15 July 2008.

[15] Stephen Wolfram, "Computation Theory of Cellular Automata", *Communications in Mathematical Physics* 96(1984), 15–57; at 16. Repr. in his *Theory and Applications of Cellular Automata* (Singapore: World Scientific, 1986), 189–231 and his *Cellular Automata and Complexity: Collected Papers* (Reading: Addison Wesley, 1994), 159–202. In *A New Kind of Science* (Wolfram Media, 2002), Wolfram maintains that cellular automata exhibit a Principle of Computation Equivalence that "applies to essentially any process of any kind, either natural or artificial" (175).

In the absence of mathematical structures that allow abstraction and generalization, computational models do not say much. Nor do they function as models traditionally have done in providing an understanding of nature on the basis of which we can test our knowledge by making things happen in the world.

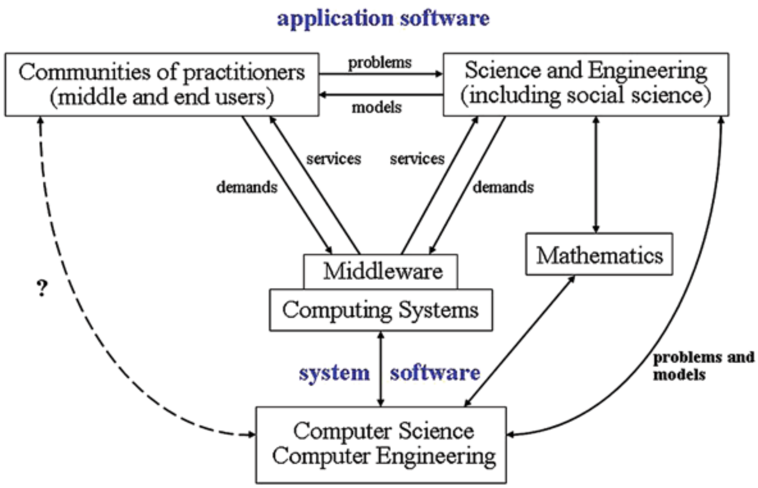
The same holds for an understanding of software and its development. One of the continuing problems of software engineering has been its inability in general to derive generic benefit from specific solutions and to transfer successful results from one context to another, seemingly similar one (Mahoney, 2004). Unable to reduce complex systems to an analytical mathematical structure and thus to deduce their behavior, researchers work with models of the systems in action, adjusting the models to trace resulting changes in behavior. Here "rapid prototyping" and "agile programming" have been taking the place of systems analysis and formal specification. Since the early 1990s, empirical software engineering has emerged as a distinct field, establishing its own journal in 1996. It is based on the premiss that the software development process, especially for large-scale projects, involves organizational and human factors for which no formal models exist and which therefore must be studied in situ, using the techniques of the social as well as the natural sciences. These developments within the mainstream seem to bring it on a convergent course with what until recently has been an alternative movement based on a socially oriented view of computing and aimed at a correspondingly broader concept of *Informatik*.

What Might a Science of Software Look Like? Would It Be a *Theorie der Informatik*?

So far, despite advances in the field, software engineering has failed to meet its initial charge to "[base] software manufacture... on the types of theoretical foundations and practical disciplines that are traditional in the established branches of engineering" and thus to establish itself as an engineering discipline. In particular, the community of practitioners has not yet converged on a shared agenda. They have not come to agreement on what the central problems are, what would constitute solutions of them, and how their solution would move the agenda forward. There are many schools of thought, many measures of progress, many measures of achievement. This failure puts computing, as the defining technology of our age, in a peculiar position with regard to the question of the relation of science and its applications. On the one hand, there now exists a well established, mathematically grounded science of computation that extends beyond computers to computational processes in general. It is the basis for thinking about the world computationally, which the sciences are increasingly doing. On the other hand, that science provides little guidance in the task of writing programs, that is, in the design and implementation of software of any significant complexity, nor is it really a prerequisite for doing so. An anecdote related in a recent study of a software project gone awry makes the point:

> Like just about every other programmer at OSAF, [Andi Vajda] had started playing with computers in high school. By the time he had graduated, he had hacked open the operating system for the school's minicomputer. "Basically," he later told me, "everything I've learned about operating systems, multitasking, memory usage, hard drives, file system layout, all of these things go back to that. When I learned it again in college, it was old news. I thought, 'Yeah, I've seen this before.'"[16]

Thus, programming remains a craft skill, and neither the computer scientists nor the software engineers have effective control over the technology. Their tools are accessible to its users, who can use them to achieve significant results without formal training. Quite complex systems have been built by people with little or no theoretical training or background. The line between amateur and professional is drawn differently in computing than in other areas of high technology.



So too is the line between academia and industry, or indeed academia, industry, and the user community. Industry has always supported theoretical research, believing that its future depends on staying at the cutting edge. Microsoft and Google have followed IBM's lead in establishing their own research units. As noted above, industry has had its share of winners of the ACM's prestigious Turing Award, recently made all the more prestigious and lucrative by industrial support, and academic computer scientists have no trouble finding research funding and work as consultants.[17] Many move back and forth between the academic and industrial workplace. But industry has a primary stake in short-term results and its own methods and tools for implementing them. The most advanced products remain proprietary knowledge,

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[16]Rosenberg, *Dreaming in Code*, 110. OSAF is the Open Software Applications Foundation (http://www.osafoundation.org), engaged in building Chandler, a complex personal information management system conceived by Mitchell Kapor, creator of Lotus 1-2-3. Rosenberg describes the vision and vicissitudes of the project, summarized in the full title of the book (see above, Note 1).

[17]Intel recently raised the value of the award from $100,000 to $250,000.

increasingly protected by patent, copyright, and trade secret. What industrial programmers know is not the same as what computer scientists or software engineers know, whence the continuing complaint from the software industry that academic computer science is not teaching students what they need to know on the job. Beyond industry and academia lies a vast body of individual practitioners and cooperative groups, creating imaginative and powerful software in a free and unregulated market, mediated by the World Wide Web. Software engineering exercises no devolved public authority to monitor the quality and safety of the system or its products, even as these pose some of the most basic and interesting problems for both software engineering and computer science.

While the configuration of the world of software and current trends in software engineering suggests that a foundational theory would involve more than the mathematical science of computation, efforts to create a suitably encompassing *Theorie der Informatik* have also failed so far to define a shared agenda that might lead to that goal.[18] It is not a discipline but rather a confluence of concerns, a common focus of a variety of perspectives on computing as a technical, socio-political, and philosophical phenomenon. The questions raised in Paderborn remain open. Does software have a science other than computer science? If so, what is that science or, if more than one, what are the sciences and how are they related? Does software need a science? Does *Informatik* need a *Theorie?*

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[18]For recent developments, see the websites "Theorien der Informatik" (http://waste.informatik.hu-berlin.de/~bittner/tdi/tdi.html) and "Ausarbeitung: Grundlagen einer Theorie der Informatik" (http://ddi.cs.uni-potsdam.de /Lehre/DidaktikSeminarKerninformatik/Papers/fuchs), accessed 16 July 2008. I thank Reinhard Keil for pointing these out to me.

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Black-Boxing Organisms, Exploiting the Unpredictable: Control Paradigms in Human–Machine Translations

Jutta Weber

> *... a kind of simultaneous safety with risk, a transcendence over the 'world' in question at the same time that one is somehow inscribed within it, engaged with an autonomous and therefore not fully predictable other. This produces a simultaneous sense of control over the virtual from 'outside' while being 'inside,' controlled by larger and more powerful forces. The result is a controlled simulation of the experience of not being in control; hence, the best of both worlds.*
>
> (Lucy Suchman, 2006, 6).

Introduction

Cybernetics as well as new, behavior-based robotics implicitly or explicitly claims to reach beyond the old linear and mechanical logic of modern science and to develop a new and more complex technoscientific rationality.[1] This shift is celebrated as paradigmatic by technoscientists as well as social scientists and humanities scholars. For some scholars, new technosciences[2] such as robotics and "cybernetics directly thematises the unpredictable liveliness of the world and processes of open-ended becoming" (Pickering, 2002, 430). With this supposed shift in (techno)scientific rationality new approaches and methodologies of technoscientific research and design[3] but also theoretical work in the social sciences and humanities is supposed to become possible.

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[1] This paper draws on my German paper "Vom 'Teufel der Unordnung' zum Engel des Rauschens. Kontroll- und Rationalitätsformen in Mensch-Maschine-Systemen." In: Blätter für Technikgeschichte Heft 66/67, 2004/05.

[2] For the concept of technoscience see Weber (2003, 2006, 2010) and Nordmann (2004, 2006).

[3] For example Deleuze and Guattari (1983), Pickering (2002), Law and Urry (2003).

M. Carrier, A. Nordmann (eds.), *Science in the Context of Application*, Boston Studies in the Philosophy of Science 274, DOI 10.1007/978-90-481-9051-5_24,

Being curious as well as sceptical about this claim of a more complex and inclusive technoscientific rationality, I will analyse *the epistemological and ontological*[4] *foundations of cybernetics and new robotics with regard to the move towards more effective but not necessarily more complex models of human–machine communication*.

My interest in the epistemological and ontological moves and the reconfiguration of the order of knowledge is partly motivated by my suspicion that the celebrated biologically-inspired versions of human–machine relations in new robotics are following reductionist strategies of problem-solving and politics of translation already known from systems theory and cybernetics:

In the 1930s and 1940s, systems theory and cybernetics developed new epistemological strategies and ontological foundations which made it possible to (dis)solve or at least circumvent the old dispute on vitalism and mechanism (in biology), holism and reductionism (e.g., between the German "Lebensphilosophie"[5] and the natural sciences). Thereby a new science of command and control came into being. Historian of science Maria Osietzki has shown how the strong interest in the living and the dissolution of the dichotomy of vitalism and mechanism[6] led to a departure from the old mechanic-thermodynamic model of thought with its unsolved epistemological problems, thereby establishing a new order of knowledge that integrated the living with its capacity for self-preservation. Relying on this new model, a much more efficient translation between organisms and machines became possible which interpreted both as "parts of a higher organization" (Osietzki, 2003, 147; translation J.W.).

In my view, a quite similar translation took place *from Good Old-Fashioned Artificial Intelligence (GOFAI) towards New (Embodied, Embedded, Behavior-Based) Robotics* which relies on interdisciplinary knowledge transfer, the use of effective analogies – especially from biology,[7] but also from philosophy, psychology or cognitive science. *My contention is that the recent transformation of the technoscientific rationality in new robotics leads to an integration and reconfiguration of central epistemological and ontological problems prevalent in cybernetics and systems theory – which are closely related to issues of unpredictability, noise, and spontaneity.*

[4]In the following I use the term ontology to signify the meta-theoretical core of a theory which contains syntactical structures, ontological options and central semantics. Ontological options lay down what set of things, entities, events or systems (including their ascribed properties) are regarded as existing; see Ritsert (2003), Weber (2005). The term ontology here is *not* used in the metaphysical sense of a categorical structure of reality.

[5]Osietzki (2003); Schürmann (2003).

[6]On the controversy about vitalism and mechanism in biology see Keller (1995); Penzlin (2000).

[7]The recent interest of roboticists in biology is not primarily motivated by epistemological discussions (e.g., on vitalism versus mechanism) but by the contemporary encompassing scientific and economic success of the life sciences.

I suggest that cybernetics and systems theory were part of the shift from the classical sciences towards the technosciences,[8] of the configuration of a new technoscientific rationality. The shift from the technoscientific rationality of cybernetics to robotics can be interpreted as the shift from a more static *biocybernetic rationality* towards a more flexible one (robotics). Nevertheless, this new paradigm with its greater flexibility is still committed to traditional conceptions of *technological efficiency and control*. It does not aim or achieve a more comprehensive theoretical understanding or greater representational adequacy – to the contrary. It abandons the value of representation and black boxes traditional epistemic questions and concepts.

In the following I want to work out ontological and epistemological foundations of cybernetics and GOFAI and their transformation by behavior-based robotics. Thereby I will focus on the reconfiguration (and intensification) of human–machine translation, the idea of a new interdisciplinary (meta)science which transforms the mechanical and linear thought of traditional science and the black-boxing of traditional questions and concepts through the shift in epistemological and ontological assumptions.

By analyzing the new ontologies and epistemologies of cybernetics and behavior-based robotics, I want to contribute to the understanding of the emergence of recent technosciences (Haraway, 1991/1985; Latour, 1987; Nordmann, 2004; Weber, 2003), at the same time differentiating between a static and a dynamic version of biocybernetic rationality.

> *So we don't know if the inside of the box, the black box is correct but at least the outputs are very much correct. So it gives some hope that we're not too far away from the real . . .*
> *(from an expert interview with a roboticist)*

System, Black Box, Information & Code: New Ontologies and Processes of Translation

The cybernetic dream of a universal and interdisciplinary science was motivated by the search for new tools and approaches as well as the desire to reorder the modern sciences. The rhetorics of universality provided cybernetics not only with a powerful strategy to support its supremacy in the envisioned new order of disciplines but also with a "new set of funding possibilities" (Bowker, 1993, 123). Cybernetics was supposed to be a "cutting-edge science, which was proving itself in all spheres (physical, social, chemical, political, microbiological . . .) and proving the analytic conflation of those spheres." (ibid.) Cybernetics claimed to develop a science working with innovative epistemologies, methodologies and taxonomies that could better grasp the complex relations between diverse fields of knowledge.

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[8]On the concept of technoscience see Nordmann (2004, 2006, and the last chapter of the present volume); Weber (2003).

It was supposed to be a science capable of handling interdisciplinary problems in our complex postmodern world that is characterized by the blurring of diverse ontic realms, the intense interweaving of science, technology, industry and politics as well as the accelerated production of sociotechnical systems, hybrid objects of knowledge and artefacts. Listen to Norbert Wiener's description of the needs and challenges of modern life in the 1950s: "The needs and the complexity of modern life make greater demands on this process of information than ever before, and our press, our museums, our scientific laboratories, our universities, our libraries and textbooks, have been developed to meet the needs of this process. *To live effectively is to live with adequate information*." (Wiener, 1950, 124; my emphasis)

From the 1940s to the 1970s, the universal, interdisciplinary and at the same time multi-layered approach of cybernetics with its many application fields was quite successful in scientific as well as funding terms. Nevertheless, it might have been the lack of homogeneity which led in the long run to a decline of cybernetics as an autonomous field of research and knowledge: "In spite of its important historical role, cybernetics has not really become established as an autonomous discipline. Its practitioners are relatively few, and not very well organized. There are few research departments devoted to the domain, and even fewer academic programs. There are many reasons for this, including the [. . .] *difficulty of maintaining the coherence of a broad, interdisciplinary field in the wake of the rapid growth of its more specialized and application oriented 'spin-off' disciplines, such as computer science, artificial intelligence, neural networks, and control engineering,. . ."* (Heylighen and Joslyn, 2001, 4; my emphasis)

The ability to conduct interdisciplinary knowledge transfer, to find effective analogies covering a vast array of meanings and to build bridges between diverse ontic realms were important means for a future universal science that wanted to overcome the differentiation of the sciences. But it seems that exactly this broad approach was the reason for its decline.

But in the beginning, one of the main reasons for the success of cybernetics was exactly its abilities in translation, to find convincing analogies and connections between diverse realms. One of the central ontological groundings is cybernetics' belief "that machines and organisms were behaviourally and in information terms 'the same'" (Bowker, 1993, 110). This was quite an effective way for a tighter coupling of humans and machines than ever before. The universal language of systems theory with its principles of open systems, the concepts of information and communication as well as the new cybernetic epistemology and ontology in general made a comprehensive and universal theory of organization and communication relations in teleological and functional systems possible – applicable on organisms as well as machines.[9]

The literary theorist and science studies scholar Katherine Hayles points towards the central function of analogy in developing these new approaches in cybernetics: "Analogy is not merely an ornament of language but is a powerful conceptual mode

[9] see Haraway (1991/1985); Keller (1995).

that constitutes meaning through relation" (Hayles, 1999, 91). With the help of analogy and new epistemological and ontological foundations, cybernetics is capable of radically questioning the borders between human beings, animals and machines. While *any questions concerning the intrinsic properties of organisms and systems were disregarded,* it became an important part of cybernetic ontology to study the *behavior* of biological and artificial systems as well as the coupling of system and environment.

The interest in the *behavior* of a system is not at least driven by cyberneticians' involvement in military research. For example, during World War II Norbert Wiener tried to develop an anti-aircraft predictor (but never succeeded). He was mainly interested in the prediction of the *behavior* of the enemy's aircraft. To conceptualize the pilot of the bomber and his machine as one entity – a system – made the calculation much easier and the neglect of intrinsic properties necessary.[10] Cybernetics became a tool for the construction of (anti-)systems with analogical behavior (and not only a theory of anything). Fusing humans and machines conceptually means to ascribe at least in principle the possibility of analogical behaviors in humans and machines. As a result, not only the machine, but also *human beings and animals were black-boxed,* de-essentialised and de-naturalized. Philosopher Donna Haraway characterizes this development in the following way: "Any objects or persons can be reasonably thought of in terms of disassembly and reassembly; no 'natural' architectures constrain system design. . . . Human beings, like any other component or subsystem, must be localized in a system architecture whose basic modes of operation are probabilistic, statistical. No objects, spaces, or bodies are sacred in themselves; any component can be interfaced with any other if the proper standard, the proper code, can be constructed for processing signals in a common language." (Haraway, 1991, 162p.)

The systems analogy which couples human beings as machines via black-boxing are crucial tools to intensify the translation of humans into machines and vice versa. The former so-called intrinsic properties of the entities in question are made invisible by these tools.

While "(e)nergy and matter were the scientific darlings of the nineteenth century." (Wiener, 1950, 128), in the first half of the twentieth century cybernetics shifted the focus of science towards information. In the 1930s the biologist Bertalanffy developed a general systems[11] theory in which all living organisms were thought of as systems based on homeostatic balance. According to that all organisms were able to maintain steady states as well as their structure and identity in the interaction with their environment and to regenerate and reproduce themselves.[12] This systems logic was not only ascribed to single organisms but to systems in general whether they are biological, economic, or social systems.[13]

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[10] See also Galison (1994).

[11] See Bertalanffy, von (1927, 1940); Penzlin (2000).

[12] see Gloy (1995, 244).

[13] see Leps (2000, 614).

This idea propels the idea of organic and non-organic entities, of the material and non-material as equally compatible with processes of communication and control. This tendency intensified in the 1950s, when cybernetics more and more used theories and concepts from molecular biology (and vice versa): In his book "The Human Use of Human Beings" Norbert Wiener claims that the physical identity of an organism is not determined by its materiality, but by its form or organization. The latter stabilizes the organism's identity in its ongoing transformation processes. This ontological claim helps to smooth the communication and translation processes between organic and non-organic entities as Wiener believes that in principle there is no difference between the transport of matter or messages. He states that it is (theoretically) possible to send a human being over a telegraph line, even if it is now (and may be forever) impracticable: "To recapitulate: the individuality of the body is that of a flame rather than that of a stone, is that of a form rather than that of a bit of substance. This form can be transmitted or be modified and duplicated, although at present we only know how to duplicate it over a short distance. When one cell divides into two, or when one of the genes which carries our corporeal and mental birthright is split in order to make ready for a reduction division of a germ cell, we have a separation in matter which is conditioned by the power of a pattern of living tissue to duplicate itself. Since this is so, *there is no fundamental absolute line between the types of transmission which we can use for sending a telegram from country to country and the types of transmission which at least are theoretically possible for a living organism such as a human being*." (Wiener, 1950, 109; my emphasis)

In the (bio)cybernetic paradigm, the most important property of organisms are (self)-organization as well as information processing, transformation and transportation. With the rise of the life sciences and especially molecular biology, there is a growing tendency to interpret the organism as a biotic component in a (cybernetic) network. The borders between the physical and the non-physical are getting more pervasive and the organism is understood as a communication system controlled by the genetic code. These ontological foundations are the basis for the new intimate coupling of man and machine embedded in a "movement from an organic, industrial society to a polymorphous, information system" (Haraway, 1991, 161) which is populated by new hybrid, technoscientific objects of knowledge[14] which are redefined as toolboxes consisting of organic or technical respectively biotic components that can be assembled, dis- and re-assembled in a way that is specific for this new techno-rationality.

