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Marcel Herbst *Editor*

The Institution of Science and the Science of Institutions

The Legacy of Joseph Ben-David

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Preface

This anthology is an outgrowth of my interest in higher education and my discovery, so to speak, of Joseph Ben-David (1920–1986), i.e. after I had assumed, in the late 1980s, new duties as a planning official at the Swiss Federal Institute of Technology in Zürich (ETHZ). Prior to my new administrative position I had been unaware of Ben-David. For one and one-half decades I had worked outside the framework of the university, focusing on multi-purpose water resources planning, on hydro-electric and agricultural development, primarily in developing countries around the globe, or on waste-disposal systems in the chemical industry. Before that time, although being associated with universities as a student and faculty member, my focus was not on the institution of higher education.

In my new function at ETHZ, however, higher education was now the focus, and I was immediately confronted with a clash of cultures. My experience with universities in the late 1950s until the mid 1970s in Germany, Switzerland and the US, and my experience outside the premises of the university during the late 1970s and the 1980s, appeared to indicate that higher education cultures were nationally oriented. This was much more the case than it has for the industries surrounding the higher education landscape in the wake of globalization. The question posed itself as to what extent higher education systems that differed that markedly in their structure were in a position to be equally effective.

Quality stands at the core of research universities, and quality related issues became a central part of the daily work of many university administrators. Quality management, first developed in the industrial context in the US and Japan (Juran 1995), was taken up in the sphere of public administration (Osborne and Gaebler 1992) and universities under an expanded focus and name: Total Quality Management (TQM). TQM spawned a great number of workshops and publications, and the hope was that a collective focus on quality improvement would lift institutions to new heights of excellence. However, TQM did nothing to iron out differences in effectiveness which separated national higher education systems, and the hype subsided a few years after it had entered the stage of the higher education scene.

The focus on excellence and benchmarking however remained. Higher education, and research universities in particular, were increasingly seen as agents of economic development, as safeguards of and paths to prosperity. In this context, questions of effectiveness posed themselves, "value for money", particularly because higher education had expanded markedly (Trow 1970) and the respective societies were at a loss on how to fund such systems. In the late 1970s, a study of the two polytechnic institutions in Switzerland (ETHZ and École Politechnique Fédérale de Lausanne [EPFL]) (Fritschi et al. 1977, 1980) investigated the link between size and productivity, and the hunch, presumably, was that size effects could play a role, so called economies of scale or agglomeration economies, in which case the institutional management would have to pay attention to them.[1] At the end of the 1980s the planning commission of ETHZ had speculated again that size and performance were related and had proposed to redistribute the available resources over an enlarged faculty. The reform plans were short-lived and failed to be implemented because they conflicted with the conservative posture and the guild-like partisanship of the incumbent faculty and their representatives.

Early in the 1990s, browsing in a local bookstore while attending one of the annual fora of the Association of Institutional Research (AIR) in the US, I encountered Gad Freudenthal's anthology on Joseph Ben-David (1991). I immediately realized the significance of Ben-David's researches for my own work. In 1995 I organized two conferences at my institution: the 17th Annual Forum of the European Association of Institutional Research (EAIR), the sister organization of AIR; and to profit from some of the scholars present, a smaller second conference was directed at a Swiss higher education audience. In this context, a range of higher education researchers presented papers, among them Martin Trow (1926–2007), whom Ben-David had encountered while spending a year in Berkeley, and Burton Clark (1921–2009); the papers of this conference, including some additional material, were published subsequently (Herbst et al. 1997).[2]

After my early retirement in the year 2000 I wanted to devote my time to matters other than higher education, but soon thereafter François da Pozzo, the now retired head of the *Centre d'études de la science et de la technologie* (CEST),[3] approached me with the idea of working on a study to compare MIT with ETHZ, two natural peer-institutions and, in the course of this comparison, to use bibliometric indicators that CEST had developed (Herbst et al. 2002). To assess possible dangers of misuse of bibliometric data, particularly in the context of performance-based budgeting and funding, CEST commissioned a second study (Herbst 2004) that was subsequently expanded and issued as a book (Herbst 2007).

This research transformed my daily routines, and I became ever more conscious of the dilemmas with which higher education or research systems were grappling. The idea evolved to organize a conference focusing on Ben-David's legacy and to commemorate his 25th *Jahrzeit* (2011). Higher education or research systems are

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[1]The findings of that study were inconclusive, mainly for methodological reasons.

[2]I would like to express my indebtedness and deep gratitude to both of these scholars; for an overview of their work, see Clark (2008) and Trow (2010).

[3]Now part of the Swiss Science and Technology Council.

best understood comparatively, as viewed from the vantage point of the ‘outsider’, quasi-ethnographer or cultural anthropologist, and Joseph Ben-David was one of the few scholars in higher education research who had worked comparatively. His position in a sense was ideal. As a Jewish refugee from Hungary he could not that easily fraternize with, and feel part of, the tradition of the European or Humboldtian university he was unable to attend—although as a young immigrant to Palestine he attended the Hebrew University of Jerusalem which grew out of this tradition. His subsequent encounters with the British higher education system while enrolled at the London School of Economics on a scholarship from the British Mandate and his experience with US universities, particularly the University of California at Berkeley and the University of Chicago, must have provided him with a perspective few other scholars could muster.

Preparations started in 2007 to plan the conference. I had contacted Liah Greenfeld, a scholar of nationalism and modern culture at Boston University and presumably Ben-David’s last PhD-student, and she was impelled to co-organize a conference dedicated to her mentor’s legacy and to co-edit (sic) an associated anthology. Markus Christen from the Centre for Ethics at the University of Zürich agreed to participate; Michael Hagner, a professor of science studies at ETHZ, was brought on board; and one of Michael Hagner’s PhD-students at the time, Kijan Malte Espahangizi, was recruited as well. The five individuals (Christen, Espahangizi, Greenfeld, Hagner, Herbst) formed the program and the local organization committee for the planned conference. The *Centro Stefano Franscini*, the international conference centre of ETHZ located in southern Switzerland on the *Monte Verità*, a hill overlooking Ascona and the *Lago Maggiore*, agreed to host and to partially fund the conference. The remaining funds were donated by the “René and Susanne Braginsky Foundation”, by the Department of the Humanities, Social and Political Sciences of ETHZ, and by the Swiss *Studienstiftung*: I gratefully acknowledge their financial support. The conference entitled “The Role of the University in our Time: the Legacy of Joseph Ben-David as a Guideline for Today’s Challenges” took place in the summer of 2009 (Herbst 2009). Scholars from eleven countries and various disciplinary orientations participated. The conference eventually bore three publications, one in German (Gugerli et al. 2010), and the other two in English, i.e. one edited by Liah Greenfeld (2012) and the second one presented here.