There is no need to integrate the human being into the machine, if the machine is already part of the human being.
Volker Grassmuck, 1988, 52 (translation J.W.)

[14] see also Latour (1995/1991).

Holistic Approaches, The Promises of Analogy and Transdisciplinarity

The cybernetic coupling of man and machine is made possible via the "scientific darlings" of self-organization, information and communication as well as the universal systems approach. Another important mean is the development of an interdisciplinary approach of cybernetics, paradigmatically translated into action by the Macy Conferences[15] in the 1950s, which aims at a non-reductionist and more holistic technoscientific rationality which overcomes the old logic of modern science and is capable of handling the questions of a complex postmodern world. Science studies scholar Andrew Pickering describes this new epistemological approach of cybernetics in the following way: "... there is something philosophically or theoretically pregnant about cybernetics. There is a kind of seductive mystery or glamour that attaches to it. And the origin of this, I think, is that cybernetics is an instantiation of a different paradigm from the one in which most of us grew up – the reductive, linear, Newtonian, paradigm that still characterizes most academic work in the natural and social sciences (and engineering and humanities, too) – 'the classical sciences' as Ilya Prigogine and Isabelle Stengers (1984) call them" (Pickering, 2002, 413f). This new technoscience seems to leave science's representational view from nowhere behind. According to Pickering, the decisive difference between the new (biocybernetic) and classical scientific way of thought lies in its engagement with the real world, in its performativity, and its focus on emergence, the unknown and unpredictable: "cybernetics [...] is all about this shift from epistemology to ontology, from representation to *performativity, agency and emergence*, ..." (Pickering, 2002, 414; my emphasis) The promise and relevance of cybernetics as well as new AI/robotics is seen in its attention towards the liveliness of the world, its openness and its unpredictable behavior.

But why do some believe that this new science is engaged in a particularly profound and illuminating way with the liveliness of the world? Andy Pickering dichotomises representation and performativity by pointing toward a central difference between cybernetics and traditional AI. In his view, cybernetics rests on an intimate coupling of system and environment. With its idea of "autonomy" it gives its artefacts a certain "elbowroom". Heylighen und Joslyn identify this tendency as the cybernetic claim of an (as if) free will of every actor, which is oscillating between intentionality and adaptation[16]: "Perhaps the most fundamental contribution of cybernetics is its explanation of purposiveness, or goal-directed behaviour, an essential characteristic of mind and life, in terms of control and information. Negative feedback control loops which try to achieve and maintain goal states were seen as basic models for the autonomy characteristic of organisms: their behavior, while purposeful, is not strictly determined by either environmental influences or

[15] see Hayles (1999).

[16] There are interesting analogies between cybernetic epistemology and ANT concerning the agency of entities resp. agents.

internal dynamical processes. They are in some sense 'independent actors' with a 'free will'." (Heylighen and Joslyn, 2001, 3) While concepts like purpose, behavior and teleology have been under suspect in biology to support vitalism, they change to central features of a new science of communication and control in the animal and machine in cybernetics.

In 1943 the seminal paper "Behavior, Purpose, and Teleology" by Arturo Rosenblueth, Norbert Wiener and Julian Bigelow was published in "Philosophy of Science". It is often interpreted as a kind of birth certificate of US-American cybernetics.[17] Rosenblueth, Wiener and Bigelow conceptualize (human) behavior as the (negative) feedback of errors, of processes of trial and error and as the result of a tight coupling of system and environment. The focus of attention shifts towards the (prediction of) teleological or non-teleological – which means contingent – behavior of systems (black boxes), while the features of organisms are no more of interest. This approach of negative feedback and the concentration on behavior, on the relation of system and environment, of input and output is regarded as part of a new and "holistic" method.

It looks as if cyberneticians tried to develop an *approach that allows them to theorize dynamics and complexity and to translate these into practices of knowledge.* But while they are able to predict dynamic and complex behavior and to combine diverse ontic realms in a new and unknown way, *they loose the possibility to analyse the immanent characteristics of the single systems by reconfiguring entities (inclusive organisms) as black boxes*.

Cybernetics concentrates on the function and classification of the behavior of systems in general. Its openness to the dynamics, complexity and liveliness of the world is motivated by the desire to describe and control the dynamic behavior of organisms and technological systems (for example, weapon systems) which are very difficult to calculate and predict.

The insight of cybernetics is that the control of dynamic systems can't be static or (too) centralized, if one wants to integrate the unknown or even unforeseen in one's calculations. This is also the reason for the cyberneticians' interest in probability and game theory. Cybernetics is not about the exact calculation of behavior but about its probabilistic estimate – at least in the dominant version that was propagated by Norbert Wiener, who was searching for a universal theory of knowledge, order and calculation.[18] And it was primarily Wiener's cybernetic approach which was transported in disciplines such as pedagogy, control engineering, politics, and sociology. According to Wiener, noise – the disruption of communication – was associated with entropy, decay and death.

While cybernetics enabled the control of (more) dynamic systems and an estimation of systems' behavior, it is highly questionable to identify this approach with

[17] see Stewart (1959/2000), Bowker (1993), Hayles (1999).

[18] For the differences in the epistemological approaches of Wiener and von Neumann see Lenhard (2007).

an interest in the "unpredictable liveliness of the world and processes of open-ended becoming". The cybernetic interest according to Pickering is a very *specific* and reductionist kind of interest in performativity which rests on the calculus of probabilities and the systematization of dis- and reassembling (trial and error).

Symbol-Processing AI, Philosophy and Behavior-Based Robotics

In the 1970s and 1980s cybernetics disappeared as an independent, autonomous field of knowledge and it lost its relevance in the field of Artificial Intelligence (AI) already in the late 1960s. At this time, the symbol processing approach of AI won over the more biological-oriented approaches of cybernetics and early connectionism.[19]

Traditional AI is predominated by classical mathematics and formal logics, while biology and neurophysiology didn't play a role in AI research. The latter is dominated by the paradigm of information processing in which intelligence, the brain and the calculation of symbols is equated. Mental processes – identified with cognition or even intelligence in general – were more or less interpreted as the processing of calculations equated with algorithms. Alan Newell and Herbert Simon (1976) developed the well-known hypothesis of the "physical-symbol-system" which stated that "the processing of symbols, which are necessarily based upon a physical system, is sufficient to model and produce intelligence, if the rules for processing symbols and for the physical machine are powerful enough. In addition, they argued that the rules of the physical machine 'computer' dispose of this power. These ideas explain why the representation of knowledge, i.e., *the adequate modelling of the world via symbols and logical inferring* [. . .] have played, and continue to play such a prominent role in this research paradigm" (Christaller et al., 2001, 66; my translation and emphasis).

This kind of modelling abstracts from all physical and material aspects. The assumption is predominant that mental processes can emerge regardless of the physical system. Embodiment is irrelevant for GOFAI. The internal processing of symbols and the representation of knowledge are regarded as the distinctive features of intelligence. Accordingly, robots are more or less understood as mobile computers. They were equipped with a few sensors and actuators to make some environmental information available, but the main focus was on internal processing, representation and *plan-based* action on the basis of pre-programmed "knowledge".

In the 1970s and 1980s, AI researchers believed that decision making follows precise rules. As Lucy Suchman formulated in her critique of traditional AI: "The

[19]Think for example of Rosenblatt's neuron-inspired learning device "perceptron" which was radically critised by Marvin Minsky and Seymour Papert (1969). The success of their critique was one of the reasons for the following dominance of traditional AI until the mid 1980s (Pfeifer and Scheier, 1999).

logical form of plans makes them attractive for the purpose of constructing a computational model of action, . . . ” (Suchman, 1987, ix)

Given the precondition, traditional AI assumed that cognitive processes could be formalized and mechanized through expert systems which contained these rules and the help of databases with experts ́knowledge and (decisions). After some years of research it became evident that patterns of human behavior are much more complex and dynamic – as many critics argued before: “I will argue that all activity, even the most analytic, is fundamentally concrete and embodied” (Suchman, 1987, vii). As knowledge is related to experience, which mostly implies tacit knowledge beyond precise rules, it cannot be (easily) extracted and abstracted and used in a different context. Difficulties and unsolved problems were not only dominant in the field of expert systems, but also in robotics. After decades of research, AI could not present much progress in such fundamental research areas such as navigation, speech or object recognition. The robots were very prone to any kind of disturbances and noise and couldn’t agitate properly in real world systems (think, for example, of walking, climbing stairs, moving on rough underground, etc.). Despite the ambitious visions of early AI, many of its projects seem to be at least impracticable. Rolf Pfeifer, head of the AI laboratory at ETH in Zurich (Switzerland) and his colleague Christian Scheier describe this situation in their book “Understanding Intelligence” (1999) in the following way: “. . . we began to run into fundamental problems with artificial intelligence. In the mid-1980s we had already been working with expert systems for a number of years. Over time we realized, as did many others, that the technology did not fulfil its promises. Accomplishing what we proposed turned out to be much harder than expected: Only a very few of the projects we undertook ended up with systems that could be used in everyday routine practice. *The problems were not simply of practical nature, they were somehow insurmountable*.” (Pfeifer and Scheier, 1999, xviii; my emphasis)

While symbol processing systems such as chess computers or industrial robots with clear defined tasks which operated in static, in-door environments were quite successful, any systems that should cope with non-planned behavior and react in real-time to an unknown environment didn’t work properly – even after one decade of research. Considering the limitations of GOFAI, more and more roboticists reoriented themselves towards biologically-inspired approaches such as artificial life and connectionism. They distanced themselves more and more from the information processing perspective and its favour for formal logic and mathematics. Biological concepts such as emergence[20] or life got more and more prominent, while old concepts such as representation and the quantitative understanding of information were questioned. Katherine Hayles describes this situation in an illustrating anecdote: “[. . .] researchers assumed that artificial intelligence should be modelled on conscious human thought. A robot moving across a room, for example, should have

[20]There is no common understanding or even acceptance of the concept of emergence by the AI and AL community – despite or maybe because of the central function of this concept; see Emmeche (1994); Langton (1996), Cordis (2001), Christaller et al. (2001).

available a representation of the room and the means to calculate each move so as to map it onto the representation. [Today's director of the MIT AI Lab, Rodney; JW] Brooks believed this top-down approach was much too limiting. He saw the approach in action with a room-crossing robot designed by his friend [...] Hans Moravec. The robot required heavy computational power and a strategy that took hours to implement, for each time it made a move, it would stop, figure out where it was, and then calculate the next move. Meanwhile, if anyone entered the room it was in the process of navigating, it would be hopelessly thrown off and forced to begin again. Brooks figured that a cockroach could not possibly have as much computational power on board as the robot, yet it could accomplish the same task in a fraction of the time. The problem, as Brooks saw it, was the assumption that a robot had to operate from a representation of the world." (Hayles, 2003, 101)

Brooks (2002) was influenced by the cybernetician and neurologist William Grey Walter who built his famous "tortoises" Elsie and Elmer in the 1940s. These two small, animal-like robots were based on a tight coupling of system and environment and able to explore their environment, to search for light sources as well as to recharge autonomously their batteries. Central principles of these electro-mechanical tortoises beside autonomy were self-regulation (feedback) and spontaneity. They functioned without central representation (of their world). Putting up Grey Walter's ideas from the 1940s, Brooks claimed that *intelligence doesn't need central representation* and that the world would be its own best model.[21] This approach does not only rediscover principles and theorems of cybernetics, but also draws explicitly on the philosophical critique of symbol-grounded AI. Since the 1970s, philosophers such as Hubert Dreyfus and Barbara Becker as well as science studies scholars like Lucy Suchman or Harry Collins[22] criticized AI's functionalist concept of intelligence for its lack of embodiment, materiality, situatedness and embeddedness. For example, in the 1970s the US-American phenomenologist Hubert Dreyfus challenged the reductionism of AI and its Cartesian separation of body and mind in his well-known book "What Computers Can't do" (1973). He profoundly challenged the idea that cognition should be nothing more than the simple and passive input of information. For him, the body is not an obstacle for, but a constitutive element of cognition. He regards the interaction with the environment and the sensual, bodily experience – the embodied, sensory input of information as roboticists call it – as essential for cognition.

It is amazing that embodiment became a distinctive feature of the new behavior-based robotics. It is increasingly regarded as a central condition of intelligent systems. In his memo of 1986, the roboticist Rodney Brooks uses the philosophical critique of Hubert Dreyfus to argue for a new and embodied robotics that relies on a tight coupling of system and environment and leaves behind pure simulation and the artificial impoverished toy worlds of GOFAI.

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[21] Brooks (1986); Brooks (2002).

[22] Becker (1992); Dreyfus (1972); Suchman (1987).

But it is not by chance and not only due to his professional background that Rodney Brooks stresses his solely technical interest in solving the problem: "In this note we use a technical rather than philosophical argument that machines must indeed have a rich background of experience of being if they are to achieve human level intelligence. Unlike Dreyfus however, we conclude that artificially intelligent behavior is achievable with computers without the aid of holograms, resonance, *or other holistic techniques*. Rather, by adopting an incremental construction approach, progress towards this goal can be expected soon. (Naturally, the author and his students are currently following this enlightened path.)" (Brooks, 1986, 1; my emphasis).

In the paper it becomes obvious that the path from GOFAI towards new robotics leads towards the design of new ways to model and to control robots and technical systems, respectively. This approach is not (mainly) about a better understanding of intelligence, of how the mind works and the relation between representation and performance but about building systems and mobile computers, in particular that are capable of interacting with the world – in one way or the other.

New AI now tries to build embodied systems. The construction of these systems is inspired by biology and "its natural principles" and works "bottom-up". Only mobile and embodied agents that adapt themselves to the environment are seen as capable of managing real-time interaction with the environment, navigation and object identification.[23] They regard embodied, autonomous and mobile systems as the future of intelligent systems.

The interest in bottom-up approaches can be seen as part of their search for alternative methods and approaches. A roboticists described his view of the necessity of new methods and approaches in an expert interview[24] in the following way: "I believe, that in biological contexts people are still too much fixated on the world view of the physical sciences, as it originated in the mechanistic time, especially concerning exactness and so on, ..., rigid organization [of their research; J.W.], or causality, mono-causality. .... I think this is not adequate in this field [of research; J.W.] and – as one can see on other levels as well – in ecology or in research of the biosphere. What is really important is to understand the boundary conditions, under which certain processes are possible. And I am not sure on which level it will be possible to understand these processes at all. I am not sure whether this knowledge will be necessary in detail, but it is for sure important to understand under which conditions what kind of processes are possible. I think we will not get much further with regard to living systems. At least in my view it would be a quite demanding goal to achieve this. ... *The classical world view of the physical science is much too narrow to understand the phenomenon of the living world. And the level on*

[23] see also Christaller (1998, 106).

[24] I conducted these (and other) expert inverviews with Artificial Life reasarchers and roboticists in the USA and Germany during the research project '*Mathematik des Lebens – Konstitution und Geschlechtscodierung eines neuen Lebensbegriffs durch die Artificial Life-Forschung* ' (The Mathematics of Life – Constitution and Gendering of a New Concept of Life in Artifiical Life Research') at the Department of History, Technical University of Braunschweig, 2001–2003.

which one can comprehend them is for sure one beyond the mono-causal, analytic, reductionist view, but at the same time it is not about holism, but something has to be developed which goes beyond that and encloses both parts." (from an expert interview with a roboticist; my translation and emphasis)

The questioning of the body-mind dualism is part of this quest for an alternative approach. For example, roboticists Kerstin Dautenhahn and Thomas Christaller (1997) claim that the relation of cognition and the physical constitution of a system must be understood not as independent from each other but as a tight feedback coupling.[25] This stance with its critique of Cartesian dualism became also prominent in some approaches of brain research. Think for example of the well-known neurologist Antonio Damasio who claimed that embodiment is a central condition for human intelligence: "(1) The human brain and the rest of the body constitute an indissociable organism, integrated by means of mutually interactive biochemical and neural regulatory circuits . . . (2) The organism interacts with the environment as an ensemble: the interaction is neither of the body alone nor of the brain alone; (3) The physiological operations that we call mind are derived from the structural and functional ensemble rather than from the brain alone . . ." (Damasio, 2000, xvif) While he is not challenging the hierarchical order between intelligence and the body, between the brain and "the rest of the body", he advocates their intimate entanglement.

Some researchers of new AI put the values of science even more radically into question by abandoning – at least partly – its claim to "model the world without contradictions in an objective and complete way" (Christaller et al., 2001, 72; my translation). This epistemological stance might be the logical consequence of an approach that favors embodiment, situatedness and embeddedness.

This epistemological stance is different from that which dominated traditional AI, mathematics, cognitive science as well as philosophy. The mathematics which is now on the agenda, is the statistically-based mathematics of nonlinear dynamics.

> *"the real thing is: how do we get spontaneous creation of surprising things" (from an expert interview with a roboticist)*

Biological Machines: Autonomy, Adaptation and Trial and Error

New robotics – influenced by cybernetics and artificial life research – strives for artificial intelligent systems that operate autonomously in open and complex environments.[26] Biological processes are regarded as the decisive conditions for intelligent behavior instead of precise calculation or knowledge representation. Embodiment, situatedness, adaptation, autonomy, system-environment interaction, learning and self-reproduction[27] are seen as the central features of intelligence. Accordingly new

[25] see Christaller et al. (2001, 84).

[26] see Becker (2000).

[27] Boden (1996), Christaller (1998), Christaller et al. (2001), Brooks (2002); Pfeifer (2001).

approaches in robotics emerge such as behavior-based robotics,[28] evolutionary[29] or situated robotics,[30] "Embodied Artificial Intelligence"[31] or autonomous intelligent systems.[32]

By approaching biology, the researchers hope not only for a better understanding of living systems but for the emergence of new, successful ideas concerning the construction of software as well as hardware for artificial systems. A researcher describes this move in the following way: "a direction we are trying to go is to get closer and closer to biology. In the sense that we are abandoning a lot of conventional electronics or conventional circuits because we think that it is already too much constrained. It doesn't have space for reactive autocatalytic properties where you get new matters coming out. So, it is maybe *to go back to the biological basis of real life and try to put it under different conditions*, try to expose it to different types of experiences or try to direct evolution in different ways. And try to see what are the possible alternative mechanisms that you get out of it." (from an expert interview with a robocist)

Differing from traditional AI, new robotics is focusing on the intrinsic properties of the physical quality of embodied intelligent systems. Researchers hope for new materials that might support emergent effects. The development of new combinations of materials – such as organic (neuronal) tissue and chips – is regarded as promising for the production of new, more flexible and intelligent artefacts. Today, many roboticists are convinced that it is important to build artificial systems out of the right material because this can – for example – help to optimise their energy efficiency or to simplify their control mechanisms.[33]

The principle of "bottom-up" is another important slogan, if not magical incantation of cybernetics and especially new robotics. It builds on the old idea that the whole might be more than the sum of its parts. What else expresses the idea of emergence as something that is triggered by the multi-layered interplay of many modules or programs? It rests on the condition that intelligence is the product of the system-environment coupling and that organisms in general function on the basis of a huge number of very loosely-coupled parallel processes. Consequently new robotics breaks down the behavior of the system into small modules, in so-called reflexes based on the principle of stimulus and reaction or sensory-motor feedback circuits (such as e.g., the avoidance of obstacles or the search for a source of food/energy, etc.). Rodney Brooks famous "subsumption architecture" is an architecture for autonomous robots, in which modules can be implemented independently to enable their mutual interaction. To reduce symbol processing as far as possible,

[28] Brooks (1986); Christaller et al. (2001).