A few acknowledgements are necessary. My focus on higher education developed rather late in my professional life, and I wouldn’t have been prepared to delve into a relatively new field without the earlier exposure to a range of mentors and scholars whose impact was formative. It is difficult to list a few among those who have supported me as a young person or whose ideas I had absorbed, but I should mention, above all, Arnold Niederer, Horst Rittel, Hanno Kesting, Lucius Burckhardt, Henry Hightower and Maynard Hufschmidt, teachers who had nurtured my critical thinking or quantitative—comprehensive, systemic—analyses; and Walter Isard, Kenneth E. Boulding, Russell L. Ackoff and C. West Churchman whose writings I had encountered in the 1960s; particularly Churchman, the philosopher in this group, had a significant impact on my thinking.

After I had joined ETHZ, I intensified my local contacts with colleagues whom I had known since the days of my concentration on optimization and systemic designs (operations research) to review higher education systems, specifically systems of research universities. The occasional discussions with Hans-Jakob Lüthi, and the regular exchange of ideas I had with Kurt Hässig or Hanspeter Eichenberger over coffee in the faculty club or over a plate of *chop-suey* in a nearby restaurant, were both elucidating and supportive; the debates within the planning commission of ETHZ, headed at the time by Konrad Osterwalder, were far-sighted; I came to know Herb Kells, and subsequently also Bob Simha and Martin Trow, all of whom have become personal friends; ETHZ profited from the visions of Jakob Nüesch (past president) and the tenacity of Katharina von Salis who had initiated ETHZ's first office of equal-opportunity (*Equal!*); and with colleagues like Burton Clark, François da Pozzo, Gary Matkin, Ian McNay, Terry Russell, Frank Schmidtlein, Michael Shattock, Lydia Snover, Henry Wasser, and others, I had an enriching professional relationship.

Lastly, I should thank the various associates, friends and organizations that have contributed, directly or indirectly, to this volume: the people who had served on the organizing committee for the Monte Verità conference, particularly Michael Hagner; the sponsoring agencies (mentioned above) and the participants of that conference; Nicolas Carayol, Rivka Feldhay, Gad Freudenthal, Michel Haymann, Beate Krais, Eric Kubli, Christoph Mandl, Mary Lou Mettler, Sandy Otis, Terry Russell, Thomas Schøtt, Chikako Takeishi, Nina Toren, Kurt Weiss, an anonymous reviewer and, of course, Miriam Ben-David. But above all, I want to thank my co-authors who stood steadfastly behind the project of this anthology and my family members (Jacqueline, my wife, and our children Rachel, Joshua and Rebecca) who supported me in my work on Ben-David—against all odds.

Zürich and Promontogno, Switzerland Marcel Herbst
April 2013

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Prologue

Chapter 1
Introduction

Marcel Herbst

Abstract The chapter introduces Joseph Ben-David as a scholar of sociology of science and elucidates briefly his particular comprehensive—comparative, international—approach to the study of scientific growth and higher education research. Competing views regarding a study of (a sociology or philosophy of) science serve to disambiguate Ben-David's position, and all chapters of the present volume are summarized.

Science depends on minds. The history of science forms a succession of great minds, of scholars. Scholars had their teachers and mentors, their schools and circles, their *yeshivot*, their convents and universities. Philosophers and scientists always had an environment within which to grow, and some of them eventually transcended their own role, from that of a student to that of a teacher, affecting future generations. Irrespective of how we define or date the institution of science, there is an environment, a culture, an institutional setup for science to grow.

Looking at the history of science one can look at the scholars and the scientists who stood out.[1] Alternatively, one can focus on the institution of science, that is, on the institutional or cultural setup in place to foster science.[2] Science feeds on models, on theory, on approaches, on concepts and beliefs, on technologies, and these in turn are affected by science. Aspects like these are the domain of various interrelated fields within history, philosophy or sociology which came to the fore under such names as sociology of knowledge (or, in the British version, sociology of scientific knowledge), sociology of science, philosophy of science or, more recently, science studies. Science, however, also depends on institutions of a material kind,

[1] There is a certain tendency in the historiography of science to do just that—and to stop right there; Ben-David (1964a, 455) remarks, in this context: "The development of science is often viewed as a process where the intellectual heroes of mankind speak to each other above the heads of nations and down the generations."

[2] In the words of Ben-David and Collins (1966, 452), "on the transmission and diffusion of ideas" and "on the environmental mechanism which determine the selection of mutation [of such ideas]".

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be they of a 'red brick' variety (i.e. a physical campus), an organizational setup, or a funding scheme. The study of such institutions is mainly the domain of social sciences (sociology, policy sciences, economics), history, education, or management sciences.

Occasionally the distinction between the two approaches, i.e. the focus on scholars on the one hand versus the focus on institutions on the other, is blurred if one looks at centers of learning, to use the diction of Joseph Ben-David, or at republics of scholars (Parsons and Platt 1973) located at particular places or institutions at a specific point in time, without addressing the conditions affecting science. Joseph Ben-David followed the approach to focus on institutions—on the institution of science and the science of institutions—and he covered this topic not only from a historical, but also from a socio-political perspective geared to shed light on the workings of science. He wanted to find out why science develops and which conditions are conducive for its vitality and growth.

Comprehensive Approach

Ben-David's major contributions addressed a whole spectrum of concerns and conjectures. He used historiography to expound change processes that drove science onward from the Greeks to modern times, and he had a concept of the primacy of basic—'pure'—science as well as of socio-political leanings that he shared with a range of scholars or major philosophers of science who were active in the first part of the 20th century (see Chap. 3). Because there were overlapping notions that bridged philosophy of science and sociology of (scientific) knowledge, he found himself trespassing, if you will, on other discipline's turf, but his main approach was that of a sociologist with a specific focus. He used his interest in the institution of science to contribute to a science of institutions. Indeed, his insights on the workings of modern higher education systems are exemplary—and perhaps even critical—for a proper understanding of the evolvement, growth and functioning of the research university, past and present.

Ben-David was one of the few scholars who studied science and higher education systems in a comparative—international—context, and in this orientation he is clearly a member of a vanguard: "Scarcely any sociologists have been equally at home in the history and problems of the conduct of science and in the study of the institutions of disciplined study—in academies, universities and research institutes—in as many countries as Joseph Ben-David" (Shils 1987b). And yet when Ben-David died in 1986 there were already strong tendencies away from a (comparative) sociology of science championed by Parsons, Merton or Ben-David (Ben-David and Sullivan 1975). More fashionable foci came to the foreground with emphases on new interpretations of a philosophy of science, culminating in the Sokal affair (Sokal and Bricmont 1997), or on micro-sociological laboratory studies. Instead of a 'sociology of science' simply studying the sociology of science and encompassing various approaches, the small field became further subdivided along epistemic notions.

Ben-David viewed science and higher education systems from a historical perspective, but we could also say, using 'modern' language, that he perceived systems in a dynamic way: it was the movement, the change, which caught his eye (Herbst 1999). He worked comparatively and, perhaps not in line with the conservative stance regarding a sociology of knowledge attributed to him, quite evaluative. He tied the dynamics of science and his search for 'centers of learning' to the dynamics of higher education, at least starting with the 19th century, and he bound the dynamics of higher education to the culture of higher education, to 'role and ethos' and 'organization'. This interlinking of macro-sociological aspects concerning the spread of science and higher education and the search for 'centers' with micro-sociological aspects concerning 'role', 'ethos', and 'organization' are, perhaps, the unique features of Ben-David's research.

Ben-David was a proponent of a relatively conservative sociology of science. Educated in history and sociology, he developed a propensity for cross-disciplinary studies, at least as far as the social sciences and humanities were concerned, and his reception hinged on the various constituencies which were grounded in the one or the other discipline. Because of his positivistic stance and his combative personality, he was not easily disposed to accepting deviant positions and his feud with proponents of a sociology of scientific knowledge may have been bruising (Mann 1993).[3] But he shared with his adversaries the basic notions of his sociology of science, namely, in the words of Randall Collins (1986), that "scientific productivity and innovation are determined by social factors". According to Collins, Ben-David wanted to "ground science in its social context, not so much in a free-floating climate of ideas, nor [even in] an economic or class system (though these might have their influences), but above all in the actual organizations within which scientists worked". Since Ben-David's theses—and those of his collaborators—were varied, they were frequently seen as separate conjectures (Turner et al. 1984) to refute, not embedded in a broader vision, and this vision appeared to have been difficult to perceive.

As a proponent of a sociology of science, Ben-David was critical of the sociology of knowledge, at least insofar as the sociology of knowledge is said to postulate a systematic relationship between 'class' systems (or social groups) and their associated systems of thought (Ben-David 1984/1971, 7f).[4] Ben-David did not deny

[3]In his "To Jerusalem and Back", Saul Bellow (1976, 105) characterizes his friend in the following way: "I have learned to think twice before offering Ben-David an opinion on any matter, because his tolerance for vague views and inexact formulations is limited. He is a short, compact man. His blue gaze is mild enough, and he can even look contemplative and dreamy, but he fires up easily. Our discussions would turn into arguments if I didn't give ground, so, because I respect him, I invariably back off".

[4]The sociology of knowledge, as conceived in the 1920s and 1930s by figures like Max Scheler or Karl Mannheim, was to refer to a much broader context, namely to the interplay of knowledge, conceptions and beliefs in its various forms and the respective societies or social environment in which these emerged: it was meant to be a "theory of the social or existential determination of actual thinking" (Mannheim 1985/1936, 267). This 'determination' was not meant to be strict: that would imply a one-to-one relation between class, social stratum or setting on the one side and knowledge, conception and beliefs on the other.

that philosophers and scientists approached the world in ways that were reflective of their own formation and vision and, in his normative views on science, he appeared to embrace an operational philosophy (Rapoport 1965) or a realist stance (Kitcher 1993), rather than a sociology of scientific knowledge. What he did deny was the "regular relationships between the perspectives and motives of social groups on the one hand and philosophical, legal, and religious (or ideological) systems of thoughts on the other" (*op. cit.*, pp. 7f). However, if one construes a sociology of knowledge as a "sort of sociologically enlarged hermeneutics", as Freudenthal and Löwy (1988) suggest, Ben-David's position could be read as a paradigm for a—possibly reinterpreted—sociology of knowledge[5] (see Chap. 4).

Ben-David tied science as it was practiced and lived to social variables, and he was interested to explore the forces governing its development and usage. He postulated that the conduct of science, and the roles of those doing science, were related to—and codetermined by—the environment and the working conditions in which science was cultivated.[6] This made it possible to study scientific growth, or performance, in terms of social conditions or 'styles' that were seen to be characteristic of the various scholarly environments in which science was conducted;[7] and, furthermore, novel scientific practices, avenues or roles could be explained in terms of an exploitation of opportunities that had cropped up in the environment of corresponding scientists or professionals. In his studies of science and higher education, Joseph Ben-David clearly crossed disciplinary boundaries, affecting scholars in various fields, and the reception of his contributions and his legacy were uneven, perhaps due to his cross-disciplinary approach. The sociology of science, as championed by figures like Parsons, Merton or Shils, has been relegated to the background, replaced somehow by 'post-modern' approaches and the new science studies (Bloor 1976; Hands 2001). With these general reassessments, Ben-David's *œvre* has lost

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[5]The respective broader nexus was already widely discussed in the 19th century, not only by Marxists, and it was further amplified through the work of ethnographically oriented social scientists or philosophers—like Lucien Lévy-Bruhl, Marcel Mauss or Claude Lévi-Strauss—who were dealing, in one way or another, with "social consciousness" (Mead 1912); some decades later, "social perception" was demonstrated experimentally (Ames 1951).

[6]It is obvious that science practice is culturally codetermined: faculty-faculty relations and faculty-student relations (Herbst et al. 2002), the conduct of experiments, forms of presentations or lecturing, intellectual discourse, science languages, et cetera, are culturally affected. Less obvious is perhaps the notion, shared at least partially by Ben-David, that science practice codetermines the effectiveness—and possibly even the content—of science.

[7]It appears that Ben-David did not know, or did not pay attention to, Ludwik Fleck—and vice-versa (Werner and Zittel 2011). Fleck used the term *Denkstil* to refer to a culturally molded scientific practice, a notion that was quite alien during Fleck's time—and has remained alien in many circles until now. In his foreword to Ludwik Fleck's "Genesis and Development of a Scientific Fact" (Fleck 1979, viii), Thomas S. Kuhn cites the former Harvard president James Bryant Conant to whom Kuhn had introduced Fleck's work. When Conant had become US High Commissioner for Germany a few years later, "he [i.e. Conant] reported with glee the reaction of a German associate to [Conant's] mention of the title of Fleck's book: 'How can such a book be? A fact is a fact. It has neither genesis, nor development' ".

appeal among contemporaries.[8] With the relative neglect of the sociology of science in the recent two or three decades, broad questions—central to an understanding of today's science and higher education enterprise—remained under-researched.

Today's higher education stands at a crossroad, and it is unclear where the various routes may lead. Higher education systems of the developed world started to change their character after 1980 to respond to mass higher education (Trow 1970), to retrenchment (Clotfelter et al. 1991), and to a novel understanding of capitalism which introduced quasi-markets in the public sphere and elaborate stock market instruments of extended risks (Khurana 2007). Performance funding gained support (Herbst 2007), rankings of dubious quality became ubiquitous (Billaut et al. 2010), adjunct teaching tended to replace tenure (Teeuwen and Hantke 2007), and the institution of the university was frequently paralyzed by the conflict between a conservative faculty, claiming their corporate rights, and a change-oriented governance or polity. While higher education and science systems scramble for funds, there is a lack of understanding as to how such complex systems evolve or work, which role institutional diversity (Clark 1997; Trow 1997) or entrepreneurship (Clark 1998; Matkin 1997) should play, which resources ought to be tapped, or how funds ought to be allocated to improve performance.[9] In emerging economies, higher education systems expand at an unprecedented scale and speed, fusing their own traditions with imported models.