[29] Husbands and Meyer (1998); Nolfi and Floreano (2000).

[30] Steels and Brooks (1994).

[31] Pfeifer and Scheier (1999); Pfeifer (2001).

[32] For example "Autonomous Systems" is the name of the research unit on behavior-based robotics of the Fraunhofer-Institute at St. Augustin (Bonn, Germany).

[33] Pfeifer (2001).

sensory and motor signals get short-circuited to ensure a tight coupling of system and environment and to support emergent behavior. Researchers hope that this might provide a basis for the "evolution" of unexpected, not pre-programmed behavior. This behavior is used as a central resource to evoke new intelligent behavior which can be analysed via post-processing. These new approaches and research strategies are often labelled as an inclusion of *spontaneity, versatility and shape-shifting* into the research process and new properties of the now *biologically-inspired* systems. In a way, unpredictability, spontaneity, versatility and shape-shifting become essentials parts of the leitmotif of this new techno-rationality. It contains the *vision of the construction of self-adapting, evolving, living machines that 'outgrow' their programming and which develop their own categories, language and other sophisticated features which are characteristic of autonomous systems in the literary sense of the word*.

> *"Contrary to the expectation, that the on/off-position of a switch is a concrete, stabile phenomenon of information, it is a very fragile thing. Endlessly is the danger that it is engulfed by the noise of the channel. This enemy of information, >the wild animal<, is permanently on the lookout to destroy signals" (Volker Grassmuck, 1988, 45 (my translation)).*

On the Devil of Disorder and the Angel of Noise

Since its very beginning, cybernetics and Artificial Intelligence were very effective and effectful in telling powerful salvation as well as apocalyptic stories about their research fields while "real" successes in technical terms were often missing. It is true, that at least robotics made considerable progress in terms of more smoothly and flexibly moving robots, climbing up stairs, dancing etc. The same could be said about the cooperation of robots with their environment. But still many basic capabilities in the field of navigation, object and speech recognition, complexity (scaling-up) etc. are missing.

Against this background, the new attention on contingency, trial and error as well as tinkering methods and their hasty identification with spontaneity, versatility, and the living could be interpreted as another smart salvation story and clever research strategy to promote the interest in one's own research, to help its funding and to secure the attention of other researchers and of the media.

Andrew Pickering perpetuates these semantic strategies by describing the ontology of cybernetics as a pure thematization of the living which is absolutely different from classical science: "My suggestion is that cybernetics grabs onto the world differently from the classical sciences. While the latter seek to pin the world down in timeless representations, *cybernetics directly thematises the unpredictable liveliness of the world and processes of open-ended becoming*. [. . .] [I]t is as if the cyberneticians have lived in a different world from the classical scientists." (Pickering, 2002, 10; my emphasis) Pickering sketches a very similar picture of behavior-based, autonomous robotics: "Hard-line autonomous robotics is deeply anti-representational. It wants to build robots that are always in the thick of things – essentially embodied, operating on inputs from the world, transforming them into outputs, monitoring what comes back, adjusting outputs again, and so on – *and all of this without the existence of any abstract, formal, detached representation of the world* in which the robot lives. *An exemplification of the dance of agency itself.*" (Pickering, 2002, 10f; my emphasis)

This romantic and over-optimistic description of cybernetics as well as new robotics is grounded in their attention on contingency, trial and error, the surplus of the living as well as the method of tinkering. The latter is a more or less systematically performed way of combining modules in a bottom-up way, of trying out which parts might fit to each other and what the outcome of the interaction of these parts might be. Tinkering – now interpreted as a genuine method of nature herself[34] – is seen as an important tool to bring emergent processes into being.

Pickering is too rash when he ascribes cybernetics an unlimited interest in the unpredictable and claims their systematic usage of tinkering and trial and error. The idea of operating at the edge of order and chaos as well as that of a systematic production of unexpected processes seems to be more a product of the theory of dynamic systems, of chaos theory and a certain version of self-organization theory (like e.g., autopoiesis theory) which understands self-organization as a dynamic (re-) production of the internal order of a system and as a "springboard to emergence" (Hayles, 1999, 11). Accordingly, Peter Galison (1994) and Andrew Pickering (1998) himself stress that Norbert Wiener regarded surprise, contingency and noise as the source of disorder and uncontrollability.

To clarify this point: In the 1940s Norbert Wiener developed an >Antiaircraft (AA) predictor<, a planned air defence system, that filtered the irregularities of the zigzag path of an enemy airplane to track its future position and thereby enabling one to shoot down the plane despite the delay of the air defence missile. The unexpected, surprise, chance and noise are the "natural" enemies in a (military) research project that wants to calculate a dynamic human–machine system: "It [the anti-aircraft predictor; J.W.] lived in real time, but always looking backwards to extract a trend that it could project in the future, and, in extracting that trend, chance (chaos, noise, fluctuation) was the enemy, a confusing disturbance that one had to struggle to counteract, mathematically and technologically."(Pickering, 1998, 5)

Pickering and Galison stress that Norbert Wiener regards disorganization, chance and noise as the arch enemy, as the source of disorder and unpredictability.[35] Wiener writes in "Human Use of Human Beings": "The scientist is always working to discover the order and organization of the universe, and is thus playing a game

[34] see Jacob (1977).

[35] Pickering claims that the early British cyberneticians such as Ashby, Beer, Pask and Walter, were those who engaged themselves with the unpredictable, the surprise and the unforeseen, while Norbert Wiener built on more total visions of communication and control. In this paper I concentrate on the work of Norbert Wiener because he seems to be the key figure in cybernetics in the midst of twentieth century on the one hand. On the other hand he was also very successful in translating his approach into other disciplines.

against the arch enemy, disorganization." (Wiener, 1950, 35) Galison comments: "Cybernetics, that science-as-steersman, made an angel of control and a devil of disorder. ... But perhaps disorganization, noise and uncontrollability are not the greatest disasters to befall us. Perhaps our calamities are build largely from our efforts at superorganization, silence, and control." (Galison, 1994, 266)

Unpredictability, emergence and noise have become the 'angels' of behavior-based robotics today. According to this new techno-rationality order emerges out of chance, out of the unpredictable, dynamic and multiple combination of simple processes and clever strategies of trial and error. These processes are not instantiations of the living, but by working with repetition and difference, relying on the calculus of probabilities, sometimes results in something new and productive which can be exploited for improving human–machine systems. Relying on emergent processes and the production of the unexpected (probability) does not mean to abandon the demand on controlling nature as Peter Galison and others had hoped for. It is the other way round: *This new science – romanticized by Pickering and some of its own proponents – tries to exploit technically dynamic and complex processes that cybernetics avoided. Spontaneity and the so-called surplus of the living – which was regarded for a long time as the non-exploitable – are getting more and more integrated via tinkering, methods of trial and error*, *postprocessing etc. (and modern and increasingly fast computers) in this new bottom-up technique of control*. A roboticist describes this approach in the following way: "if non-linear systems are interacting, than we do not have any theory which can predict what might be the outcome of such an interaction. I bet that with the help of evolution there might emerge cognitive processes – whatever that means. ... Under which conditions might it be possible that emergence happens? What are the necessary boundary conditions for such a process? It is not possible to let somehow something self-organize and then there will be emergent processes. That is how people often picture it. *I am sure there are boundary conditions under which emergence can become possible and others when it will not become possible.* If it will happen under the right conditions – that is another question." (from an expert interview with a roboticist)

This new approach is centered on the *determination of optimal boundary conditions to bring emergent processes into being, while ignoring the intrinsic properties of organisms and refraining from the objective description of universal laws. Evolution via tinkering, the processes of trial and error are the main tools to help the construction of complex dynamic and therefore intelligent systems, which are beyond the analysis and control of the classical sciences.* These processes and methods are inspired by biology and the theory of dynamic systems. The use of biology (and especially ethology and theoretical biology) is justified – as cybernetics already did 40 years ago – with the gain of genuine valuable knowledge for biology itself, but also by the usefulness of biology as a test bed for engineering and robotics: An engineer pictures this two-fold task in the following way: "So, if you're expecting biology to provide this template for engineering, it just isn't going to, but it can provide a challenge [...], for engineering technology that is very analogous and potentially powerful. So [...], I'm not doing it because I expect to learn specific things that I can carry out in engineering, I'm doing it [...] primarily to help the biologists and primarily trying to build tools that will help biology and medicine. Secondarily I'm trying to create a test bed for a general set of tools for studying complex networks that will be critical in our engineering infrastructure. So that's a secondary issue and very, very casually is any hope that specific principles will come out of biology that will be relevant, that'll be nice but I think betting on that would be a mistake" (from an expert interview with an Artificial Life researcher)

At the heart of this new science lies the search for the proper boundary conditions which will enable to trigger emergent processes. The main belief is that there are at least some central principles of organization in complex dynamic systems – let them be organic or non-organic. *While the analytical approach breaks down its object in single parts to analyze them, this new techno-rationality builds on (re-) combining different modules in nearly endless repetition to stimulate the emergence of more complex behaviors and systems*.[36] *This means an inversion of the analytical approach.* The contemporary science of communication and control looks forward instead of behind.

The logic of research centers on the emergence of the unexpected (by tinkering and testing what might work). It searches for specific conditions so that it can foster processes of emergence and to open up possibilities which allow the exploitation of surplus processes in a technical way.

These processes are identified much too rashly with the openness of the living, creativity and the unknown – features which were for a long time regarded as the specific property of human beings or organic systems, respectively. Now they are effectfully ascribed to biological and technological processes. Galison hoped for noise, chaos and chance as potential remedies against the control mania of cybernetics. But now it seems that they are transformed into effective research strategies of systematized tinkering, postprocessing and genetic programming. Thereby they have become productive means to ensure new ways of control and to construct efficient artefacts on the basis of a comprehensive systemic biocybernetic techno-rationality. The Augustinian devil of noise and chaos, which was fought by Wiener, has changed its role. It is advanced to the position of the angel in biologically-inspired and behavior-based robotics.

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[36] See Hayles (1999) and Weber (2003).

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An Epoch-Making Change in the Development of Science? A Critique of the "Epochal-Break-Thesis"

Gregor Schiemann

Introduction

In recent decades, several authors have claimed that an epoch-making process of change in the development of science is currently taking place. The authors conceive the development of modern science as a continuous process that began approximately between the sixteenth and late eighteenth centuries, and that is discontinuously ending in our time. But the *epochal break* thereby formulated is only rarely dealt with on the conceptual level, and even then not in a uniform manner (see section "Assertions of Current Epochal Changes and the Problem of Their Conceptual Definition").[1]

This terminological weakness makes it more difficult to assess the various assertions of an epochal break. What is it that lends an epoch-making character to a process of change? Is there a specific dynamic that distinguishes epochal changes from other processes of change? What is the significance of the claim of discontinuity associated with the word "break"? In what way are contemporary descriptions involved in the assertions of epoch-making changes (which might occur only at a moderate pace)? In order to be able to answer these questions, I will propose a *concept of epochal change* that takes up the intuitions of the authors asserting such a change, but which also allows for a critical assessment of these claims. According to this concept, it is typical of epochal changes that they begin within a particular subarea of the sciences, that they occur in a manner that is at best partially discontinuous – the concept of an "epochal break" therefore appears inappropriate – and that they transpire over a relatively long period of time (see section "The Concept of an Epochal Change in the Development of Science").

In the interest of assessing the transformations of contemporary science asserted by the authors in question, as well as transformations that they have not taken into

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[1] The term "epochal break" is not found in all the relevant publications, but is suitable to characterize the assertion of a discontinuous process of epoch-making change.

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consideration, I think it is sensible to stick to *societal subsystems as a frame of reference.* Given this prerequisite, differences between the subareas of science, to which the current transformations refer, become more clear: they are correlated with different societal subsystems. I will take these correlations as a guideline in assessing the historical origins and the form of progression of some transformations that are *candidates for the status of an epochal change* (see section "Candidates for the Status of Epochal Transformations in the Recent Development of the Sciences").

Assertions of Current Epochal Changes and the Problem of Their Conceptual Definition

The most recent assertions of an epochal break in the sciences appeal to developmental tendencies that have been apparent since approximately the 1980s. They concur with respect not only to their estimation of the beginning point of the changes but also to some fundamental elements of their characterization of the changes. The commonalities appear above all in the historical demarcation of the new characterizations, which are constitutive of the concept of the epochal break. For example, the *contrast to modern scienc*e, as it developed up to the second half of the last century, is included in all the definitions of the transformation. Accordingly, the denominations often claim to distinguish a type of science that follows upon modern science. M. Gibbons et al. speak of "Mode 2", S.O. Funtowicz and R. Ravetz of "post-normal science", J. Ziman of "post-academic science", and P. Forman of the "postmodern primacy of technology". In the following, I would like to discuss some examples of the common historical positioning of the epochal break, and to show that the concept of an epochal break cannot be sustained in the cases under discussion. I will not take the conception of post-academic science into consideration.[2] In addition, I will draw upon the conception of the "Triple Helix of university-industry-government relations", as well as two descriptions of a fundamental transformation that do not assert a discontinuity, or do so only in a qualified manner.

Mode 2

Gibbons et al. identify Mode 1, which precedes Mode 2, with modern science as it goes back to early modern times.[3] They characterize it as the "complex of

[2]John Ziman's conception of post-academic science is related to the conception of Mode 2: cf. Ziman (2000, 81), and Nowotny (2006). Moreover, B. Latour's and D. Haraway's conception of technoscience will also be left to the side here. There are different variants of it, a comparative discussion of which is beyond the scope of this critique, which will be limited to dealing with particular examples. On Latour's and Haraway's use of the term "technoscience" as an epochal conception, see Reichle (2004), Weber (2003) and Ihde and Selinger (2003).

[3]The Mode-2 thesis is presented and elucidated in Gibbons et al. (1994, 2003), as well as in Nowotny et al. (2001). For criticism, see Elzinga (2004), Weingart (1997) and Schiemann (2009).

ideas, methods, values and norms that has grown up to control the diffusion of the Newtonian model of science to more and more fields of enquiry and ensure its compliance with what is considered sound scientific practice" (Gibbons et al., 1994, 167). They maintain that Mode 2, which arose in a discontinuous fashion, differs "in nearly every respect" from Mode 1 (loc.cit., VII). The former has not replaced the latter but, rather, appeared alongside it as a distinct system. The persistence of Mode 1 presents an element of continuity that contrasts with the idea of an epochal break. The authors characterize the difference between the two modes by appealing to characteristics of Mode 2 that share a common tendency to foster an orientation toward socially *useful applications* (loc.cit., 3 ff. and 167). While this practical component of the current transformation of science is common to the various conceptions of the epochal break, judgments of the structural changes connected to it differ and are the subject of controversy. The Mode-2 conception asserts a partial *dissolution of the boundaries* that previously separated the subsystems of society (science, the state, the market and culture), and gives special prominence to the dissolution of the separation between academic and non-academic production of knowledge. In place of these separations, it envisions the formation of new, heterogeneous structures, in which scientific, technical, economic, political and public interests are taken up in multifarious ways (Nowotny et al., 2001, 21 ff. and 245).[4]

It is claimed that these institutional changes have an impact upon the "epistemological core", which no longer consists in "irrefutable and invariant laws" (loc. cit. 196) but in "individual, social and cultural visions of science" (loc. cit. 198).[5] This *new conception of the epistemological core* is taken to reveal the fundamental character of the epochal break. There is indeed a basis for this viewpoint, insofar as epistemological characteristics represent a decisive historical constant for science over a long period of time. I group these characteristics together under the label "*classical conception of science"*, according to which scientific knowledge is marked by truth, generality and necessity.[6] The new conception introduced by the notion of Mode 2 remains ambiguous, though, since it denies epistemological characteristics, claiming that the epistemological core is empty (Nowotny et al., 2001, 225), but at the same time continues to grant them significance, as is revealed in the demand for a new epistemology (loc. cit. 247 f.).

Although the authors give particular reasons for the *beginning of the epochal break* in the 1980s (Gibbons et al., 1994, 10, 17 and 44), they also trace some essential characteristics of Mode 2, such as the development of non-academic research and the retreat from traditional validity claims, back to the nineteenth century

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[4]For criticism of the supposed dissolution of the boundaries between societal subsystems, see Section "Candidates for the Status of Epochal Transformations in the Recent Development of the Sciences" below.

[5]This thesis is emphasized especially in Nowotny (1999): "What is currently at stake is nothing less than a new conceptualization of the epistemological core of science, and therefore also a central component of the image of science (loc. cit. 29).

[6]The classical conception of science was paradigmatic from antiquity until the nineteenth century, cf. Schnädelbach (1983, 106 f.), Schiemann (2009, Chapter 2).

(e.g., loc. cit. 22; Nowotny et al., 2001, 197). As an historical claim, the epochal break thesis thereby becomes questionable. What speaks against pushing back the start of the transformative process as well? What is the relationship between the factors that seemingly prepared the way for the supposed break and those which initiated it? Is it a matter of a more gradual or a more discontinuous change?

With regard to the present state of affairs, the authors assert a mutual influence between the clearly distinct forms of knowledge production: they believe that Mode 2 relies upon and also transforms Mode 1. Not much is said about the continuing development of Mode 1, except that it "will become incorporated within the larger system [. . . of] Mode 2" (Gibbons et al., 1994, 154). The revolutionary transformation is therefore not yet complete, and the form of science that will succeed upon modern science as it has existed until now cannot yet be characterized fully.

Post-normal Science

In contrast to the conception of Mode 2, the conception of "post-normal science" espoused by Silvio O. Funtowicz and Jerome R. Ravetz distinguishes the new form of knowledge production not only from the science that came about in early modernity.[7] The authors regard this science as belonging to a type that arose in antiquity and which could appropriately be characterized by T.S. Kuhn's concept of "normal science". While they, like Kuhn, impute a one-sided theoretical orientation to normal science, they see in the discontinuously arising post-normal science a *twofold turn to praxis*: to the praxis of knowledge production and to new objects of this production, which arise in specific contexts of application (Funtowicz and Ravetz, 1993, 118 f.). These objects – an example of which would be the ecological crisis brought about in part by the application of scientific technology (loc. cit. 95 f.) – are marked by a *complexity* which can be only partially grasped by theory. Epistemically, *uncertainty* is therefore a most salient characteristic of post-normal knowledge.[8] The processing of such new objects is, in their view, marked by conflicting values and high risks, and is only possible in direct relation to politics (loc. cit. 86 ff.). Just as Mode 2 takes over from Mode 1 its leading role, normal science is said to persist and to be substantially influenced by post-normal science (loc. cit. 110 f.). Hence, we again find an element of continuity that contrasts with the thesis of discontinuity.