That is the context for a renewed reflection on, and examination of, Joseph Ben-David's work.

The Chapters of This Anthology

This anthology assembles nine essays that deal with Ben-David's varied vision of science and its institutions, or cover similar turf. Apart from the first and the last essay, the contributions are subdivided into three major themes—and corresponding parts of this book—that stood at the core of Ben-David's studies which concern us here: Center and Periphery (Part III), Role and Ethos (Part II), and Organization and Growth (Part IV). An Epilogue (Part V, i.e. Chap. 10) finally, summarizes the evolvement of Ben-David's thoughts and theories and its relevance in today's discussion in the field of higher education research.

Chapter 2 on "Academic Organization and Scientific Productivity" is my attempt to trace those features of Ben-David's work that I found so attractive when I first

[8] This seems shortsighted, as Ilana Löwy points out (in Chap. 4): "The dismissal of Ben-David's heritage in the name of more progressive ideas may deprive the defenders of these ideas of efficient tools to promote them".

[9] It might be instructive also to note issues within higher education which Ben-David, for all practical purposes, did not—or didn't have to—address. Among such issues, a topic of some reverberation during the past two or three decades concerns tuition fees (Bowen et al. 2006).

encountered Gad Freudenthal's anthology (Ben-David 1991) and that I find so pertinent for an understanding of the systemic aspects of higher education. This tracing of Ben-David's corpus of ideas has an economic bent: it covers adaptation and science diffusion-diversification processes, science cultures as well as issues pertaining to effectiveness and survival, and it might serve as a sketch for a—as yet to develop—reinterpretation of Ben-David's sociology of science.

Chapter 3 by Yaron Ezrahi on "Ben-David's Critique of the Sociology of Knowledge and his Politics of Freedom" focuses on Ben-David's normative notion of science and its relation to an 'open', rationality-focused society (Popper 1971/1962). Like a range of his contemporaries and friends, and like an array of influential politicians in the Western world, Ben-David believed in the enlightenment function of science and in the associated benefits accrued to society. Early in Ben-David's career, in the 1950s and 1960s, there was a social contract in place which accorded universities and research centers a high degree of autonomy, or so it seemed, and academia and the respective societies were both profiting from this consensual arrangement.

This situation changed slowly, first with the Sputnik Shock and later with the Vietnam War, and the associated recruitment of academic know-how—or the revival of scientific engagement—to serve the Cold War and a war machinery in the Far East was instigated. On the politicians' side, there was the strong notion that science was instrumental and effective in supporting the tactical means and the strategic ends of a Western world, and large planning endeavors—comprehensive metropolitan transportation and land-use studies, "war on poverty"—were enacted and funded by the US government. But the subsequent quicksand experience brought about a strong opposition to the Vietnam involvement and, to some extend, a disillusionment on the part of the decision-makers. The role of science and scientists had changed, and the reflection on science transformed itself as well.

Against this backdrop Ben-David's "idealization of the autonomy of science", as Yaron Ezrahi points out, had become somewhat "anachronistic"; but we are still called upon to share Ben-David's "persistent passionate concern to preserve the integrity of science and the cultural foundations of the politics of freedom in contemporary society".

Chapter 4 by Ilana Löwy on "The Scientists' Role and Medical Innovations" uses medicine, the focus of her research, to illustrate her reading of Ben-David. She regrets the "hasty dismissal" of much of the classic sociology of science by a new generation of scholars. Earlier criticism frequently stressed the role of the individual, the genius, and depreciated the link between institution and performance (Kuhn 1972) that stood at the center of a sociology of science. The newer science studies with their much freer interpretation of what constitutes science rejected the implicit science notion of a sociology of science and simultaneously blended out the very core or substance of the field they were to supersede. A range of vital topics of investigation, hence, "disappeared" from the agenda of contemporary scholars or remained rarely researched.

Ben-David tied the diffusion of disciplinary thought and innovation to his concept of the scientist's role. Bacteriology (launched by Pasteur) and psychoanalysis (developed by Freud) served as examples. In both cases, a “role-hybridization” was said to be instrumental in the move from the original disciplinary field to the new focus. Ben-David’s diction of ‘hybridization’ may have been replaced today by ‘transdisciplinary’, but the underlying idea is similar. It is frequently a move of concepts, ideas or methodologies—and implicitly also of scholars—from one field of investigation to another (new or old). Löwy explores this diffusion process on the basis of two examples: screening for cancer; and prenatal diagnosis. In both innovations, the impetus to develop these novelties did not originate with a specific group of scholars or professionals—and specifically not in their intent to transcend a constricting ‘role’, as Ben-David might have suggested. Rather the innovations came into being as the result of a complex interplay of various stakeholders “within and outside the studied scientific domain”.

Chapter 5 by George Weisz is entitled “The Ongoing Tension: Clinical Practice and Clinical Research”. Ben-David had studied the medical sciences on a number of occasions and traced their impact on the development of the sciences and professions in general. Weisz exemplifies with his paper a range of aspects which Ben-David addresses, not only in the context of medicine: the interplay of teaching and research; the relative position of basic research versus applied research; the role of the sciences and professions within higher education; and the professional ethos regarding research and service.

Medicine is an old goal-oriented, human-centered profession and, as Ben-David had shown, it stood at the beginning of modern science with its propensity to specialize. Weisz traces the early development of modern medicine (in the 19th century) and locates scientific progress in regions where medicine opened itself up to the entire society, where social inequalities existed and where patients of the lower stratum could be ‘used’ as subjects for scientific investigations: “[...] it was the medicalization of large urban hospitals that made a new sort of research possible”. This brought the medical profession into an internal conflict which foreshadowed the tensions subsequent: the curative aspect of medicine, the necessities to base practice on evidence, the comprehensiveness of medicine versus various specializations, interests of an evolving—and now even domineering—pharmaceutical industry with its emphasis on (ex-post) treatment, aims of public health with its emphasis on (ex-ante) prevention, the foci and incentive structures of health insurance programs, etc.

Weisz focuses on some of these tensions as they developed at the interface between practice and research orientation and as they affect medical schools. Medicine has followed its own path, it appears. Other professions like law, business administration, architecture, education—even engineering—have a clearer focus on practice, and educational institutions reflect this tendency. In contrast, medicine has evolved into a highly regulated field stressing licensing of professionals, schools or medication as well as treatment standards and protocols, with the estates of the medical professions, bio-medical industries, insurance companies and government organizations as the major players. Today, clinical, pharmaceutical and bio-medical research, driven by various economic and social forces, has gained ground over medical practice, public health and prevention. Medical schools, following the current of these forces, mirror this orientation. However, Weisz also observes countercurrents, supported by clinician-teachers, to build new bridges between clinical practice and research.

Chapter 6 contains Richard Münch's "Faded Grandeur: Disciplinary Differentiation, Interdisciplinarity and Renewal in the German Academic System". Münch starts out by tracing—or illustrating—the productivity of German, or European, science *vis-à-vis* US science (in terms of various science indicators). The early dominance of European science (before World War II) turned into a dominance of US science (after WWII). In recent decades, this imbalance has given rise to a performance-measurement or ranking craze, coupled with corresponding demands or improvement directives—in terms of performance contracts (*Leistungsaufträge* or *Leistungsvereinbarungen*) or various financial incentives. The systemic links to performance, however, and the "oligarchic system" responsible for stagnation, were basically left untouched.

Münch's analysis of the German university is linked to the role of the German humanities, the *Geisteswissenschaften*, with its strong ties to Philology and Philosophy. Already at the beginning of the 20th century, there was a tendency in Germany to separate the natural sciences from the universities in dedicated research institutes. The Humboldtian notion of the "unity of teaching and research", exported to the graduate schools of US research universities, became a myth, at least for the natural sciences and for some areas of engineering (with their own research institutes). With the separation of dedicated research institutes from universities, the humanities formed a stronghold of the German university.

Because of the dominant standing of the *Geisteswissenschaften* in the 19th century, and because of their ties to an educated elite of professionals, teachers and scholars, Münch sees a homology between *Geisteswissenschaften* and the social stratum carrying it: a "social and cognitive closure". After WWII, and after the ascent of mass higher education in Germany, the "social opening" of higher education was not properly accompanied by a "cognitive opening" of academia due to academia's inherent structural deficits. The old *Geisteswissenschaften* and the old notion of science and scholarship were pushed to the fringes, while a diversified spectrum of newer professions and disciplines, often located at the margins of—or between—established fields, failed to develop. Increasingly, the various disciplines, including what Münch terms the "new humanities", are forced to follow the doctrine that "large is beautiful", without improving performance. Top-down efforts to reform, initiated at the European or national levels, remain ineffective or ill advised in Münch's view, and higher education remains deficient as long as the structural deficits of the respective science systems are perpetuated. For the time being at least, European science rests in the shadow of US science.

Chapter 7 by Shaul Katz on "The Scion and its Tree" focuses on the transfer of German science models, i.e. the transmission of intellectual ideals, research traditions and organizational forms that affected the newly established Hebrew University of Jerusalem. During the formative years of the Hebrew University (prior to

World War II), Europe—and in particular Germany—was still seen as the center of the academic world, and a range of great European scientists were called upon to be involved. But from its beginning, the Hebrew University was an international undertaking, and unique in this form trying to bridge Europe, America, and the Orient. This tripartite orientation caused internal stress, to be sure, but it also served as a source for cross-cultural inspiration. Specifically, it combined pure science notions, which Shaul Katz elaborates, with practical considerations deemed necessary, and it combined a European self-assurance with American pragmatism and an American admiration for German institutions.[10]

In his chapter, Shaul Katz traces the preparatory phase and the early decades of the Hebrew University, examining three constituting parameters of institutional development slightly different from the tripartite orientation just mentioned: Germanness, Jewish-ness, and local-ness; and he traces the development in mathematics, natural sciences, oriental and Jewish studies, and medicine (medical research). Prior to World War II and the holocaust, and certainly prior to 1933, the Hebrew University was basically the product of a Jewish "national liberation movement", of voluntary immigration; and after it had been established and found form, the Hebrew University had a "formative influence upon the whole of Israel's scientific research framework and higher education system".

Chapter 8 deals with spurious concepts, and is written by Andrew Abbott. It is entitled "The Excellence of IT: Conceptions of Quality in Academic Disciplines". Abbott approaches the subject matter from the perspective of the quasi-enthnographer who is not familiar with the concepts in question and who, therefore, has to investigate like an alien person the cults or customs of a local tribe.

In the UK, as funding councils have started to finance institutions on the basis of a Research Assessment Exercise (RAE) designed to measure the 'quality' of a field or disciplinary orientation, questions arose as to how such elusive concepts as 'quality' can—or should—be measured or conceptualized, whether research groups ought to be funded on the basis of past performance or their future potential, and to what extent quality measurements are subject to manipulation (Trow 1994). Abbott addresses himself to the first question.

Abbott studied responses to the RAE by academics, and analyzed those. He alienated (in the Brechtian sense of a *Verfremdungseffekt*) common concepts like 'excellence' and 'quality' used in these responses by replacing them with markers: 'excellence' was replaced by IT, 'quality' by ITQ, 'excellent' by ITLIKE, and so on. On the basis of such alienations, he read and interpreted the texts anew. His aim was "to recover from the language and assumptions of these responses their underlying conception of excellence in research".

Abbott was seeking differences separating the perceptions within the humanities and the sciences, but he found similarities in the responses across the disciplinary

[10] Abraham Flexner (1930), an American expert on medical and higher education, and the founder and first director of the Institute for Advanced Study in Princeton, was involved in an advisory capacity during the preparatory and early years of the Hebrew University.

boundaries and a focus on "canons of common work". He studied his attributes along five dimensions, namely cognizability (or measurability), units of measurement (or what should be measured), space and time (or their spatiotemporal character), modality (positive and negative aspects affecting perceptions) and content ("what is 'excellence' in research?"). But he was unable to deduce any shared foundations on which to base a RAE: "nowhere in these comments is there anything about the substantive content of excellent work nor, indeed, of work at all".

Chapter 9 by Ivan Chompalov, entitled "Lessons Learned from the Study of Multi-Organizational Collaborations in Science, and Implications for the Role of the University in the 21st Century", addresses forms of research which engage teams spread over one or more institutions (research universities and industrial companies). The practice to pool 'brains' to address complex, interdisciplinary questions has become evident since the famed Manhattan Project during WWII. In that case, researchers were concentrated in one location, Los Alamos, and the subsequent fashion to build 'science parks' followed this tradition. Other forms of pooling became necessary when research depended on costly infrastructure, such as in high energy physics.

With the advancement of the Internet, the easy exchange of information, and the simple and secure access to data repositories, research teams in different locations were—and are now—in a position to cooperate much more easily and to exploit various scale and agglomeration economies resulting from the pooling of 'brains' or the sharing of infrastructure, i.e. instrumentation. Chompalov and his colleagues have studied a range of such research cooperations over the past years and they have tried to identify factors which make such cooperation successful. They identified four types of research cooperation: bureaucratic, leaderless, non-specialized, and participatory. Except the 'participatory' category, which the authors found to dominate in the field of particle physics, the remaining three categories cover cross-disciplinary endeavors. The more formally organized and tightly managed projects were seen to be "prevalent in the field sciences (e.g. space science, geophysics), while small, more informally organized and more loosely managed [...] projects are more common in the [...] laboratory sciences (e.g. materials science)". No "association could be established between size and perceived success of the collaboration".