In distinguishing post-normal science from a kind of science that goes back all the way to antiquity, the authors impart to the epochal break *a more far-reaching dimension* than is the case for the Mode-2 conception. With the increased historical scope, the characterization of the rift undergoes a shift toward a greater focus upon *epistemic characteristics*. The authors refer to the latter as constituting the "ideological function [of science] as the unique bearer of the True and therefore

[7] I am basing my presentation of post-normal science on Funtowicz and Ravetz (1993, 1994, 2001).

[8] Uncertainty is also a characteristic of knowledge in Mode 2. Cf. the subtitle of Nowotny et al. (2001): "Knowledge and the Public in an Age of Uncertainty".

of the Good” (loc. cit. 85, cf. 95 and 111).[9] The beginning of its destruction is dated at the beginning of the twentieth century (Gödel’s incompleteness theorems, Einstein’s theory of relativity, Heisenberg’s uncertainty principle), and is said to have enabled the subsequent genesis of post-normal science (loc. cit. 93 ff.). In a fashion similar to the Mode-2 authors, the relationship between the appearance of the supposedly epochal break and the processes preceding it remains somewhat vague. The break can be understood as an emerging insight that the truth claims of the classical conception cannot be realized. This insight has become established in particular in subareas of science occupied with certain complex objects. But the authors do not adequately justify their denial of the possibility that the theoretical understanding of complex objects could in the future continuously improve.[10]

Even though Funtowicz and Ravetz consider modern normal science a part of the more comprehensive type, they still regard it as a historical unit that they explicitly say began with the “scientific revolution” (loc. cit. 85, 117 f.). They take the impact of the caesura at the start of the early modern era to be in fact so profound that they even compare it to the break between normal and post-normal science (loc. cit. 117). Will this break have been the final revolution? A more practice-based and de-localized science could lose the capacity for discontinuous change, which is a typical feature of normal science in Kuhn’s sense. But the authors rightly distinguish clearly between the “scientific revolutions” of normal science and the revolutions that, as epochal breaks, affect the entire system of the sciences, and which cannot be ruled out for the future.

Triple Helix

“Triple Helix” is the term with which Henry Etzkowitz and Loet Leydesdorff dub the model they propose for characterizing the *new institutional interactions* among the three societal subsystems of university, industry and government.[11] Accordingly, these three distinguishable areas constitute bi- and tri-lateral networks and hybrid organizations (Etzkowitz and Leydesdorff, 2000, 111 f.), which in turn affect the definition as well as the development of each subsystem, and their relations among each other. Within this structure, there are communicative processes that are constantly re-organizing themselves and bringing about an endless innovative movement in which all the elements are, so to speak, able to switch sides, and which is illustrated by the image of the Triple Helix escalating ever upward. The authors believe that the formation of this new structure, which occurred during

[9]The conception of science that Funtowicz and Ravetz label “classical” is, with respect to the theoretical understanding of validity, related to Mode 1 (Funtowicz and Ravetz, 1993, 198 and 120).

[10]Cf. the critique in Carrier (2001, 30).

[11]The authors have presented and elucidated their model in numerous publications. For an introduction, see Etzkowitz and Leydesdorff (1998, 2000).

the second half of the twentieth century, resulted from the increasing importance of scientific knowledge for *economic development*. With respect to the university, the central feature of the model in this context is the claim that the industrial relevance of knowledge led to a *second academic revolution*. During the first such revolution, which we are told dates back to the late nineteenth century, the universities added research to their already existing function as teaching institutions. During the second revolution, the universities have, according to these authors, added a third task, namely the production of economically useful knowledge.

I would like to advance two points of criticism against this model. The first addresses the *historical localization* of the beginning of the increase in economic importance of scientific knowledge. Some elements of the interactions described by the model can be traced back to the nineteenth century. Structures of the technical universities founded at that time, for example, can be viewed as hybrids of university, government and industry. In Germany, research units at these state-financed and academically organized universities began to work more extensively and more closely with industry in the 1880s.[12] The other point of criticism has to do with the insufficient consideration that is given to the *general conditions and consequences* for the production of knowledge in the twenty-first century that result in fact from the new relations obtaining among university, industry and government. Although these relations appear more clearly here than in other conceptions, Etzkowitz and Leydesdorff do not adequately account for their scope.[13] Regarding the general conditions, the globalization of economic processes and the exponential development of information technology can be regarded as most important. As for consequences for the production of knowledge, I would point to the partial privatization and commercialization of knowledge production, as well as to the capitalization of universities and to their management according to business principles, the market-oriented direction of research, the increase of competition among individual researchers and research groups, the rise in intensity of work in knowledge production, and the standardization of education. Insofar as Etzkowitz and Leydesdorff do address these consequences, it is in relation to the increase in communication and networking. In doing so, they lose sight of aspects that are connected to the differences among the subsystems and to the criticism of the formation of the Triple Helix dominated by economic interests.

The "second academic revolution" only transforms a part of modern science. Science remains not only distinct from other societal subsystems, but also retains its academic structure. While post-normal science presents a more extensive break than Mode 2, the second academic revolution is a *comparatively more minor historical change*. Accordingly, there is hardly any relevance given to precise estimates of the point in time when the Triple Helix arose (cf. Etzkowitz, 2004). The authors in question speak of an arising *evolution* of the relations among university, industry and

[12] Manegold (1969, 395 ff.), and Wengenroth (2003, 242 ff.).
[13] Cf. Elzinga (2004, 8 f.).

government instead of an epochal break (Etzkowitz and Leydesdorff, 2000, 109). Their notion of an "endless transition" implies the onset of a period of continuous progression.

Postmodern Primacy of Technology

Paul Forman's assertion of a "postmodern primacy of technology" demonstrates that preserving the demarcations among societal subsystems within a description of the current fundamental transformation need not entail the conviction that this transformation is devoid of a discontinuous historical dynamic.[14] Forman believes he can show that there was a "sudden and drastic shift ca. 1980 in cultural presuppositions" concerning the *relationship between science and technology*. In Forman's view, the cultural primacy of science relative to technology, which persisted in the west for 2,000 years (Forman, 2007, 2), has been inverted within an astonishingly brief period of time. Rather than dissolving the boundary between technology and science, the transformation has brought about a new orientation of the relations between them and therefore a continuation of their distinguishability. While Forman's model comes close to the Triple Helix model with respect to this distinguishability between societal subsystems, it differs in that it is restricted to the *level of cultural ascriptions*. Forman is concerned with the "general discourse, of the denotative capacities of the terms 'science' and 'technology'", for which the "actual, factual relationship between science and technology is relatively unimportant" (loc. cit. 4 and 6), whereas Etzkowitz and Leydesdorff deal with real structural changes.

In focusing on cultural ascriptions, Forman is seeking to do justice to the *comprehensive character* of the epochal break he postulates – a connection that is similar to the relation between historical scope and epistemic characteristics in the conceptions of Mode 2 and post-normal science. With the onset of the modern era, which preceded postmodernity, the concept of science that arose in antiquity came to an end. Forman ascribes to science and technology each a meaning in which it is specific to an individual epoch as well as a meaning that is *constant throughout history*. According to the latter, "science" signifies conceptions of the world, while "technology" refers to things that would also exist independently of our conceptions (loc. cit. 10). As a further historical constant, Forman implies also that science is concentrated upon the processing of means, whereas technology aims to achieve ends (loc. cit. 3 and 71). In the *modern era*, the concept of science took on the historically specific character of "pure science" serving the "disinterested pursuit of truth" (loc. cit. 43, cf. 12 f.). Forman's conception of modernity is similar to the notion of a classical conception that we encountered in the discussions of Mode 2 and post-normal science. Because of its subordinate status within this conception, technology was

[14] For a presentation and discussion of Forman's thesis, see, above all, Forman et al. (2007).

apparently at risk of losing its independent conceptual definitions. It was not until the postmodern *valorization of technology*, which Forman, invoking the historically constant distinction between means and ends, dubs a "pragmatic-utilitarian subordination of means to ends" (loc. cit. 2), that the specific characteristics of technology came clearly to light. Forman's concept of technology, however, remains quite general and indeed vague. Technology, for him, is "simply the collective noun for all the many ways things are in fact done and made" (loc. cit. 10). Such a broad definition does not distinguish between everyday practices and industrial technology, which is Forman's chief concern. Moreover, it has an ahistorical character that runs counter to the thesis of a transformation of science.[15] In Forman's defence, though, one may note that the breadth of the definition is no accident. Rather, it is intended to do justice to the epoch-making content of the transformation. At any rate, according to Forman's construal of the cultural discourse, postmodern science accords primacy to theory-independent practice, which is neutral with respect to specific societal interests.

The countless pieces of evidence with which Forman seeks to substantiate the two primacy-relations reveal that he thinks of the concepts of science and technology as persistently opposing coordinates during the epochal transition. But it is questionable whether the relations among interpretational patterns, which have existed for centuries as basic definitions, can really undergo a radical shift in a comparatively brief period of time. While in Forman's description the *putatively abrupt transition* from modernity to postmodernity is quite clear, the causes of this caesura remain unclear. The "cultural revolt of the 1960s", which Forman cites as the cultural source of the reversal of primacy relations between science and technology, cannot in itself be regarded as sufficient, since it occurred 20 years before the beginning of the epochal break, and Forman gives no reasons to explain its supposedly delayed impact (loc. cit. 5). Moreover, one would have to inquire into the causes of this event as well.[16]

An assertion of an epoch-making change that is confined to cultural interpretive patterns is not plausible. Changes in the development of these patterns are indeed significant, but they constitute not sufficient conditions for epoch-making new conceptions of the sciences. Such new conceptions are *comprehensive* in the sense that they include various dimensions of knowledge production: its institutional structures, interactions with other societal systems, methods, theories and practical procedures, as well as related cultural interpretive patterns.

[15] Kline (2007) makes a similar argument against Forman's concept of technology.

[16] Forman regards the "demand for 'relevance' of science" (Forman, 2007, 5) as an aspect of the "cultural revolt of the 1960s" that helped prepare the way for the epochal break. He could have pointed to the "finalization-theory" as an example of this, but he assigns this theory to modernity in his sense (loc. cit. 47). Weingart, however, has shown that it, like Mode 2, is directed toward the context of application.

Second Modernity and Knowledge Society

Some of the prerequisites to the concept of an epochal transformation can also be encountered in descriptions of current fundamental changes in the sciences that do not claim a discontinuity, or do so only in a qualified manner. Such descriptions are well-suited to characterize the constitutive elements of an epoch and of a possible transformation within this framework.

The *conception of the “second modernity”* is a paradigmatic example of this. Its proponents speak of a profound “structural transformation of the system of science”, brought about by the “displacement of the primacy of reflection to reflexivity”. At the same time, they emphasize that there is “no complete break in the process of modernization” (Beck and Lau, 2004, 20 and 183).[17] In the second half of the twentieth century, they say, a process began in the sciences as well as in other societal subsystems and in the relations among them, by which the hitherto dominant reflective form of rationality itself became the object of reflection, and thereby entered into the state of reflexivity. The partial discontinuity connected with this change is understood with reference to the distinction between basic principles and basic institutions. The latter are “institutional solutions” that aim in different ways to realize the guidelines implied by the former. It is only these institutional solutions and not the basic principles that are undergoing a discontinuous transformation. In other words, modernity is marked by a set of principles that have in themselves remained constant, but which have been *understood* differently during the different developmental phases they have gone through so far – namely, during the first and the second modernity, the latter having arisen in the second half of the twentieth century. One example has to do with the institutional role of the sciences in the discourse concerning the orientational function of the distinction between nature and society. While the determination of this distinction “in the first modernity clearly counts among the tasks of science, this demarcation and its justification are pluralized in the second modernity” by the influence of other institutions, civil society, the state and the market (loc. cit. 21, cf. 65 ff.). If one accepts the theory of the second modernity, the *transformation* of basic principles would constitute a sufficient condition for an epochal break.

To name another example of a claim of continuity, the *theory of knowledge society* describes new components of the order of knowledge, which consist above all in the “increase of practical relevance of science” for society, but do not present “a fundamental or qualitative break” with the order of knowledge existing since early modern times (Weingart et al., 2007, 33). The continuities claimed by this theory are more far-reaching than those claimed by the proponents of the notion of the second modernity. They are not limited to general conditions that are related to the basic principles of the second modernity (e.g., epistemic orientation, ideological neutrality of research) in their fundamentally guiding function. Rather, they also include

[17] Programmatic presentations of this view are found in: Beck and Bonß (2001) and Beck and Lau (2004).

institutional facts, such as the system of the disciplines (loc. cit. 41 ff. and 182 ff.) and the distinction between basic and application-oriented research (loc. cit. 31 ff. and 97 ff.). It is an open question what kind of dissolution of the continuity would lead to a new order of knowledge and whether the establishment of such a new order would constitute an epoch-making event.

The Concept of an Epochal Change in the Development of Science

With the exception of Forman's conception of postmodern science, the aforementioned characterizations of current fundamental changes in the development of science make claims that are not limited to a transformation of cultural interpretive patterns. For the most part, they start out from investigations within *sociology of science* dealing with structural changes in the institutional constitution of the scientific production of knowledge, and derive transformations of the epistemological characterizations of scientific knowledge. The depth of the transformation, according to the conceptions of Mode 2 and post-normal science, is precisely reflected in the scope of the breakdown of classical epistemological characterizations of the sciences. It is worth noting, however, that this breakdown is also taken up in Forman's conception.

Having surveyed various claims of an epochal break, it is apparent that the changes that are under discussion are, as a general rule, presently in a *beginning stage*, and are focused on a *subarea of the sciences*. The authors tend to anticipate that the emerging new characteristics will *in the long run* take on a leading role in the sciences. Hence, Mode 2 and post-normal science are said to establish themselves alongside their predecessors and, without undermining a continued relevance of these predecessors, to stake a claim upon the guiding function that has until now belonged to them. The Triple Helix model starts out from a particular sphere of knowledge production, namely the areas that produce economically useful knowledge. Forman's thesis can also be understood as relating to a restricted beginning of a more comprehensive process. The epochal change is initially limited to a (former) subarea of science, namely technology, and its cultural interpretive patterns. Subsequently, the change could progress to other subareas and no longer be limited to the cultural dimensions of science.

My definition of the *concept of an epochal change in the development of science* refers in a twofold sense to the aforementioned claims. It takes up the relationship between subareas and the entirety of science (Section a) and seeks to do justice to the possible long-term character of the transformations under discussion (Section b). Moreover, the concept I am proposing incorporates conditions for the description of an epochal change (Section c). Alongside the current changes that I have been discussing, a further point of reference for the treatment of these three issues is presented by the early modern beginnings of modern science, the epoch-making character of which is largely uncontroversial in the literature on the history of science.[18] The concept of an epochal change is specific, since it refers to particular historical events and seeks to descriptively characterize their common features.[19]

(a) *Epochal changes begin in a subarea of science and proceed to transform the entire system of the sciences.* They are comprehensive, since they change the concept of science and affect various (cultural, societal, institutional, theoretic, practical) dimensions of scientific activity. The term "subarea of science" is intended to pick out the restricted character of the beginning of epochal changes. The restriction can refer to certain disciplines, theoretical or methodical aspects, objects of inquiry, or relations to other subsystems.[20] Epochal changes that affect the entire system of the sciences from the outset may be imaginable, but they are as yet unknown in the history of science.

I would like to discuss this part of the definition using the example of the *early modern epochal transformation*. It took its departure within a subarea, namely within certain physical disciplines (above all astronomy, mechanics and optics), which subsequently rose to become the very paradigm of scientific soundness.[21] Among the new elements incorporated in the concept of physical science were the transformed understandings of the relations obtaining between nature and technology, physics and mathematics, experience and theory, as well as the invention of the experimental method. While these new elements were only partially applied to concepts of science in other disciplines, the concept of physics, on the other hand, was still compelled to make reference to existing criteria, which stemmed from the classical conception of science and were valid for other disciplines as well. The specific nature of this mutual interaction is crucial for determining whether the transformation is of an epochal nature. Hence, referring to the restricted scope of the transformation which began in physics could lead to an argument against regarding it as epoch-making. Did the early modern transformation of physics not lead more to a dissolution of the systematic connectedness of the sciences than to an upheaval of the system of the sciences? One might recall in this context the early modern formation of dichotomies, for which the conception of the two cultures has been described as an ideal-type. But, contrary to this line of thought, one could object

[18]For an overview of the literature on the history of science concerning the early modern transformation, see Cohen (1985), Cohen (1994) and Shapin (1998). The genesis of modern science can be seen as part of an epochal change that also affected other societal subsystems – an assumption which can hardly be regarded as controversial either. Skalweit (1982) gives a presentation of this broader process that is still well-regarded today.

[19]I am borrowing this characteristic from Cohen (1994, 21), where it is applied to the concept of the scientific revolution in early modern times, in contrast to the concept of scientific revolutions introduced by T.S. Kuhn as a general structural feature of scientific development.

[20]These possibilities are intended to do justice to the aforementioned conceptions of a current epochal change as well to reconstructions of the early modern epochal change.

[21]That the early modern epochal change was initially restricted to certain subareas of physics is a view that has not until recently become established in the literature on the history of science. In the middle of the twentieth century, the influential studies by Butterfield (1949) and Hall (1954) assumed that the epochal change affected the entire system of the sciences from the very outset. For a critique of this view, see Cohen (1994, 121 ff.), and Shapin (1998, 80 ff.).

that the methods of disciplines that were similar to today's humanities also underwent a profound change in the wake of the early modern epochal transformation, and thereby remained integrated in the system of the sciences. In particular, the valorization of experience vis-à-vis theory, which was initiated by this transformation, also made its way into the concept of science in these other disciplines.

Epochal transformations presuppose the existence of a *system of the sciences* and lead to its re-orientation or vitiation. With the dissolution of the system of the sciences, as it is assumed in connection with the irreducible heterogeneity of the sciences in the conception of Mode-2 or post-normal science, the concept of an epochal change in the development of science itself runs up against a limit. But, as long as this is not the case, epochal changes in the development of science are distinct from fundamental *changes within a discipline or a group of disciplines*. The latter do not have the comprehensive character of the former. Although they can effectuate the abandonment of epistemological prerequisites and the introduction of new elementary assumptions, they can not force the identity of the entire movement to an end.[22] This identity, which is set out in the very concept of science, is precisely the object of epochal changes in the system of the sciences.

(b) The fact that epochal changes consist in the unfolding of the influence of one subarea upon other areas of science has consequences for the spectrum of possible dynamics of these changes. Much *longer periods of time* can be necessary for the spread of new conceptions throughout the system of the sciences than for the appearance of fundamental changes in a subarea. In particular, the progression of an epochal change need not be entirely discontinuous. I would therefore like to *avoid committing to a specific form of progression in formulating the concept of an epochal change*.

One also finds arguments in favor of this kind of openness in the aforementioned descriptions of recent epochal changes. They only claim a discontinuous appearance of new conceptions with respect to individual subareas, not to the preceding genesis of the conditions for new forms of knowledge. Since these processes cannot be distinguished clearly from the genesis of the new conceptions, it is advisable to incorporate their element of continuity in the concept of an epochal change. Another reason for including the gradual form of progression is the fact that the descriptions I have been discussing have yet to demonstrate a break in the transformation of the entire system of the sciences. Indeed, older forms of knowledge – such as Mode 1 or normal science – are integrated into the system and assure an element of continuity. Moreover, the transformation of the entire system has generally not advanced far enough that the form of its progression could conclusively be judged.[23] Against this backdrop, the use of the term "epochal break" appears problematic. It would only

[22]Blumenberg (1976, 16), and Footnote 19.

[23]That goes for the assessment of the epochal nature of a change, not just for its form of progression: Cf. the third part of the definition of the concept of an epochal change, which follows below.

be justified if the entire impact of an initiating event upon the system of the sciences were of a discontinuous nature.

Finally, the fact that one need not conceive of the progression of an epochal change as discontinuous is demonstrated by historians' reception of the early modern epochal transformation. In general, a discontinuous form of progression is not ascribed to the transformation of early modern physics or to its consequences for the other areas of science.[24]

(c) *For contemporaries, epochal changes in the development of the sciences might be observable only to a limited extent.* The concept refers to observations of individual events, which can only be attributed an epochal character once they have been brought into connection with a presumably comprehensive transformation.[25] Insofar as the epochal character depends upon the consequences of new conceptions upon the entire system of the sciences, it can only be evaluated once these consequences have reached a certain stage of development. If the epochal changes are spread out over a long period of time, it can be problematic for contemporaries to observe them. The transformation can proceed so slowly that its epochal character cannot be inferred in an unqualified sense.[26]

Epoch-making transformations in the production of scientific knowledge go hand in hand with observable structural changes, but also include the appearance of new patterns of interpretation, which evaluate states of affairs in novel ways and are incorporated in the description of the structural changes. This *normative element* makes its way into the conceptions under discussion as well. These conceptions ascribe great importance to the changes they describe and call for support – as the paradigmatic title "Re-Thinking Science" illustrates (Nowotny et al., 2001) – for the completion of the transformational process. Their descriptions, which are meant to refer to a desirable concept of science that so far only applies to certain branches of science, are understood as part of the transition (cf. loc. cit., 64, 168, 180, 184 and 192).

Hence, observers of epoch-making transformational processes not only bear witness to but are also potential creators of these processes. In order to do justice to the relations obtaining between descriptive and normative elements of the concept of an epochal change, it is advisable to include in the concept the *conditions for witnessing it*. A good point to set out from in this direction is I.B. Cohen's distinction

[24]Cohen (1994, 147 ff.), discusses the relationship between continuous and discontinuous elements; Shapin (1998) denies that the entire beginning of early modern science had a revolutionary character; Cohen (1985), on the other hand, ascribes just such a character to this episode in the history of science.

[25]The conditions for observability of a transformation include not only objective conditions that cannot be influenced but also subjective conditions. The latter are discussed in Nordmann (2008). The two, taken together, allow the observation of a transformation only when there is a suitable distance between the epistemic subject and its object.

[26]I think Blumenberg goes too far with his claim that there can in principle be no witnesses to such events since epochal changes proceed at a slow pace (Blumenberg, 1976, 20). But one must agree with him when he claims that an epochal change can have a discontinuous progression even if it proceeds too slowly to be observed.

of four types of observations of scientific events: 1. The "judgment of scientists and non-scientists" [of the period in question . . ., 2. the] examination of the later documentary history of the subject [. . ., 3.] the judgment of competent historians [. . .and 4.] the general opinions of working scientists in the field today (Cohen, 1985, 41 ff.). Cohen applies these types "quite generally to all of the more significant scientific events of the last four centuries", and thereby also to fundamental changes within a discipline as well as to changes that effect the entire system of the sciences (loc. cit. 40 f.). The latter kind of change is exemplified by the early modern scientific change (loc. cit. 77 ff.). He refers to his types as tests for assessing whether a fundamental change occurred in a discontinuous fashion. They can also be invoked to determine whether a given change is of an epochal nature. The presence of an epoch-making change should be corroborated by all four types. The absence of one of the types would call for special justification.

Applied to the claims of a current epochal transformation, the first and fourth type partly collapse into one another, while the second and third are only available in a limited sense. Regarding the third type, the judgment of competent historians, Cohen mentions only examples of presentations that appeared long after the relevant events (loc. cit. 43). But there is no reason why one could not also look at contemporary presentations. To a certain extent, current descriptions being offered by sociologists of science, which I would classify as belonging to type 1 or 4, overlap with historical studies.[27] In general, though, the question whether epoch-making changes in science are currently taking place is not a central topic in the literature on the history of science.[28]

In summary, we can hold on to certain features of the concept of an epoch-making change: it is a matter of a comprehensive, not necessarily discontinuous, transformation of science, which starts in a subarea of science and spreads from there. Epoch-making changes lead to new concepts of science. They must be attested to in various ways, and can only be evaluated satisfactorily when the interactions between the subarea and the entirety of science have sufficiently taken shape. Insofar as the phenomena invoked in current descriptions of epoch-making changes have not yet affected the entire system of the sciences, these claims take on a *hypothetical character*. The discontinuity-claim in these descriptions refers only to a subarea of science and can only be demonstrated for this subarea. In other words, current observers lack the requisite distance to be able to assess conclusively whether a discontinuous form of progression and an epoch-making character can be ascribed to a process comprehending the entire system of the sciences.

[27] Historically oriented arguments are given above all by P. Forman, as well as B. Latour and D. Haraway in their conceptions of technoscience.

[28] In the historiography of science, people do not speak as much of an epochal break in current science as they do of certain recent transformations in the historical description of science (e.g. the experimental, practical and cultural turns), cf. Hagner (2001).

Candidates for the Status of Epochal Transformations in the Recent Development of the Sciences

It is characteristic of the subareas of science to which the aforementioned claims of an epochal transformation refer that they *are correlated with other societal subsystems*. This commonality expresses the *orientation toward praxis* that is typical of the current transformational process in general. Mode-2 science is connected in the context of application to various societal subsystems (technology, industry, the state, the public, culture, etc.); post-normal science is policy-related research; in the Triple Helix model, the significance of the relationships obtaining among science, the state and industry is reflected in the title of the conception; Forman’s thesis places the relationships between science and technology at center-stage. Regarding the areas of physics from which the early modern epochal transformation took its departure, one can also establish the mark of an orientation toward practical contexts. Astronomy, mechanics and optics, for example, were closely tied to technical traditions of craftsmanship, which were of fundamental importance in developing experimental science. Although the transformational processes in science cannot be fully grasped simply by appealing to their relations to other societal subsystems, and although multifarious internal conditions also played a constitutive role, these relations are nevertheless *helpful guidelines* in investigating the possible epoch-making character of the current changes in science.

In order to make use of this orientational function, I would first like to clarify the extent to which the structure of societal subsystems is itself the object of a fundamental transformation. Do the traditional or *modern classifications* of these subsystems still present a suitable basis for describing the interaction of society and science? As I have already mentioned, the authors of Mode 2 believe that they can demonstrate “the erosion of modernity’s stable categorizations – states, markets and cultures” (Nowotny et al., 2001, 245). The context of application has, in their view, taken the place of a part of the previously existing structure of interactions between science and society. But they themselves are not fully able to make good on the claim of a dissolution of the demarcations. Science and society remain separate insofar as their transformation is described as a “co-evolutionary” process (loc. cit. 30 ff.). The state, the market and culture have not so much fundamentally declined as categories but instead have become invested with new definitions (loc. cit. 22 ff.).

Other conceptions of a recent epoch-making transformation appeal – in my view, rightly – to the categories of modernity in characterizing the changes they observe. That is obviously true of the Triple Helix, the postmodern primacy of technology and the knowledge society.[29] It is less obvious for post-normal science and for second modernity. The conception of post-normal science describes border infractions between science and neighboring subsystems, which bears a certain resemblance

[29]The conception of the knowledge society separates the production of knowledge from the areas of politics, economics, the media, the law and technology (Weingart et al., 2007, 13 ff.).

to Mode 2.[30] But it remains focused on a new concept of science that does not significantly affect the traditional definitions of technology, industry, politics and the public.[31] These definitions are still not given up by proponents of the second modernity either; rather, they lose their uniform character and are pluralized in ways depending on different discourses and decision procedures.

If one differentiates the current transformational processes in science according to the societal subsystems to which they relate, differences in the *respective historical origins* of the processes appear. In the following, I will be guided by an ideal-type schema, which takes up not only the changes addressed by the conceptions I have been discussing, but also changes not taken into consideration within these conceptions. My account groups the societal relations of science into the *areas of technology, industry, the state and the public*.

Science and Technology

The relationship between science and technology that is largely constitutive of today's concept of science can be traced back to the *early modern epochal transformation*. Among its essential achievements is the insight that technology, just like nature, can be made an object of scientific investigation. Looking at the ensuing relationship between science and technology, people have labeled these two societal subsystems twins.[32] The characterization of the current relations obtaining between science and technology as "technoscience" can also be traced back to early modern times.[33]

Forman's thesis, according to which technology has won primacy over science, does not have an epochal dimension insofar as it is limited to cultural interpretive patterns. It does however take on certain aspects of a transformed concept of science, which indeed can be regarded as aspects of *a possible future epochal transformation*. In Forman's view, science, given the primacy of technology, is no longer governed by the epistemological goal of truth or by methodological provisions, but by pragmatically established ends. To put it succinctly, truth becomes a means to technical ends. This kind of pragmatism has not been established within the currently dominant concept of science.[34] Furthermore, one must bear in mind that the

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[30] For example, Funtowicz and Ravetz (1990, 752 f.), cf. Elzinga (2004, 10).

[31] Cf. Funtowicz and Ravetz (1993), in which technology, culture and science are separated early on (loc. cit. 85); while science is distinguished from policy (loc. cit. 87 and 90 ff.) and professional consultancy (loc. cit. 96 ff.) and brought into relation with the public (loc. cit. 109 f.).

[32] Jacob (1997, 9), Layton (1971), cf. Wegenroth (2003, 230 and 244).

[33] Carrier (2008).

[34] In determining the current concept of science, one can refer to the types of observation sketched by I.B. Cohen (cf. section "The Concept of an Epochal Change in the Development of Science" above), above all to the judgment of scientists, including philosophers of science. Representative presentations that discuss the concept of science are offered by Bartels and Stöckler (2007), Schurz (2006) and Carrier (2006).

relation to technology only marginally determines the concept of science in some disciplines, such as literary theory, history and religious studies.

Science, the State and Industry

The formation of the current structural relations among science, the state and industry *began in the nineteenth century*. Scientific knowledge, at that time, was systematically built into large-scale industrial production-processes (above all chemistry and electrical engineering). The state founded the organization of the professional education of young scientific and technical researchers, and began the massive funding of experimental research. State institutions regulated the use of scientific and industrial technology. As I have argued elsewhere, the formation of the relations among science, the state and industry were closely tied to criticism and relativization of the meaning of the classical features of science. The classical conception lost its previous validity earlier than is supposed by current assertions of an epochal break.[35] In a nutshell, one could say that the real epistemic insight in science in the nineteenth century was the discovery that science can be socially quite useful even if epistemological questions, which had the highest priority in the classical conception, were left unanswered.

In my view, though, it is not yet possible to determine whether the transformational process in science, which goes hand in hand with the formation of relations among science, the state and industry, can be considered in its own right an epoch-making transformation of science. It appears not yet to be clear whether the orientation of science toward the realization of its potential social or economical utility might in fact be a *continuation of the early modern relationship between science and technology*. One point that speaks against this possibility is the connection that exists between the formation of the relations among the three subsystems and the criticism of the classical conception of science that was paradigmatic from antiquity until the nineteenth century. The loss of validity of the classical conception points back to an epoch-making dimension of the transformational process at work in science in the nineteenth century. It is worth asking, though, whether the consequences of this process upon the system of knowledge *reach all the way into the present* and therefore cannot yet be regarded as a completed development. Hence, some features of science that are demonstrated by the Triple Helix model, for example, can be understood as consequences of the relations that were brought about in the nineteenth century.[36] Moreover, the discussion over the classical conception has persisted into the present. Some tendencies of the debate suggest a renaissance of this conception of science – among them, for example, would be Positivism, which restricts scientific knowledge to observable phenomena, Pragmatism, which derives

[35] Cf. Schiemann (1995, 2008, 2009).

[36] See above, section "Triple Helix".

truth from the success of scientific theories, and Scientific Realism, according to which scientific knowledge gradually approaches the truth.

It would constitute a new, perhaps epoch-making constellation in the context of the relations among science, the state and industry, if one of the subsystems involved were to take over the *leadership* and if its boundary to science were to be vitiated. Some of the phenomena under discussion – such as privatization, commercialization, and commodification of knowledge production – suggest that the economic influence arising from industry could attain primacy.

Science and the Public Domain

The relationship between science and the public domain has come to the center of interest in recent years in disciplines reflecting upon science.[37] The current attention could have to do with a transformation that in particular Mode 2 describes. At the center of the new structures that are taking shape, in which scientific, technical, economic and political forces come together in various ways, the authors of Mode 2 place the so-called "*agora*". The agora is conceived as the space of an informed public, highly influenced by the media, which demands socially useful knowledge from science, and before which science presents and sometimes justifies its activities. The public domain and science not only act upon each other, but also face each other as different discourse systems.

In this constellation, which goes back to the second half of the twentieth century, we can perhaps see a re-organization or even a *reversal of the previous relationship between science and society*. Early modern science was initially an elitist endeavor, which was only accountable to itself (cf. the "House of Solomon" in Francis Bacon's "New Atlantis"). Through the formation of the relations obtaining among science, industry and the state, the institutional autonomy of science was restricted in the nineteenth century, but the definitional power which shielded it from external criticism was not. On the contrary, scientific knowledge enjoyed a great reputation.[38] It was not until the pluralization of knowledge in the twentieth century, as described in the theory of the second modernity, that the presently typical acknowledgment of the equal validity of various kinds of knowledge came about. Through this process, scientific methods and projects became disputable objects of public debate.

The discussion of the social utility of scientific knowledge that arose in this context took shape, as various authors have noted, in such a brief period that it does not

[37]For the history and philosophy of science, this widespread interest is reflected in the numerous entries on "science and the public domain" in relevant databases for journal articles. For the history of science, that would be, for example, "Eureka", organized by the History of Science Society, and for the philosophy of science, "The Philosopher's Index 1940–2007". For sociology of science, cf. Weingart (2005).

[38]Cf. Daum (1998).

seem far-fetched to speak of a discontinuity.[39] This beginning phase can be characterized with reference to the issues of depletion of natural resources (beginning the 1970s with the oil crisis and the movement against nuclear energy) and – closely connected with this – the destruction of the natural environment (especially in the context of the discussion of climate change since the 1980s).[40] The focal points of debates up until now reveal that *existential questions for humanity* constitute an important impulse for public interest in scientific knowledge. It is not only the hope of a solution to existing life-threatening problems, but also the fear that the application of scientific knowledge could threaten the foundations of human existence, that leads non-scientists to participate in the public discourse on science.[41]

It is indeed only since the previous century that scientific technology has the (epoch-making new) potential to threaten the continuance of human life at a global level. It has made possible planned and irreversible transformations of nature, which could to a large extent destroy the conditions of life on earth.[42] The paradigmatic example of the qualitatively higher-order means of intervention is the scientifically constructed potential for destruction by means of *military weaponry*, which could undermine the further existence of the human species with one stroke. Insofar as science is among the societal subsystems that have participated in the construction and the implementation of this potential for violence, the relationship between science and the public domain is still asymmetrical. Public discourse offers the opportunity to counteract this asymmetry.

In what way could public discourse contribute to a re-organization of the relationship between science and society, such that the concept of science would thereby be changed? Instead of discussing this far-reaching question here, I would like to limit myself to referring to two approaches to bringing to light the possibility of a transformation of our understanding of science by way of the public discourse. The first approach is presented by *participatory models*, in which the individuals affected by a line of research are involved in the production and application of relevant scientific knowledge. The participation of the affected individuals has an influence, in turn, upon the structure of the production and application of knowledge exactly when the participation is mediated by the public domain, as is paradigmatically illustrated by the formation of "recursive learning processes" in the so-called "real world experiments".[43] On the other hand, public discourse can contribute to the choice of goals for the application of scientific knowledge and to the transformation of the ethical attitudes of scientists. These potentialities of the relations between science and the public domain can be observed in the case of Janet Kourany's program of *socially*

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[39] Beck (1986, 254 ff.), Funtowitz and Ravetz (1993, 109 f. and 117).

[40] Nowotny et al. (2001, 15 ff.), Funtowicz and Ravetz (1993, 95 and 110 ff.).

[41] Cf. Office of Science and Technology and Wellcome Trust (2000), EU-Kommission (2001).

[42] From among the conceptions that I have been discussing, it is especially the second modernity that addresses the "irreversible endangerment of the life of plants, animals and humans" (Beck, 1986, 17).

[43] Groß et al. (2005).

engaged and responsible science.[44] In this context, public discourse is a forum in which non-epistemic values involved in knowledge are formulated, demands for the production of socially relevant knowledge are made, and scientists report on the utility of their results. In order to be efficacious, though, the relationship between the public domain and science would have to be incorporated in the institutional structure of science.

Conclusion

The epochal-break-thesis is based on verifiable, probably quite far-reaching, changes that have recently been occurring in the production of knowledge and in our understanding of what it means to be scientific – at a global level, but especially in the developed industrial countries. On the whole, there are enough phenomena to make it appear not implausible to think of a fundamental transformation, perhaps even of an epoch-making discontinuity in the development of science. Scientific objects, for example, have attained to new levels of complexity; they permeate ever more areas of life; on the other hand, science, by being subjected to economization and to public criticism, is losing the autonomous status that it has enjoyed since antiquity.

Nevertheless, there are substantial reasons that speak against the claims of a current epochal break. The prerequisite of my criticism is a more precise concept of an epoch-making transformation than is currently in use. The concept I propose takes up the relations obtaining between, on the one hand, the subareas from which new conceptualizations of science emerge, and on the other hand, the entirety of science, to which the concept of an epoch-making transformation is applied. It also incorporates the conditions of observability of transformational processes. Applied to the current changes at issue, it becomes apparent that some of them do indeed have an epoch-making character, but that they have historically earlier origins. Other changes are so recent that it is not yet possible to tell whether they have an epoch-making character. In part, the current changes involve discontinuous factors, but there are also opposing indices pointing to the far-reaching influence of continuous elements in the development of science.

Hence, it is difficult to attain a comprehensive overview of the situation. The convoluted, even contradictory, relations can at least be regarded as possible signs of a transformation of the whole system of the sciences. At present, there are different conjectures that can be made about the future development. I have grouped together some hypotheses about this development according to the relations between science and other subsystems. Roughly, the results can be summarized in two conclusions. *First*, the relations obtaining among science, technology, the state and industry can

[44] Kourany (2003) formulates this program, which refers to science as a whole, with the help of the example of philosophy of science. The public dimension is introduced through the reference to the feminist critique of science, which she develops throughout.

essentially be traced back to the nineteenth century or to earlier phases of modernity. Fundamental re-conceptualizations could come about in these contexts if one of the non-scientific subsystems were to take over a position of priority vis-à-vis science. Forman assumes that this has already taken place for technology. The conception of the Triple Helix addresses phenomena that suggest that an economic interest emerging from industry could assume primacy vis-à-vis science. *Secondly*, certain aspects of the current public discourse on science do not have a comparably early historical origin. Although the sciences have been an object in the public domain since the beginning of modern times, the ways of understanding science that have been formulated in this discourse since the twentieth century cannot be reduced to those origins. In their social orientation, the viewpoints presented in the public domain stand in contrast to economic interests. Hence, it seems that divergent directions are open to the further progression of the transformational process of science.