Chompalov mentions the significance of such organizational constructs for the development of research in the 21st century. He does not claim that US universities are particularly successful in forming inter-disciplinary *ad hoc* research networks which stretch over a range of institutions, but it should be clear that outside of a large infra-structural arrangement (such as CERN), many—if not most—non-US universities lack the organizational foundations and flexibilities necessary to embark on bottom-up driven projects as those described.

Chapter 10 on "The Legacy of Joseph Ben-David" is my attempt to pay tribute to an exemplary scholar and to review Ben-David's formation and intellectual positions, in the context of his own environment and on the basis of the spectrum of contributions collated in this anthology.

In this Epilogue, I sketch Ben-David's professional development from that of a young Hungarian refugee and immigrant to Palestine to that of a preeminent scholar of science and higher education, and I attempt to emphasize aspects within Ben-David's system of thinking which might form a base on which to build.

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Chapter 2
Academic Organization and Scientific Productivity

Marcel Herbst

Abstract The chapter is an attempt to trace those features of Ben-David's work that appear attractive from a structural—or science propagation—point of view and are pertinent for an understanding of the systemic aspects of higher education. This tracing of Ben-David's corpus of ideas has an economic bent: it covers adaptation and science diffusion-diversification processes, science cultures as well as issues pertaining to effectiveness and survival, and it might serve as a sketch for a—as yet to develop—reinterpretation of Ben-David's sociology of science.

Early in his academic career Ben-David published a sociology of science paper that was to foreshadow his major research orientation (Ben-David 1960b). This paper followed his initial explorations into sociological concepts of professions, social structure, class and role, and it related organizational aspects of academia to its productivity. Ben-David addressed this relationship with the conceptual apparatus and the tools of the social scientist steeped in history and sociology, but it is clear that he also addressed a central economic question. In the following, I shall introduce the major tenets of his system of thought—and the major aspects this anthology is, directly or indirectly, dealing with. In doing so, I shall try to follow Ben-David's own historiographic path to explain the system, with occasional excursions into philosophy of science, economics, and the management sciences.

Diffusion, Role-Hybridization, and Diversification

If one looks at the growth of science, one may distinguish a number of phenomena. Starting with the industrialization in the late 18th century, there was a manifest need for professionals in technical fields (mining, civil engineering, mechanical engineering, *génie rurale*, surveying, architecture, etc.). These professionals were to be educated and trained in newly established poly-technical schools and *Bergakademien*

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and, in the 19th century, in *technischen Hochschulen* and institutes of technology. Parallel to this development, there was also a demand for an educated class beyond the clergy (law, philology and philosophy, medicine, etc.) to fill positions in public administration or service, industry, and educational institutions, and existing universities broadened their curricula, or new institutions of higher education were founded.[1] This was the actual start of the growth of higher education around 1800.

Ben-David tied the initial ignition of growth in science to the emergence of a new role, that of the scientist. He used the concept of role, and the role of the scientist,[2] to explain the emergence and partially the growth of modern science. Role is an early notion of Ben-David to concentrate on: he used it in his initial studies on medicine (Ben-David 1958, 1960a), the concept was an outgrowth of his earlier studies of professions (Ben-David 1955, 1956, 1957, 1958a), and role was accompanied by a professional ethos. In looking at science as a profession, the 'role' of the scientist was linked to other sociological concepts like recognition, status, stratification, position, et cetera.

Scientific growth manifested itself by a number of related—co-evolutionary—developments:

- the geographic diffusion, propagation and dispersion of science, i.e., the growth in the number of higher education institutions and an associated growth in the number of faculty positions, or enrollments of students, in given fields;
- the disciplinary differentiation and diversification, and the growth in the number academic—disciplinary—fields (see also pp. 197f).

In the 19th century, this evolution was particularly fertile in the cultural sphere embracing Prussia and the Austrian-Hungarian Empire for a range of reasons which Joseph Ben-David had pointed out: professional development tied to polytechnic institutions and other schools found a complement in the new research orientation of universities; the institute, the laboratory, became the sustaining locus of research in the natural sciences; and regional competition fostered the geographic diffusion of research and the formation of new academic foci or disciplines. In fact, the German university[3] of the 19th century became the role model, the ideal-type, to be emulated (Schwinges 2001).

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[1] I do not distinguish here between the various 'layers' or 'orientations' of higher education, that is, between professional schools on the one side and universities on the other, a distinction with fuzzy boundaries that has persisted until now.

[2] In what follows, I shall use the concept of 'scientist' in a loose way, unless I specifically depart from this convention. Under scientist I understand the person who does science (primarily, but not exclusively, in a formalized research setting, e.g. a university), irrespective of the merit that is attributed to this activity—and irrespective of the perception whether said activity does in fact conform to scientific standards as defined by this or that party. My notion of science embraces not only the natural sciences but also various professions (e.g. law, medicine, engineering), the social sciences (including the humanities), as well as the sciences of the artificial (e.g. mathematics, computer languages, theoretical operations research).

[3] When we talk of the 'German' university, we talk of a ahistorical generalization of a university concept that we associate, today, with Wilhelm von Humboldt (1964a), one of the founders of

Diffusion and diversification were linked in an overlapping two-step process. Once an academic field was created with a corresponding scholarly following and associated academic chairs or faculty positions, the field diffused to institutions (and nations) where such chairs had to be established. The diffusion process slowed down—or even came to a temporary halt—when there were no higher education institutions left without corresponding open positions. The slowing down of the diffusion process, in Ben-David's notion, fostered a diversification process that he linked to 'hybridization' in two varieties: role-hybridization and idea-hybridization. Once new hybrids of scientific fields were established, the process of diffusion could start *de novo*.

Ben-David's notion of the diffusion and diversification of scientific disciplines was developed on the basis of two new sciences, bacteriology (developed by Louis Pasteur and others, in the 1850s and 1860s) and psychoanalysis (initiated by Sigmund Freud and others in the 1880s and 1890s)[4] (Ben-David 1960a). In both cases, he reasoned, the particular impulse to develop a new avenue of investigation and to innovate was initiated by 'outsiders' of the scientific establishment, that is, by practitioner-scientists interested more in curing illness, more in solving practical problems, rather than in furthering the scientifically accepted ways of their time. In doing so, both Pasteur and Freud gave up the established role of the career scientist, perhaps involuntarily, to assume a new role that fused their old orientation to achieve new aims: they applied their old role—"exact observation and isolation of factors through experimentation or clinical reasoning"—to the new role of practitioner and innovator (Ben-David 1960a, 566). Ben-David called this move role-hybridization.

Ben-David, as a sociologist, places great weight on the motive to change a role to explain role-hybridization: "Freud attempted to maintain his status by trying to raise medical practice into a form of scientific research, and as a result created psychoanalysis. Similarly, Pasteur gave raise to bacteriology by maintaining his theoretical perspectives after moving into research on wine fermentation, and elaborated his discovery into a new speciality" (Ben-David and Collins 1966, 459f). The general underlying idea of role-hybridization, and to "raise status [. . .] through [. . .] innovation", is described in the following (Ben-David and Collins 1966, 460):

> "Mobility of scholars from one field to another will occur when the chances of success (i.e., getting recognition, gaining a full chair at a relatively early age, making an outstanding contribution) in one discipline are poor, often as a result of overcrowding in a field in which

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the University of Berlin (1809). This concept of the university, rooted in various implicit rules and regulations (March et al. 2000), or in a specific culture, affected research universities in the German speaking regions or neighboring countries of the 19th century (or, in today's terms, in Eastern Europe, Germany, Scandinavia, Holland, Belgium, and Switzerland).

[4]In reference to what I had said in footnote 2, we should note here that Karl R. Popper (1962, 33–38) regarded psychoanalysis, in the context of his demarcation of the sciences, as a classic non-science because it was not falsifiable. Ben-David (1960a, 564) himself was not taken aback by various attacks on Freud: "There was a great deal of non-scientific elements in Freud's thinking [. . .]; "prophetic overtones were not that unusual among nineteenth-century scientists, and—at least in the early writings of Freud—they are not difficult to separate from the scientific elements of his work".

> the number of positions is stable. In such cases, many scholars will be likely to move into any related fields in which the conditions of competition are better. In some cases, this will mean that they move into a field with a standing relatively lower than their original field".

Furthermore,

> "[...] the chances of [...] major innovation occurring in a discipline into which there is mobility from a high-status discipline are considerably greater than in a discipline into which there is no such mobility, or which stands higher in status than the discipline from which mobility takes place".

In other words, Ben-David makes three claims: first, scholars leave their original discipline to find a new role in an associated field if the conditions for career advancement in the old field are constrained and the corresponding prospects in an associated—new—field appear better; second, innovation is often a byproduct of role-hybridization and is used to raise status; and third, new fields profit the most when their progenitors originate from a high-status discipline.

While Ben-David sees in role-hybridization the primary motor behind disciplinary diversification, he sees in idea-hybridization, i.e. "the combination of ideas taken from different fields into a new intellectual synthesis", a similar force.[5] This force, in Ben-David's reasoning, "does not attempt to bring about a new academic or professional role, nor does it generally give rise to a coherent and sustained movement with a permanent tradition". But the two forces, role-hybridization and idea-hybridization, can be seen to be linked: in the words of Gad Freudenthal (1987, 138), "role hybridization gave rise to [a] corresponding 'idea hybridization' ".

It is unclear to what extent Ben-David's distinction of role-hybridization and idea-representation needs to be maintained, and Ben-David's exposition—or model—of the diffusion and disciplinary diversification of science, one of his major contribution in his *œvre* from my point of view, does not hinge on his concept of role-hybridization (nor on the three claims mentioned above).[6] Role-hybridization is clearly not the only reason why scholars trained and acculturated in one field will create—or move into—another; furthermore, it cannot be considered as the dominant reason to initiate a paradigm change.[7]

[5]We would call these forces, today, cross-disciplinary or trans-disciplinary.

[6]In his later monograph on 'role', Ben-David (1984/1971) appears to have abandoned the use of the concepts—or the terms—of role-hybridization and idea-hybridization (the terms are not listed in the index).

[7]In the case of Albert Einstein, one can speculate as to whether he would have produced his major papers had he not been forced to leave the university (ETHZ) to assume a position of relative low prestige at the patent office (*Patentamt*) in Bern. The case of Ludwik Fleck, as Freudenthal and Löwy (1988) show, appears to confirm Ben-David's notion in that particular case. But there are many instances where scholars have produced idea-hybridization in the past without role-hybridization—or, at least, without the particular motive that Ben-David claims to induce role-hybridization.

Center, Periphery, and Diversification of German Science

Following the reasoning of Joseph Ben-David,[8] sketched above, scientific growth is attributable to two major currents: disciplinary diffusion, and disciplinary diversification. Diffusion is regulated by rules that specify the particular roles of scientists as well as the creation and succession of scientific positions. Diversification, in turn, is regulated by a disciplinary specialization and hybridization process.

In his historiographic studies regarding Germany of the 19th century, Ben-David observed that diffusion as well as diversification of scientific activities took place, thus establishing Germany as the world center of science (Ben-David and Zloczower 1991/1962; Ben-David 1977a). German science was in a position to expand, to grow. Diffusion was not only regulated by the adopted rules, it was specifically fostered by those. The rule, for instance, to have each discipline at a given institution represented by one single chair-holder (and head of an institute) forced young scholars (*Habilitierte*) to look for new turf.[9] They had to move to an institution where the corresponding discipline was not yet established (or where the position of the incumbent was to be vacated).[10] The root of this rule lies in the specific social contract that bound universities to the state (*Länder*); the social contract, in turn, is based on the *Allgemeines Landrecht* of the 18th century—see Paulsen (1902, 88). The social contract that regulated the interplay of universities and state stipulated that the university was autonomous with regard to teaching and to the research of those who were members of the corporation of scholars, but that appointments of faculty were the domain of the state.[11] In this way the state assured that the university that was under its jurisdiction was paying attention to the spectrum of sciences that the state thought necessary. It was also a safeguard against inbreeding or aloofness of the community of scholars. Other rules, such as the unity of teaching and research, or role conceptions or norms that were directly linked to the status of faculty members, unique in a sense to the German system, can be seen as a co-requisites that worked to foster diffusion and diversification (Schwinges 2007).

Rules, regulations and cultural norms, together with the decentralized state structure that was characteristic of the German university, and the ensuing competition among the various *Länder* and states, were directly responsible for the disciplinary diffusion and, in due course, also for the disciplinary diversification.[12] The particu-

[8]And, in particular, of two of his associates and co-authors, Randall Collins and Awraham Zloczower.

[9]I call this the "ecological argument" of Ben-David; see p. 198.

[10]In the case of a vacated position, the new chair-holder was called the *Nachfolger* (successor) of the Emeritus. In this way, lineages of chair-holders could be drawn (like those of royalty). The term *Nachfolger* is still in use today.

[11]In the case of appointments of successors of existing chairs, the faculty had the right to suggest three candidates (but the state was not obligated to limit its search to those suggested); in the case of positions that were to be created *de novo*, the corporation of scholars (limited to the ranks of full professors [*Ordinariate*]) was not involved in the decision at all (Paulsen 1902, 95–102).