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Everything New Is Old Again: What Place Should Applied Science Have in the History of Science?

Ann Johnson

Much ink has been spilled over the past generation about the changing nature of science in the post-modern, post-industrial world. Scholars in many different fields have found that older expectations about the social and epistemological structures of science fail to explain the current economic concerns that some, perhaps many, feel dominate scientific activities in the twenty-first century. Some scholars have responded simply by decrying economic and other "external" influences as a corrupting force (Brown, 2000; Forman, 2007). Others have called for a new project to understand the situation as the new normal. Many have spent some time devoted to finding a new term that will capture the complexities and contradictions of late twentieth century science, generating a lexicon of neologisms from technoscience to Mode-2 science, from the triple helix to post-normal science (Etzkowitz, 2008; Latour, 1987; Nowotny et al., 2001; Ziman, 2000). It is not my goal here to undermine any of these accounts, but rather to challenge the hype afforded this allegedly new development. Instead of documenting the emergence of new values and motives in scientific work, I argue we would be better served by more carefully examining the history of science for similarities and the re-emergence of previously weakened values in science. Such an approach will allow us to learn from the changing nature of science over centuries, even millennia, rather than pick out changes that date back scarcely more than a lifetime.

I begin with the suggestion that rather than trying to understand the new categories of science or even the old modifiers such as pure and applied, we should consider *what counts as science*. I intentionally word this question in this way to avoid an ahistorical and idealized investigation of what science is. However, I do not intend this question to be overly focused on terminology, doing so would restrict my question temporally to the period when the words "science" and "scientist" (in English) were in everyday use. Many things count as science that are either not

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called science (e.g., natural philosophy) or that fail to meet the rigorous and narrow definitions of scholars; present day medicine and engineering come immediately to mind. But are the difference between medicine and physiology really greater than the differences between high-energy physics and sedimentology? These differentiations fail to be meaningful to practitioners and to practices quite quickly. But asking what counts as science contains another benefit over sticking with current definitions. The question, "What counts as science?" also implies a second perhaps even more contentious question, "By whom?" In other words, who gets to determine what counts as science and why various activities and bodies of knowledge might be counted or not counted at any given moment in history? If being scientific is a powerful designation, then the drawing of lines around what does or does not count as science constitutes a position of considerable power. So this socio-epistemological question must be included in the analysis of what counts as science. The answers to these questions are never simple, and there is obviously a spectrum of activities and bodies of knowledge some of which will be central and unarguable while others are marginal and contentious. But regardless of where an activity lies along the spectrum of what counts as science, these activities and bodies of knowledge should be fair game for historians, philosophers and other scholars of science studies. This approach has several advantages, not least that it will break the narrow hold that physics has had for quite some time as the most exemplary of the sciences, much to the frustration of those of us who study chemistry, biology, psychology, engineering, medicine, economics, and so on. Other advantages will be the generation of new interdisciplinary questions about how scientific activities work, how new scientific knowledge is produced and valorized, and how new technologies are designed.

Another way to focus on the question "what counts as science?" is to consider the attention that the phenomenon of technoscience or science in the context of application is getting today. Why does biotechnology or nanotechnology look like a new kind of science? Scientists engaged in these fields often violate the assumed underlying social values of scientists, as expressed by the sociologist Robert Merton. The so-called Mertonian Norms are communism, universalism, disinterestedness, and skepticism. Let's examine the claim of disinterestedness in its baldest form. Financial benefit should not pull research in one direction versus any other; research choices and findings should be driven by the search for truth. One might be skeptical of the claim that scientists used to be more may be motivated by the quest for truth rather than the promise of financial gain, but being skeptical of the reality doesn't imply any disbelief of this being a value of science, found in many textbook descriptions of what makes science as well as in countless popular biographies of scientists. The quest for truth is taken as a constitutive value of science; J.J. Carty, an electrical engineer and a founder of the Bell Telephone Labs, makes this claim in a report for the Smithsonian (Carty, 1917). The existence of financial motives, which may or may not align with the quest for truth, for scientists in the pharmaceutical industry, for example, is therefore taken a corrupting move. This is the most notable point in Henry Rowland's influential "Plea for Pure Science," written about 125 years prior to Paul Forman's (2007) decrying of the commercialization of university research in the postmodern era (Rowland, 1883). The worry about the corrupting influence of money is hardly new, but why does it attach only to the applied sciences? Do the applied sciences have particular vulnerabilities due to their structure? Perhaps, but aren't there myriad differences in the institutional arrangements of different scientific endeavors? Nanotech and biotech communities also look radically different from high-energy physics of the Cold War era in terms of their institutional and economic structures, yet they also bear an important resemblance to that which counted as science before the 1870s. Perhaps a more important question that emerges from the examination of science in the context of application is the peculiarity and short-lived superiority (roughly 1880–1980) of pure science? Why would science that values impracticality be socially valued – maybe the hierarchy that puts purity above application is more complicated than it looks? Is it so valued that other activities (industrial research, engineering) with scientific elements don't warrant attention from historians and philosophers of science? Looking at the canon between the 1950s and 1980s this conclusion seems valid. What did philosophers and historians miss while they had their blinders one?

It is my contention here that the apparent novelty of science in the context of application constitutes a commentary on the kinds of science that historians and philosophers of the mid-twentieth century privileged in the process of defining and describing science. These definitions became all the more important since the fields of history and philosophy of science were being professionally developed at the same time that they took up these questions. There were incentives for these scholars to agree on at least the appropriate subject matter for their disciplines. Clearly something akin to this demand for disciplinary agreement was at the core of the split between the history of science and the history of technology in the late 1950s (Staudenmaier, 1989). However, as understandable as the narrowing of the definition of science was, it came at a cost. Most obviously the cost was to cleave medicine and engineering from science; in both philosophy and history those who work on engineering and medicine belong to different, if friendly, scholarly communities. Instead of renewed efforts to differentiate science, medicine and engineering in ways that fail to map onto practitioners and the public's answers to what counts as science, a more productive set of questions can be extracted. How do these activities – sciences of all stripes, medicine, engineering, etc.–interrelate under the big umbrella of science? How do various practices and knowledge flow?

Furthermore, when the basis for defining science is drawn from a narrow view of what counted as science an incomplete or even distorted picture emerges. It is not the case that science requires a modifier such as pure or applied to be understandable. Such modifiers inherently generate questions of epistemological priority or hierarchy. Constructing such epistemologies reinforces highly idealistic accounts of scientific practices so as not to expose the tinkering and know-how inherent in even the purest of science. A similar lack of fidelity to actual scientific practice occurs in constructing engineering or medicine to lack any theoretical content; clearly there is significant abstract thinking about why questions in the activity of engineering, particularly obviously in the design of large structures (Mark, 1994). The construction of such idealizing categories creates the apparent novelty of science in the context of application by creating a world that never was and seeing that today's new sciences, whether nanotech or biotech, fit neither assumptions about technological science nor carefully structured arguments about theoretical physics.

In fact, pure science existed only in concert with science in the context of application. It was only those constructing abstract definitions of science, such as the logical positivists, who took the ideal of purity to be an expression of mainstream science. The special qualities of science in the context of application are only special if the observers' vantage point is limited. For example, financial incentives or motives are hardly new in science; the role of finances and, by extension, of patronage in the development of science is now a well-studied aspect of the history of science, particularly in the context of institutional histories of science (Biagioli, 1993; Kohler, 1991). The newly important economic motives and structures of science appear new only if a narrow band of largely twentieth century scientific activities (largely government funded) are taken as the norm. Even during the same period, many clearly scientific activities looked radically different from the pure science ideal that Paul Forman describes. While the twentieth century might be the golden age of pure science, it was also the age of the intensely organized industrial laboratory. Industrial labs supported a wide array of activities from theory construction to invention to product development and fine-tuning (Johnson, 2009). The industrial scientists did not necessarily see the differences between the practices and structures of industrial and pure science as deficiencies. Michael Dennis argues that the ideal of pure science was forged by an ever-present complementary industrial science, and in practice at places like Bell Labs, the two were inseparable, at times indistinguishable. Dennis, citing Leonard Reich's The Making of American Industrial Research, claims that at General Electric researchers might overlook patentable devices by seeing them only as laboratory apparatus. Such is not the attitude one might expect from a corporate employee driven by economic motives. Examples like this blur the distinctions between science as the quest for truth and science as utilitarian application.

Peter Dear is also concerned with the changing relationship between science's two faces. In *The Intelligibility of Nature* Dear also argues for a kind of complementarity between what he calls the natural philosophical dimension of science and science's instrumentality. Dear's point, however, is highly historicized; neither natural philosophy nor instrumentality has a fixed form. Dear draws his examples of the ways these qualities change from periods ranging over 300 years from the Scientific Revolution to the Quantum Revolution. Dear neatly points out that tension is created between these two faces of science because there needs to be no fixed, logical relationship between them. One can know how things work without knowing why (deductively or nomologically) – the steam engine is the shopworn example of this phenomenon. On the other hand, one can understand the laws underlying some bit of nature without being able to figure out how to use phenomena to serve desired ends – quantum mechanics is replete with examples of this. Furthermore one need not have accurate accounts of how things are to generate working devices; many devices have been created in face of theoretical misunderstandings of their principles. One sees a twist on Ian Hacking's distinction between representing and intervening in Dear's account. Dear's attitude is that natural philosophy and instrumentality are neither seamlessly fused as a term like technoscience implies, nor are they logically linked, as the word application implies. Instead they are entangled in a relationship like Schouten's staircase, the Necker cubes or Wittgenstein's Duckrabbit. Natural philosophy and instrumentality are in a complementary balance with each other, but seeing one aspect forces the other to recede – seeing both simultaneously is not possible, but the object (here, science) isn't changing, only the observers' perception of it. Dear describes a kind of deep complementarity while maintaining the flexibility for what counts as natural philosophy and what counts as instrumentality to change dynamically and independently.

The chief advantage of constructing a robust category of science which doesn't exclude or privilege abstraction nor application is that such an account allows one to examine the role of broadly held and fiercely defended values that shape what counts as science. One of the motives for looking at science in the context of application is to provide a platform for examining values; if this question is of interest in the twenty-first century it must also be of interest in the past. Asking what values are important in counting medicine as science, as is common practice among both the general public as well as practicing physicians, sheds important light on what qualities we believe today constitute science. Qualities like evidence-based, reliant on controlled research, and ensconced in an educational system are often used to support the notion that medicine counts as science. Other qualities, like mathematization and prediction, are not wielded in popular accounts of why medicine is scientific. Alternatively, it is precisely those qualities that are referred to in arguing that engineering is scientific. Peter Dear uses similar kinds of contingencies to show why Aristotelian cosmologies ceased to count as scientific in the sixteenth century and why Newton's mathematical account of gravity, which failed to meet Huygens' standards for natural philosophy, became exemplary by the seventeenth century (Dear, 2006).

A last important advantage to asking what counts as science is that a variety of activities, which has hitherto been unexamined, will fall into the category of science, according to the slippery, socially-constructed standards of their times. These activities are certainly culturally significant whether they count as science or not. But acknowledging their scientific nature puts their practitioners in proper perspective and shows the role science, historically defined, played in a variety of different eras. In service of this aspect of the argument, in what follows I will show how counting activities and knowledge which have previously been excluded from accounts of science broadens the kinds of questions science studies scholars might ask. The examples, therefore, provide comparisons for the novelty of twenty-first century science in the context of application.

Roman Engineering

One doesn't need to search very far to find accounts of Roman technology that discount it as science.[1] In James E. McClellan and Harold Dorn's excellent textbook titled *Science and Technology in World History* the following claims appear:

"While Roman engineering flourished, there was very little Roman science.. . .The Romans did not value, indeed they spurned, science, math, and Greek learning in general. . .. Rome itself produced no Roman scientist or natural philosopher of the first or even second rank" (Dorn and McClellan, 2006). This is not a belittling remark by McClellan and Dorn; their book includes as much technology as science. Nor is it a novel claim; references to Roman science in the history and philosophy of science are usually confined to Galen with the usual caveat that he should really be considered in the Greek tradition, so as not to be anomalous. Yet, the structures built by these Roman engineers show clear evidence of considerable abstract thinking. The Pantheon cannot simply be chalked up to successful tinkering; there's more than know-how at play in its design. In a computer-aided design study of the structural behavior of the Pantheon, Robert Mark tries to lay out what the Roman engineers must have known by reference to the design choices they made (Mark, 1994). Mark makes no claim that the master builder carried out any sort of mathematical modeling of the sort he did, but rather tries to make the case that they understood how the structure worked. Clearly there is know-how at work in the construction of monumental architecture, but Mark makes an argument for a more abstract understanding of nature, as he presents evidence that engineers examined previously constructed structures, particularly the Coliseum, to make design improvements in the Pantheon. Mark also examines the Hagia Sophia, where he points out the occupations of its master builders were given as *mechanopoioi*, those versed in mechanics, which following the Greeks has already been connected to geometry (Mark, 1994). The Hagia Sophia is clearly an exercise in geometry, and its scale alone defies any epistemologically trivial account of it as a trial and error construction. Perhaps this kind of knowledge and practice counted as science in Roman times, and it explains the absence of any Greek-style natural philosophy. It is possible that Greek-style natural philosophy, which Descartes also found lacking as science in the sixteenth century, didn't count as science to the Romans, whose accounts were based on the observation of function (i.e., through examining cracks) and geometric understanding of structural forms. Therefore, asking what counted as science may bring the whole of Roman engineering into the history and philosophy of science, where there has been

[1] Throughout this essay the word "science" is used admittedly anachronistically, but nowhere more so than in references to Ancient investigations, where the words scientia and ars do work that their English equivalents fail to. However, given the distinction I am try to efface between philosophical investigations and applications to use the more historically appropriate term here would be to muddy the waters and turn a philosophical discussion into a philological one. In fact, it is true that the question driving this essay only makes sense in the limited English usage of *science*; to ask what counted as, say, *Wissenschaft* or French *science* would be a different query indeed.

remarkably little written on it.[2] Contrary to McClellan and Dorn's claims, the Romans did not spurn science and mathematics at all, they redefined it. Their overshadowing by the Greeks in our current account of the Ancient world may be as much a historical contingency as the reintroduction of the Greek texts in the Renaissance, carried largely through the channels of Islamic scribes.

Enlightenment Navigational Science

Margaret Jacobs and Larry Stewart argue in their *Practical Matter: Newton's Science in the Service of Industry and Empire*, that contrary to conventional wisdom, Newton ought not be taken as a paragon of knowledge for knowledge's sake thinking (Jacob and Stewart, 2004). While most professional historians and philosophers are too familiar with Newton's alchemy to believe such a claim, it is still common to see activities outside those represented in the *Principia* and *Opticks* be represented as extracurricular. More important than seeing Newton as the origin point in some move toward applications of science in the eighteenth century, Jacobs and Stewart show the way that Enlightenment thought in England also influenced the question of what counted as science. Here, too, twentieth century accounts of what counted as science have been transcribed back on the eighteenth century. In their introduction to *The Sciences in Enlightened Europe*, William Clark, Jan Golinski and Simon Shaffer lament Lester Crocker's claim that "Science in the last analysis depends on discoveries from which theories are induced and the eighteenth century provided a small share of these" (Crocker, 1991; Clark et al. 1999). Clark et al., go on to challenge this view, discussing the "impoverished" state of the history of science in the Enlightenment; their book attempts to fill part of that lacuna.

Yet what counted as science in the Enlightenment? What might Crocker be overlooking, given his essentially positivist definition of science? If we look from the perspective of the British government, and take into account the crown-financed quest for a reliable measure for longitude, it seems obvious that navigation should count as science – it is useful knowledge based on observation and theoretical understanding of the movement of the heavens. Even though there was controversy at awarding the crown's prize money to inventor John Harrison for the chronometer, there should be no controversy that navigation is perhaps the signature scientific activity of the Age of Empire, which was fully underway in the eighteenth century. It is also a direct application of astronomy, and astronomical observations and charts were produced to serve the cause of navigation and surveying, thereby creating an activity of science in the context of application. The fact that these activities counted as science to practitioners is evident by a brief examination of the *Philosophical Transactions of the Royal Society*, which devoted some seventy pages to the a detailed account of the land survey of Pennsylvania and Maryland by the astronomer and geometer Charles Mason and Jeremiah Dixon. Mason and Dixon

[2] There is not a single reference to Rome in a search of the journal *Philosophy of Science.*

also published their account of the Sumatran observers of the Transit of Venus in 1761 in the *Philosophical Transactions*. The activities appear to have been of equal interest and acceptability as science by the Royal Society. Early nineteenth century letters by surveyors and navigators show some disagreement about what balance astronomical observations and chart making should obtain, but the disagreement is evidence of the fluid character of navigation as science, not as an argument to exclude any of its activities.

The Invention of Pure Science

In the twentieth century, though the development of professional communities of history and philosophy of science, the category of pure science is taken as the normative model of what should ought to be. But the category of pure science, or at least the usage of that term in the Anglo-American context is itself a historical development. The most famous, if not the first, incantation of it is in Henry Rowland's "Plea for Pure Science," which was first given as an address to the American Association for the Advancement of Science in 1883. Since 1883, those who wish to bolster the rhetoric of pure science have often misinterpreted this piece. Michael Dennis and David Hounshell have both put Rowland's address into its context. Dennis emphasizes the often-unread second half of the address that focuses on the construction of a financial structure to support research without any pretenses of utility (Dennis, 1987). This reading offers up a concern very close to the present concern about the distortional effects of commercialization in the university context. Rowland's concern was to justify investment in science without practical ends; to do so, he had to make an argument about the moral purity involved in the quest for truth. Rowland was not making any sort of claim that this was the sum total of science, or that pure science should somehow define science. In fact, there is ample evidence to the contrary that Rowland's own science often drifted toward the applied end. This is the point Hounshell makes in his reading, by showing the background of Rowland's position in a dispute Rowland had with Thomas Edison a few years earlier. Hounshell focuses more on Thomas Edison than Rowland and shows Edison to be the target of Rowland's vitriolic comments about the inventor as a cook who merely combines the ingredients others have struggled to produce (Hounshell, 1980). However, in the twentieth century, Rowland's Plea has been consistently misread as a description of what science is, and of the moral purity that must be at the core of the scientific endeavor. Taking Rowland at face value means that pure science always epistemologically trumps science in the context of application; pure science is epistemologically prior and more fundamental. But, in fact, Rowland is being normative in the Plea, and his normativity is focused on the proper structures for science (e.g., university-based, funded through disinterested endowment managed by the researchers). His claims about the proper structure for science are served by his epistemologically normative claims, not vice versa.

In a talk at the Society for the History of Technology meeting in Washington DC in 2007, historian Graeme Gooday presented a paper on Applied Science looking at the British analogues to Rowland (Gooday, 2007). Of these, the most famous and most famously misquoted is Thomas Huxley in his lecture, "Science and Culture." In this lecture Huxley closely mirrors the thesis of this paper, saying, "I often wish that this phrase 'applied science' had never been invented. For it suggests that there is a sort of scientific knowledge of direct practical use, which can be studied apart from another sort of scientific knowledge which is of no practical utility, and which is termed 'pure science.' There is no more complete fallacy than this" (Huxley, 1880). Huxley goes on to argue that a firm grasp of the fundamental principles of nature requires personal experience, what might be called "hands-on" today. Scholars in the twentieth century have taken Huxley to be a mouthpiece for the Pure to Applied linear model, but Gooday's more subtle reading of Huxley shows a more complicated, non-linear, even indistinguishable relationship between pure and applied science at work.

The Un-Doing of I. Bernard Cohen

In 1976 I. Bernard Cohen published a paper, likely in honor of the United States Bicentennial, that traced the side-by-side growth of science and the American Republic. Cohen had earned the first American PhD in the history of science and had played a key role in the creation of the professional discipline of history of science and the Harvard history of science department (Dauben et al., 2009). Cohen's bicentennial paper focused on two periods of side-by-side growth of American science and statecraft: the Revolutionary period and the period following World War II. He found little of interest in the nineteenth century, which was all the more disappointing given the promise of the late eighteenth century, led by Benjamin Franklin. Cohen would go on after 1976 to write two books featuring Benjamin Franklin, so clearly to Cohen Frankin was a figure central to the development of science in America. However, the reasons for Cohen's dismissal of, and at times even contempt for, the long nineteenth century are instructive about the effects of the narrow positivist definition of science. The first clue in the article is in a discussion about the different meanings of the word "experiment." Here Cohen argues that Franklin has a specifically scientific meaning for the idea of an experiment – an activity performed to test a hypothesis – in opposition to his political cronies who meant experimental in a decidedly un-scientific, Humean sense of trial and error, i.e., by experience. Franklin's use of the notion experiment counts as science to Cohen, the others do not. He writes, "These men were not thinking in scientific terms but rather using this word as synonymous with trial and error, which is not the method of science at all" (Cohen, 1976). He goes on to claim that the number of actual practicing scientists in America was too small even to establish a scientific community by the turn of the nineteenth century (Cohen, 1976). He also distinguishes sharply between pure and applied science, writing: "America was merely drawing on the accumulated scientific resources of Europe without adding anything of her own save the uses to which existing knowledge could be put" (Cohen, 1976). It is easy to read Cohen as a true believer in a clear distinction between scientific knowledge and its seemingly unproblematic applications, and in an epistemological hierarchy in which abstract, unapplied knowledge is on a higher plain. He seems to espouse exactly the moral position that Rowland advocates without the attention to institutional function and structure that motivates Rowland. Cohen has clearly missed out on the flurry of scientific activity taking place in America between 1790 and 1880. But antebellum scientific activity took a form that counted to its practitioners and their patrons as science, yet like Roman engineering, Enlightenment navigation, and Edison's industrial research fails to count as science by twentieth century definitions.

Instead of worrying about a well-known scientist like Franklin, consider, for example, the figure of Jared Mansfield. Mansfield was the first Anglo-American-born mathematician to publish original mathematical research in the United States. He considered himself a scientist and was in almost constant correspondence with Europeans (who would rate as scientists to Cohen). He sent off to London for the finest mathematical instruments and books. He was the first professor of mathematics at the United States Military Academy at West Point, an institution of higher learning opened in 1802 and directly modeled on the Ecole Polytechnique. But Mansfield was also the second Surveyor General of the United States, and spent years in the field laying out the rectangular survey and overseeing the settlement of the Ohio and Indiana Territories (Linklater, 2002). Cohen's standards would not count Mansfield's activities nor the community he participated in as scientific; by his own definitions Mansfield did, as did Thomas Jefferson and Albert Gallatin, Secretary of the Treasury for Mansfield's stint as Surveyor General. The consequence of Cohen's narrow account of American science is that he missed a major causal factor in the rise of America as a scientific power in the twentieth century – the existence and ubiquitousness of everyday science. Science was part of the fabric of everyday life, on the farm, in making things, and in numerate thinking. So many important twentieth century scientists emerged from small towns and rural communities. This background created scientific literacy; it did not undermine it. Cohen has misunderstood what counted as science in antebellum America. The irony of this misunderstanding is that Cohen's first book was titled *Science, Servant of Man: A Layman's Primer for the Age of Science*. This book makes the argument I am making here, that instrumentality can be difficult to distinguish from natural philosophy. The argument in *Science, Servant of Man* lacks Peter Dear's sophistication about the tense, but dynamic, relationship between natural philosophy and instrumentality, but Cohen does not dismiss instrumentality as he tends to by 1976, writing, "The writing of this book was undertaken because it deals with what I consider to be one of the most important problems of our age: the relation of scientific discovery to our daily lives, and to our well-being and national security" (Cohen, 1948). So what can explain Cohen's amnesia between 1948 and 1976? This is precisely the period in which professional historians and philosophers of science shaped the narrow definition of science, privileging the pure and dismissing application as trial and error and nothing new and, worse, began to wield it ahistorically.

Conclusion

In this article, I have tried to show that the phenomenon of science in the context of application appears novel, in large part, due to the exclusionary way science was defined by twentieth century philosophers and historians. Given this account of science, which privileged theory, and cleaved off engineering and medicine, it should not be surprising that the appearance of a new phenomenon may be more a feature of shifting one's gaze than the emergence of a truly new kind of science.[3] If this is the case, then opening up the definition of what counts as science to ask historically what counted as science and who had the power to say so then reinserts a role for social values in the examination of science. Given that this is a goal of those wishing to investigate the seemingly new phenomenon of science in the context of application, I arrive at the same agenda through different means. In the end, I think the history of science looks different, and more importantly includes a different population when the question is asked as I suggest it should be (Connor, 2005). But some might also ask why bother? Why make the case that Roman engineering or Enlightenment navigation is, in fact, science? Isn't the Pantheon equally magnificent whether it is called science or not? Doesn't navigation work equally effectively, even if it doesn't count as science? Perhaps this is true, but I think such a stance is disingenuous to practitioners who defined their work as scientific or whatever the term of art at the time was. In the end, it comes back to Peter Dear's argument that we must trace the reasons why something counted as science or not. But this question requires an inclusive, big tent approach to science.

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[3] I would not argue against the emergence of new sciences, just against the emergence of a new type.

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Science in the Context of Technology

Alfred Nordmann

Despite its ambitious title, this is a very small contribution to a very big theme. The big theme is the "Philosophy of Technoscience" – what it is, what it finds, and why it is needed. Even though most agree that every field of research calls for investigations of its particular history and specific methodology, there is pervasive agreement also that these investigations can sensibly join together under the heading "philosophy of science." The call for a philosophy of technoscience shoulders a considerable burden of proof. It must show that investigations of the special sciences and technosciences can sensibly join together also under this heading. In order to show this, it must establish, by using examples but also in a principled manner, that there is a meaningful difference between "science" and "technoscience" such that the philosophy of technoscience brings fruitful questions of its own to the various particular fields of research.

Some consider it a problem of classification whether there is a meaningful difference between science and technoscience. They require a set of criteria by which entire fields of research or specific activities can be classified as either scientific or technoscientific. Should unambiguous allocation of one activity to science and another to technoscience turn out to be a difficult task, this would suggest that there was no meaningful difference to start with.[1] But the problem is not necessarily one of classification but can also be viewed as one of interpretation. In this case, the very idea of "science" serves to guide and orient research activities, and "technoscience" then provides another kind of orientation, even if the particular laboratory practices

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The following reflections benefited from numerous discussions and conversations during my fellowship with the research group "Science in the Context of Application" (Center for Interdisciplinary Studies, University of Bielefeld)

[1] A variant to this approach is being pursued by Bernadette Bensaude-Vincent, Astrid Schwarz and the author of this chapter – to classify not researchers, research fields or activities but to investigate the different objects of research, their genesis and ontology.

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look the same in both cases. Moreover, in this case "science" and the "philosophy of science" provide an opportunity for researchers to reflect in a particular way on their own work and its place in history. Perhaps, "technoscience" and the "philosophy of technoscience" offer much-needed novel opportunities for researchers to reflect on their own work and its place in the social order. These novel opportunities are much-needed today not because the research activities themselves have changed in a drastic way but simply because self-reflection in terms of "science" does not work as well as it used to.

It is here that the small contribution comes in. It concerns "science" and "technoscience" as interpretive concepts and whether they can be associated with historical epochs or eras. Might it be that "science" has served the self-reflection of natural philosophers and scientists in the age of Enlightenment roughly from the late-18th to the mid-twentieth centuries, and might it be that contemporary research since, say, the 1990s is better served by the notion "technoscience"? In particular, what arguments would be sufficient to establish that we are now living in an age of technoscience? These questions and their answers make a contribution to the philosophy of technoscience mostly in that they underscore its cultural significance. They leave entirely unaffected, however, the main business of the philosophy of technoscience, namely its epistemological questions regarding technoscientific knowledge and objectivity or its ontological questions regarding the constitution and character of technoscientific research objects.

Indeed, some might argue that it is misleading even to contemplate the question of an epochal break that separates the age of technoscience from an age of science. The term "technoscience" was introduced by Gilbert Hottois and popularized by Bruno Latour and Donna Haraway to refer quite generally to knowledge-production in a technological *milieu*, that is, to the technical context of instruments and experiments and to the technical accomplishment of controlling and predicting phenomena (Haraway, 1997; Hottois, 1984; Latour, 1987).[2] Despite the differences between

[2]For the history of "technoscience" as an interpretive concept see Bensaude-Vincent, 2009 and Ihde and Selinger, 2003. Among philosophers of science, the usage of the term "technoscience" has suffered from its affiliation especially with Bruno Latour and Donna Haraway. In recent years, however, the term has diffused so far that it can no longer be associated with particular intellectual traditions. Reticence to adopt this term is founded on a particular suspicion: "Technoscience" implies a dissociation from an idealized "science" and thereby casts doubt on the pertinence of the values associated with science – the values associated with Enlightenment, Mertonian norms, critical rationalism, disinterested truth-seeking, theoretical representation of how things really are etc. The suspicion is that the choice of "technoscience" as the more appropriate interpretive concept amounts to its endorsement and thereby also to a triumphant rejection of the values of science that are now exposed as being obsolete. (This suspicion has been articulated especially by Elzinga, 2004.) However, rather than celebrate a postmodern age of technoscience, the philosophy of technoscience attempts to understand what kind of knowledge can be produced and validated within a technoscientific research culture. It aims to articulate the epistemic and social values that characterize knowledge production once the orientation towards the values of "science" fades away. It thereby produces a notion of "technoscience" that is no less idealized and mythical than that of "science" – and it may well do so with a sense of what is lost and ought to be recovered from the history of science as a social institution of public criticism and Enlightenment.

these authors, none of them suggests that the technosciences are new or that we have recently moved into an age of technoscience. Instead, they urge that it is important to consider "science and technology" not as it reflects upon itself with the help of the philosophy of science but as technoscience. The following pages add to this only the question why Hottois, Latour, and Haraway consider it so urgent or appropriate now to shift from the perspective of the philosophy of science to that of the philosophy of technoscience. Apparently, there is something about our current situation that prompts these three and many others to call into question a powerful set of ideas according to which all the diverse sciences are dedicated to the search for truth, that this search advances general Enlightenment, and that the sciences are therefore characterized by a "critical" attitude not just towards their own hypotheses but also towards the presuppositions that inform policies and social debate. By calling these ideas into question, the theorists of technoscience proclaim that there was a time when these ideas had considerable traction, even where the practice of science did not live up to them. They also proclaim that this time is gone and that another set of ideas has effectively displaced them.

What arguments would be sufficient to establish that we are now living in an age of technoscience? A brief clarification of what is and what is not meant by "technoscience" will be followed by general historiographic considerations of what it takes to argue for an epochal break of any kind. After specifying what kind of argument might be required, I will highlight two strategies by which the philosophy of technoscience can make a case for the age of technoscience.

What Is the Meaning of Technoscience?

Without reconstructing in detail how the term has been used by Gilbert Hottois, Bruno Latour, Donna Haraway, Raphael Sassower, Don Ihde, Bernadette Bensaude-Vincent, and many others, it is possible to identify a few defining features of technoscience and to say accordingly what it is not: It is not applied science, engineering, or engineering science. And it is not commercialized or entrepreneurial science or science that is done for the sake of utility rather than curiosity. "Technoscience" is not a disciplinary label that picks out a subset of the sciences, nor is it "science" formed or deformed by extraneous intentions, interests, or application pressures.

As mentioned above, Gilbert Hottois coined the term in 1984 and used it to refer to science that is done in a technological setting or *milieu* and that is technology-driven (Hottois, 1984). He thus uses the term technoscience much like one uses the word technomusic where the sounds cannot be separated – as in a musical score – from the technological context in which they were produced. Along these lines, Bruno Latour introduced the term initially as shorthand for and fusion of "science-and-technology," that is, as a technology/science hybrid where the two cannot be separated out from one another in terms of basic and applied research (Latour, 1987).

According to these definitions "technoscience" is an alternative to "science and technology" with its assumption of two distinct but related spheres. Hottois, Latour, and other philosophers of technoscience do not presuppose, however, that science and technology were ever actually distinct – indeed, they are always bound up with one another. They observe instead that philosophers and scientists have invested a lot of intellectual effort into separating them out – to put science here and technology there, nature here and culture there, representation of a given world here and intervention into the lifeworld there, pure theoretical knowledge here and impure social utility there. The conceptual work of separating out these interrelated domains has been called a work of purification. While this work was more or less successful during the age of science and technology, one might say that we encounter technoscience when this work of purification is abandoned because it proves impossible or unnecessary.[3]

Picking up on the first half of this definition, one can define technoscience as a kind of research where theoretical representation and technical intervention cannot be held apart even in thought. In the case of laboratory experiments this means that scientists look at an experiment and distinguish their own contribution from that of nature: the laboratory scientists provide hypotheses, instrumental apparatus, and an experimental set-up, nature then steps in to produce the observed phenomenon or effect. While it is possible to maintain this distinction in many cases, it is sometimes extremely difficult, if not impossible, because the observed phenomena and effects also appear to be engineered – when one studies the "natural" behaviour of genetically engineered organisms, for example. This difficulty might serve as a criterion to distinguish technoscience from science.

According to the second half of the previous definition, we encounter technoscience when the work of purification is not pursued because it appears unnecessary. Accordingly one can define it in terms of "ontological indifference"[4] : technoscientific research is that kind of research that has no need to distinguish between the contributions of nature and of technology in the creation of a phenomenon. If the purpose of research is to determine what is and or isn't really the case, it is crucial to know what is the case independently of what humans think and do. If the purpose of research is to show what can be done, it usually makes little sense even to juxtapose natural agency and human intervention. Showing what can be done characterizes not only the engineering sciences but more generally "an engineering way of being in science" or "research in a design mode" (Galison, 2006, Ann Johnson

[3]Modern philosophy of science from Kant, Hertz, Mach, Poincaré, and Wittgenstein via the Vienna Circle to contemporary philosophers like Michael Dickson defines good science in terms of critical awareness of the ways in which its formalisms – broadly conceived to include concepts and experimental methodology – structure and shape the phenomena. According to Dickson, for example, a good theory is formulated in such a way that its formal apparatus transparently delineates its empirical content (Dickson, 2006). Technoscience designates a technologically complex condition where this critical awareness cannot be achieved or where successful research does not require it.

[4]The term has been suggested by Peter Galison to characterize a kind of physics that is interested in making and building rather than understanding the hierarchical composition of material reality (Galison, 2006).

in conversation, Nordmann, forthcoming). These designations might therefore serve as equivalents to "technoscience."[5]

Epochal Break Arguments

The previous suggestions of how to distinguish science and technoscience do not imply that the technosciences are a recent phenomenon that somehow supersedes the sciences. On the contrary, they have coexisted and continue to coexist: science then and now includes cosmology, evolutionary biology, physiology, game theory, and technoscience then and now encompasses pharmacy, synthetic chemistry, medical and agricultural research, and nanotechnology. Moreover, in physics, chemistry, molecular biology, computer science, some of the research can be considered scientific, other endeavours are properly described as technoscientific. This contemporaneity of science and technoscience is generally acknowledged by placing Francis Bacon as a "founding father" of technoscience side by side with Galileo, Newton, or Lavoisier as patron saints of science. Thomas Kuhn relates this to the parallel development of "Mathematical versus Experimental Traditions in Western Science" and noted also the intellectual prestige and dominance in many fields of the mathematical tradition in the nineteenth and twentieth centuries – mathematization became the hallmark of the higher ranking pure and fundamental sciences as opposed to the applied sciences (Kuhn, 1977, 61).

Despite the contemporaneity of the theoretical sciences in the mathematical tradition and the creative technosciences in the experimental tradition it is possible to posit a momentous epochal break on the basis of Kuhn's account: If the dominance of the mathematical tradition characterized an age of science, the experimental tradition has gained, perhaps regained the upper hand in the current age of technoscience. But in order to actually establish such a claim, historiographic and methodological considerations are required to clarify what an epochal break is and how one can argue for it. These considerations cannot be provided here, since they call for systematic reflection on the ways to discern and distinguish historical continuity and discontinuity. For present purposes, two central propositions must suffice.

First, *the interest in epochal breaks is not at all self-evident but characteristic of a modern conception of history*. As Hans Blumenberg has pointed out, there would be no modern world without the assumption of an epochal break, namely the one that separates it from the dark ages (Blumenberg, 1983). Of course, the historiography of modernity is full of uncertainty and controversy about the precise time and place, the extent and significance of the break between the medieval period and the

[5] Even without referring to "entrepreneurial" or "venture science" (Etzkowitz, 2003; Rajan, 2006), post-academic or post-normal science (Funtowicz and Ravetz, 1990; Ziman, 2000), let alone "mode-2 research" (Gibbons et al., 1994; Nowotny et al., 2001), this brief discussion offered too many and not too few definitions of technoscience. It is one of the tasks of a philosophy of technoscience to sort through these various determinations and to evaluate whether and how they mutually support one another.

modern world. To be modern is nevertheless to frame one's own place in the world historically, part of a movement from one era to the next, each with its own character and destiny. In other words, to be modern is to distinguish oneself, to acknowledge the significance of one's age, and to answer the call of the day. And even as the moderns remained profoundly unsure how they could and should distinguish themselves, they liberally proclaimed epochal breaks, most prominently perhaps in the philosophies of Comte, Hegel, or Marx. A certain conception of history with its eras or epochs served as an instrument of the moderns to reflect upon and interpret themselves, their place in history and thus on their responsibility. There is no compulsion from facts or principle that would force anyone to see an epochal break here or there; but to see an epochal break is tantamount to accepting a historical mission, and this is what moderns do.[6]

Blumenberg's insight has an important implication for the epochal break under consideration. Especially Paul Forman pointed out that the transition from the age of science to the age of technoscience coincided with the transition from modernism to postmodernism (Forman, 2007).[7] If one takes this observation seriously, one may find that the age of science was wedded to the modern conception of history with its interest in epochal breaks and the vocation or historical calling of the scientist. Accordingly, to be a scientist was to accept a historical mission which has been described as an unending quest for the unachievable, yet guiding ideal of truth (Popper, 1976; Weber, 1946). In light of Kant's dictum that we do not live in an enlightened age but in an age of enlightenment (Kant, 1983, 44), it becomes apparent that this historical mission served as the common bond between science and the Enlightenment. In the postmodernist age of technoscience the historical calling of the scientist has lost its rank and role. For technoscience, the business of research has always consisted in the discovery, technical and intellectual control of new phenomena and in the realization of technical possibilities. To be sure and as Bacon demanded, much of this is for the achievement of social benefits, but what these benefits are owes exclusively to current needs and demands – the cure for cancer, the construction of humanoid robots, or the reduction of CO_2 emissions.

[6]The theorist of technoscience Bruno Latour has argued that we have never been modern (Latour, 1993). His claim does not contradict Blumenberg's but complements it: Modernity presupposes that one can distinguish the modern self from that of the dark ages, that one can distinguish culture from nature, science from technology, this era from another. According to Latour, since we have never quite succeeded in establishing and fortifying these distinctions, we have never been modern. And yet, it is characteristically modern to engage in such work of purification, that is, in the work of distinguishing oneself, of attributing blame either to nature or to human intervention, etc. And this is precisely Blumenberg's point.

[7]To be sure, Forman does not use the term "technoscience" to characterize this break. Instead, he speaks of the transition from an age of modernism in which technology is subsidiary to science to an age of postmodernism in which science is subsidiary to technology. In contrast to Forman, I would maintain that the coincidence of the shift from modernism to postmodernism and the shift from an age of science to an age of technoscience is part of the phenomenon under considerations but that the one shift does not explain the other: It is not postmodernism's "fault" that science has surrendered its rank and role in respect to technology.

The technosciences expand the pool of technical possibilities and in a piecemeal manner select from among these possibilities those that address current concerns. Attempting to solve the problems of the current world, the technosciences do not take their "problems" from the remaining gaps in the overarching quest to reach a more perfect theoretical understanding of the world. Due to these different conceptions of history and of the mission of science and of technoscience, the epochal break in question may thus be visible only from one side of the threshold. From the point of view of the age of science, nothing could be more momentous than the transition to an age of technoscience with its apparent abandonment of the human project of general enlightenment. From the perspective of the age of technoscience, in contrast, there is just technoscientific business as usual since research always served to find solutions to the currently pressing practical problems.[8]

Second, *scientific revolutions and paradigm shifts take place in the special sciences and thus within the traditions of science and technoscience*; in order to see an epochal break between the age of science and the age of technoscience, one needs to attend to another level of analysis, namely that of the scientific enterprise and the technoscientific regime. Each scientific discipline may have its own paradigm and within each discipline there might be scientific revolutions that involve paradigm shifts. In and of themselves, however, these do not constitute epochal breaks. Accordingly, Blumenberg characterizes paradigm shifts as "a surrender of basic assumptions and the introduction of new elementary suppositions, which get rid of a desperate situation but do not necessarily rupture the identity of the movement of knowledge that had culminated in that situation" (Blumenberg, 1983, 16). Another name for the overarching movement of knowledge that leads from one paradigm through some impasse to another is "scientific enterprise." It refers to a general movement towards truth which relies on the capacity of the various sciences to distinguish what really is from how things appear to us in the course of conducting our experiments and acting in the world. Analytically, the term "scientific enterprise" is on a par with terms like "modernity" or "the Enlightenment project." As with modernity and the Enlightenment, one might have a hard time knowing just when and where it began and whether it ended, and still remain confident that the scientific enterprise did not exist everywhere at all times. It is the name for a common project that orients the various sciences and influences their self-definition. And it suggests that, separately or together, all the different ways of knowledge production contribute to a historical process that, citing Max Weber, might be referred to as a process of rationalizing or intellectualizing the world (Weber, 1946).

The age of science is characterized by a commitment to the scientific enterprise, and the supposed epochal break would thus consist in its profound transformation or

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[8]This can explain why some philosophers of science see this break and others do not. Those who see it share a somewhat anachronistic affection for modernist conceptions of science as expressed, for example, by the Vienna Circle, Popper, Kuhn, and Lakatos, including their interest in unification programs, rational theory choice, and the like. Those who don't see it view scientific and technoscientific research as a kind of practical and well as conceptual tinkering that is required for the specification of mechanisms or for establishing a local fit between theory, model, and phenomenon.

displacement by, say, a technoscientific regime.[9] The scientific enterprise orients the sciences and technosciences of the day towards ideals of truth and understanding but allows for scientific practice to produce along the way many useful things that are, so to speak, exported into the context of application. In contrast, the technoscientific regime seeks to apply not this or that result of scientific research but co-opts the sciences and the scientists as a whole: It draws the scientists with their skills, laboratories, toolsets of theories and methods into the context of application or context of technology. Accordingly, the shift from an age of science to an age of technoscience is something quite different from a revolution within science. There are many different sciences, after all, too different to easily accommodate the singular "science." Even if "science" is restricted to the natural sciences, there are physics, chemistry and geography, then there are within physics cosmology and solid state physics, and then there are new formations like molecular biology and bioinformatics. While all these are different sciences, the "scientific enterprise" and the "technoscientific regime" designate the larger frameworks within which these various sciences or technosciences become meaningful and do their work. These notions thus offer what one might call the proper distance from which it is possible to see the epochal change in question. Laboratory studies with their ethnographic methods look too closely at the different sciences and technosciences, and some philosophers of science are assuming a detached vantage point from which they observe strategies of explanation and modelling in general. In contrast, the scientific enterprise appears as the particular historical project to which all the sciences and the technosciences, no matter how different they are, relate themselves if only by labelling themselves as sciences and claiming a home in the academy. Similarly, within the technoscientific regime all the sciences and technosciences respond to the demand for economic, social, and technical innovation. The notions of "scientific enterprise" and "technoscientific regime" thus serve as middle-terms between, on the one hand, the many particular fields of research, each with their own conceptions of science, method and objectivity, and on the other hand, the most general epistemological notions of how humans forge an agreement between their thoughts and the real world.

In summary, then, in order to show that an epochal break separates our current age of technoscience from the previous of age of science one needs to recapture *as a thing of the past* the notion of the scientific enterprise with its modernist conception of history and its close affiliation with the project of Enlightenment. However, this is not an imaginative exercise alone but shoulders a burden of historical proof: it requires that one can show how the notion of a scientific enterprise actually used to orient the sciences and technosciences in the past and that it has ceased to do so.

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[9] I use the term "scientific enterprise" in its ordinary sense. I propose "technoscientific regime" as a contrasting term largely because "regime" emphasizes a manner of organizing things in space rather than historical time: the technoscientific regime governs the search for innovative ways of fitting technical and scientific capabilities to particular societal needs and as such for local solutions that can become templates for global action (compare Nordmann, 2008).

Epochal Break and Philosophy of Technoscience

There are at least two strategies for recapturing an image of the past and of confronting it with new realities. We can hold two images side by side and find them incommensurable, judging that any transition between them would involve a profound break with the past. We can also follow the movement from one state to the next and find, for example, that one's own age was enabled by some novel question or technique which irrevocably changed the rules of the game, closing off for good any return to the good old days in which that novel question or technique originated. Both strategies have been employed to illustrate profound changes in the general orientation of research. And both strategies challenge the philosophy of technoscience.

Juxtapositions

The first strategy follows the paradigm of Kuhnian paradigm-shifts by providing detailed reconstructions of two ways of engaging in research. Hans Blumenberg characterizes it as a symmetric comparison of systems or worlds. Here is one world t_1, and there is another world t_2. One contrasts and compares them and finds them so different that one cannot see the second as merely an extension or further development of the first. This is because things have different meanings in the two worlds. In the words of Blumenberg, by placing them side by side "it soon becomes evident that they cannot have existed side by side" (1983, 31). Their succession, however, appears merely contingent: with this strategy of establishing an epochal break it is impossible to appreciate how one image gives rise to the other and how the age of technoscience originated in the age of science.

The suggestive appeal and the limits of this first strategy can be illustrated by an attempt to contrast scientific and technoscientific cultures of research (Nordmann, 2004a).[10] A "culture of research" was defined in terms of three mutually supporting dimensions: a logical or methodological orientation, a corresponding ethos, and its stabilization through social norms. This set the stage for the intended contrast that culminated in the following seven propositions where each offers a technoscientific inversion of a feature of the scientific enterprise:

[10] Throughout this section I draw on examples from my own work; they are examples of the difficulties encountered in the course of a sustained effort to establish the epochal break in question. – In the case at hand, a second example might have been derived from a paper that considers the role of concepts and theory in the deliberation of novel effects (Nordmann, 2004b). Here, it was shown that incommensurable approaches did not prompt controversy or debate in the course of technoscientific research in molecular electronics. The employment of different concepts and theories was ignored for the sake of the shared interest in improving control and performance of an experimental system. This appears striking in contrast to conceptions of controversy and theory-development in the sciences.

1. Instead of a commitment in the scientific enterprise to the conceptual and technical distinction of representing and intervening (and thus of science and technology), the age of technoscience is not interested to separate out the theoretical representation of nature and the technical intervention into nature.
2. Instead of producing hypothetically formulated theoretical representations of nature, the age of technoscience sets out to reshape the world as a hybrid of nature and culture.
3. Instead of valorizing quantitative predictions that are highly falsifiable, it is sufficient in the age of technoscience to determine structural patterns in data and to seek qualitative agreements.
4. Instead of articulating lawful regularities or detailing causal mechanisms, research in the age of technoscience is interested in exploring powerful and potentially useful processes and properties.
5. Instead of tending to anomalies and problems as defined by theory, technoscientific research agendas are dedicated to the acquisition of new capabilities – the problems they solve are milestones towards the achievement of a technical goal.
6. Instead of ranked within a hierarchy of nature (from elementary particles to society) and the sciences (from physics to sociology), research activities in the age of technoscience coalesce around transdisciplinary models, methods, and objects.
7. Instead of distinguishing scientific knowledge of lawful regularities and the (technical) construction of artefacts, the technoscientific regime programmatically equates knowing and making, physical possibility and technical feasibility.[11]

For all its imprecision, this dramatic juxtaposition serves not only to indicate the profound difference between two cultures of research or between the values that characterize the ages of science and technoscience. It also challenges the philosophy of science and technoscience to provide rational reconstructions of the two cultures of research. In the case of the scientific enterprise, Karl Popper and Robert Merton led the way to provide a coherent reconstruction that shows how such an idealized, even mythical conception of science can coordinate the research efforts in many disciplines, and how it is reflected in codified styles of writing and experimentation.

[11]This list can be continued and has been continued, to be sure. Referring to the four Mertonian norms, for example, the paper "What is Technoscience?" continues as follows: Instead of engaging in organized scepticism, technoscientific research adapts the available tool-set of theory to given phenomena and processes. – Instead of maintaining a scientific community of equals (universalism), technoscientific knowledge production involves the collaboration of numerous, unequally situated social actors. – Instead of shared intellectual property (communism), technoscience requires the circulation of products between instrument-builders and laboratories within the triple helix of academia, industry, government. – Instead of disinterestedness and a commitment to truth as the only legitimate interest, technoscience looks neither for better theories nor merely for working devices but acquires and demonstrates basic capabilities. – Ziman (2000), Rajan (2006), and Radder (2010) also revisit the Mertonian ethos of science to illustrate how things have changed.

The current philosophical and sociological literature on technoscience abounds with first attempts to show how the ideals of technoscience also coordinate a great variety of research practices.

Spaces of Possibility

The second strategy follows the movement from one age to the next: it begins in the age of science and follows its progress continuously through time, but more or less suddenly one finds oneself in the very different situation of the age of technoscience. Even on the assumption of continuous development, even allowing that sciences and technosciences have always and will continue to co-exist, the space of possible experience is altered such that the sciences and technosciences are now oriented towards a different overarching agenda than that of the scientific enterprise. This opening of spaces of possibility through the discovery of new modes of reasoning has been described as a Hacking-type revolution (Schweber and Wächters, 2000, 584). Examples of this include Lucien Febvre's famous account of Rabelais and the impossibility of unbelief in the sixteenth century: according to Febvre, it was strictly speaking impossible for Rabelais to be an atheist, due in part to the fact that the French language of his day did not provide him the necessary concepts (Febvre, 1982). Whatever led to the formation and availability of these concepts created new possibilities of belief and doubt. Similarly, Hacking argues that one needs a historically specific, economic notion of a "fact" as a discrete, countable, medium-sized unit in order to encounter the problem of induction as a logical problem of how to aggregate units which do not add up to anything more general than a sum. In this new world of facts thus arose a new philosophical problem, and Hume's formulation of that problem transformed the philosophical enterprise as a whole: "Hume became possible" – and would not go away (Hacking, 2002, 11–14).[12]

Again, one example must suffice to show how the age of science opened a new space of possibility that altered the rules of the game such that even traditional scientific disciplines abandoned their commitment to the scientific enterprise with its work of purification and became oriented, instead, to the regime of technoscience.

With animal models for biomedical research and especially with computer simulations, researchers have at their disposal powerful devices that allow them to study the behaviour of complex systems without understanding how these systems work. Both kinds of models are not properly representations but substitute realities in their own right: their purpose is not to depict certain properties of the natural or original system for which they serve as a substitute, but they are thought to share important behavioural properties with these systems. This sharing of properties underwrites the process of substitution and an entirely new possibility of reasoning.

[12]The emergence of probability and thus of a whole new class of problems, considerations, possibilities is perhaps more strongly associated with Hacking and especially with his earlier work. *Rewriting the Soul* makes a similar case for memory.

The following provides an illustration of this new form of reasoning: Rather than offering direct visual access, today's observational instruments typically generate data and use software to transform this data into a visual output. In parallel to this development, the models of scientists have gained complexity. In order to model or describe a particular situation, simulation modellers utilize formulae and algorithms from a variety of sources, including well-established scientific theory. Computers are needed to calculate these, and these calculations result in many numerical values. Again, software is used to translate these values into a visual output. Now, if the situation to be modelled is one that is observed by a particular instrument, why not create a visual representation of an imagined complex situation that emulates the way in which the instrument would display that situation? And thus one arrives at comparisons of calculated and experimental images which afford a reliable inference from similarity. The similarity of the two images is taken to be explanatory. What was observed in the experiment is attributed to a dynamic process like the one that brought about something very-similar-looking in the simulation. The apparent similarity between the experimental and simulated processes is taken as evidence for the fact that both processes share the same dynamic and partake in the same reality.

This all too brief and all too superficial presentation of an inference from the similarity of experimental and calculated images suffices to indicate that a new possibility of reasoning has come into being. From the point of view of the philosophy of science, this looks like a superficial and perhaps viciously circular form of inference. However, this may well be a perfectly robust form of reasoning that owes its robustness to the technical milieu in which it originated. This argument from similarity is characteristic of technoscience in that it unfolds in a technical rather than symbolic medium and establishes a measure of technical control. It is constrained and enabled by the technical demands that come with the need to manage complexity and to process vast amounts of information. In particular, it is enabled by the computer as a means not just to make calculations and to create visual images but to instantiate real physical processes. With animal models and computer simulations, the technosciences have at their disposal research instruments that afford complexity and that afford, in particular, the study and control of complex phenomena and their actual dynamics in the medium of the computer or the charted organism.

Numerous challenges for the philosophy of technoscience arise with the availability of research tools that do not serve to establish and represent relations between isolated features but afford the immediate presence and control of complexity (Nordmann, 2006). There is first of all the challenge to understand how this might draw the sciences into the regime of technoscience. Their interest in truthfully representing and understanding ever more complex phenomena gave rise to a condition in which the achievement of technical control appears to have become an acceptable and entirely sufficient substitute for theory-development and understanding. Then there is secondly the rational reconstruction of inferences by analogy which may owe their robustness and reliability precisely to the intellectually intractable mediation by research technologies. This provides thirdly occasion to investigate systematically whether and how the work of purification has really become impossible: especially simulated quasi-natural system behaviours appear to be attributable neither to science and nature nor to technology and culture. Classical conceptions of dispositionality suggest that there is an external, perhaps technical stimulus that is analytically distinct from the self-propelled, spontaneous, natural manifestation of the disposition to respond to the stimulus. Here, it appears to be more appropriate to speak of affordances, that is, of dispositional responses that are themselves engineered to address human interests (Harré, 2003). And indeed, the research instruments that afford complexity may also afford the arguments from the similarity of complex systems. Here, then, questions regarding the ontology of technoscientific objects intersect with questions of epistemology and methodology.

Conclusion

None of the arguments and evidences cited so far suffices to substantiate the claim that there was an epochal break between an age of science and an age of technoscience. But they showed how it is possible to argue for such a break without running afoul of obvious objections regarding precursors and continuities. If the scientific revolution allowed the moderns to imagine themselves as engaged in a scientific enterprise that advanced Enlightenment ideals, the shift to the regime of technoscience expresses no less powerfully the demands of an innovative knowledge society.[13]

Quite independently of their epochal significance, there is a need primarily to achieve a better philosophical understanding of the technosciences. So far, the question of the epochal break has been considered entirely subservient to this larger task. However, as Paul Rabinow has pointed out in his comparison of two theorists of technoscience, the very adoption of technoscience as a term of reflection and interpretation has a historical significance that ought to be acknowledged and appreciated. According to Rabinow, Latour's account falls short because he uses the notion of technoscience only to highlight that we finally see how it has always been: the designation "technoscience" serves to unveil the mythical character of "science and technology" as if these had ever been distinct and as if the work of purification had ever been a viable enterprise. What was formerly considered science and technology finally comes to fully understand itself under the condition of technoscience. In contrast to Latour, Hans-Jörg Rheinberger appreciates that the acknowledgment and recognition of the technosciences coincides with a transformation and reorientation also of the researchers who use this term as a tool to interpret and understand research activities (Rabinow, 1997). The technoscientific regime is no less mythical than was the scientific enterprise, especially if we attribute to it the power to orient a wide variety of research practices, and not just those of nanotechnology:

[13]Compare the contributions by Gregor Schiemann and Ann Johnson, this volume.

the cosmologist who used to ride high on the wave of prestige accorded to truth-seeking science is now scrambling to show the innovative potential of cosmological research.[14]

According to Rabinow, Rheinberger recognizes that "leading practitioners of the social studies of science, while claiming to be offering a comprehensive understanding of things that escape from the previous metaphysical interpretation of science as epistemologically adequate knowledge, have escaped this metaphysics only by embracing and embodying a technological understanding of being." To embody a technological understanding of being is to become someone other than an adherent of human transcendence through the pursuit of eternal truth. It subsumes all human aspirations and especially those of science under technology, thus (re)discovering Heidegger as a philosopher of technoscience (Rabinow, 1997, 40f.). This is not the place to articulate the larger philosophical implications of this discovery or of the Heideggerian reframing by Rheinberger, Rabinow, and Forman even of the scientific enterprise within a technological conception of the world. It can be noted, however, that this reframing relates to the epochal break thesis. Even if "(the regime of) technoscience" is an interpretive concept just like "science" or "the scientific enterprise," and even if many of the concrete research questions and practices have remained unchanged, how one thinks about science, technology, and technoscience is not without consequence (Bensaude-Vincent, 2009). How researchers conceive of what they are doing, assigns meaning to their practices and thus orients them, perhaps defines them. By extension this applies to the ways in which philosophers and STS scholars conceive of what researchers are doing – it orients research practice towards certain ideas and goals.

Though physics is still physics and chemistry still chemistry, there is now also nanotechnology and much physics that is "no longer physics."[15] Deprived of their traditional historical mission, all fields of research are oriented towards the regime of technoscience. And though scientific understanding remains a prized good in that regime, there have been changes in what it means to understand something. Philosophers of science and of technoscience will provide different accounts of when the search for understanding starts, how far it goes, and when and why it comes to an end. If the philosophy of science served to idealize and valorize science as a social institution for the critical and public employment of reason, the philosophy of technoscience needs to understand the promise and peril of advancing on the seemingly self-validating path of economic, social, and technical innovation.

[14]Inversely, the synthetic chemist who now rides high on the wave of support for nanotechnology, used to show how the discoveries and inventions of chemistry contributed to the larger truth-seeking mission of the scientific enterprise.

[15]Compare Peter Galison's work in progress on recent developments in physics of which many physicists claim that they are no longer physics (string theory, simulation modelling, nanotechnology).

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