[12]The diffusion of German science was not accidental or unintended. In his petition to found the new University of Berlin, Wilhelm von Humboldt (1964a, 30) claims that, in contrast to technical

lar orientation of the German—Humboldtian—university and the extended cultural sphere in the 19th century placed German universities in a position of comparative advantage *vis-à-vis* French and British universities[13]: French higher education institutions were primarily concentrated in Paris, and in Britain the old, established colleges dominated. In contrast to the situation in France and Britain, German institutions of the periphery profited from the center, in that the provincial universities where in a position to attract young faculty educated at major German universities; and the center profited from the periphery in that German universities were in a position to appoint faculty who had served in the "waiting room" of provincial universities (Ben-David 1991b, 66).

This comparative advantage of German science fizzled out with the beginning of the 20th century. The specific rules, regulations and cultures which had fostered scientific growth throughout the 19th century started to have the opposite effect. They had become, in the words of Ben-David and Zloczower (1962), a "strangling noose". To understand this 'strangling' effect, one has to bring to mind the original fostering force: (i) the diffusion of disciplinary orientations, brought about by the rule that, at any given institution, one field was represented by a single chair[14] (and aspiring scholars had to look for corresponding positions elsewhere); and (ii) disciplinary diversification which took hold after crowding effects in an established field became pronounced (whereupon aspiring scholars founded or moved to a new field). This two-legged force, diffusion followed by diversification, started to reach a ceiling when growth was eventually constrained by funding (or the willingness to fund).

Because laboratories or institutes were run by (or subordinate to) chair-holders, and because the successful laboratories were large (and expensive),[15] the funding of new chairs—particularly in the experimental sciences—would eventually become difficult. This constrained diversification and the "sellers market" of the 19th century, as Ben-David (1984/1971, 123) called it, came to an end. The funding of new individual chairs had become expensive; and there were not enough resources around to continue to fund the system in the manner it had been funded in the past. In short, "the competitive mechanism which had previously ensured the

(special) schools or high-schools (*Gymnasien*), "only universities are in a position to exert influence across the borders, and to affect education and formation in regions where the same language [i.e. German] is being spoken" (my translation). In particular, von Humboldt also voiced the advantage to attract foreigners.

[13]Paulsen (1902, 210) cites Lot (1892, 30) who speaks of a "scientific hegemony of Germany in all fields", and of "the fact that Germany alone produces more [research] than the rest of the world together" (my translation).

[14]Ben-David (1984/1971, 139) observes that the prevailing culture "encouraged professors in experimental sciences to regard their respective fields as personal domains". But in fact, the rule held in almost all fields, with the exception perhaps of mathematics and theoretical physics.

[15]"Toward the end of the century the laboratories of some of the professors became so famous that the ablest students from all over the world went there for varying periods of time. The list of students who worked in such places often included practically all the important scientists of the next generation" (Ben-David 1984/1971, 123).

prevalence of purely scientific considerations in the establishment of new fields was impaired". Where growth occurred within a relatively stable system of chairs, it "led only to a swelling of the ranks of assistants" (Ben-David 1984/1971, 131).[16] Growth based on the described two-legged force was still possible in relatively inexpensive fields, such as mathematics and (theoretical) physics (these were the times before CERN or similar installations of high energy physics), and it was in these fields that German science retained a relatively dominant position that was to last until about 1933.[17] To cope with the expanding costs of research in the laboratory-intensive natural sciences, and in response to the new American competition, the "Kaiser-Wilhelm-Institutes"—i.e. non-university research institutes—were formed in 1911.[18] With this move, the old Humboldtian ideal of the unity of teaching and research, still upheld today in the American universities, was left behind.

We are now in a position to summarize Ben-David's concept of scientific growth. Growth started with the institutionalization of the scientist's role and the transformation of a pre-modern science into a modern science around the time of the end of the *ancien régime* (or the founding of the University of Berlin in 1809). After the first third of the 19th century, German science was in a particular position to diffuse and subsequently to diversify, after which the process of diffusion and diversification could start again. The diffusion-diversification process was active throughout the entire 19th century and brought German science into a hegemonic position. The end of this process at the beginning of the 20th century was self-inflicted. The same rules, regulations and cultures which were responsible for the unprecedented growth of German science brought science of this sphere into—comparatively speaking—a state of stagnation (where it practically has remained ever since).[19]

[16]This is still the situation as we find it today. The swelling of the ranks of assistants has found a counterpart in the swelling of the ranks of students (Herbst et al. 2002).

[17]Because mathematics has subdivided and specialized early and does not require large ranks of assistants, mathematics flourished (and still flourishes) in countries like France, Great Britain, Russia, Hungary, et cetera with a mathematical culture and tradition.

[18]Now "Max-Planck-Institutes". Prior to 1911, the *Physikalisch-Technische Reichsanstalt* was founded in 1887 in Berlin-Charlottenburg, but this Institute was conceived to adopt a service function to unify measurement standards, not a research function.

[19]Ben-David ties this stagnation to the organization of German higher education, and to the specific 'feudal' form of its setup. He writes, "It is doubtful [...] that academic self-government contributed positively to the adaptability of the German system" (Ben-David 1984/1971, 120); furthermore, "[r]ather than change their structure so as to be able to take full advantage of the expanding opportunities, the universities adopted a deflationary policy of restricting the growth of new fields and the differentiation of old ones. Although the number of students and staff increased, and although there was an even greater increase in the expenditure of the universities because of the steeply growing expense of research, no modifications were made in the organization of the university" (*op. cit*, p. 129).

Diversification, Departmental Structure, and Anglo-American Science

Thus far we have concentrated on German science which had occupied a central place throughout most of the 19th century. German science, as Joseph Ben-David reasoned, entered a state of relative stagnation before World War I and definitely before 1933. The main competing spheres of German science at the beginning of the 20th century were those of Britain and the US: Britain had a range of well established universities that continued to be attractive and that had served, together with the German university, as role models for the newly established graduate schools which were starting to be founded in the US in the last 30 years of the 19th century.[20] Because US higher education and science were underdeveloped, a fair number of American students and scholars had been attracted to German universities to pursue training or advanced studies.[21] Upon returning to their homeland, they attempted to emulate the German university, at least insofar as they were able to exert influence on their own institutions, but this emulation proved to be imperfect. The college system imported from Britain was retained and fused with the newly formed graduate school introduced from Germany. In fusing the British with the German system of higher education institutions, the old British influenced college as well as the associated departmental system were kept, and the German chair system was not adopted.

By 1870, the time the first graduate schools were formed in America, US science was very far behind the development of German science. But the subsequent spurt in higher education during the years of the Gilded Age, funneled by a rapid industrialization and urbanization that was to spread across the continent, was impressive indeed. As in Germany before, US higher education and science were in a position to expand, to grow. Colleges were founded, existing colleges consolidated or upgraded into universities, universities and institutes of technologies were formed, standards of education and scholarship raised, and professional organization established. By the time of World War I, Anglo-American higher eduction and science had become a serious contender to German's dominating role in science.

The exact dating of the period during which the baton of the leading science nation was handed over to the US is not important in the present context. Important is Ben-David's observation that US science was in a position to expand and to grow continuously, without subsequently falling into stagnation. This ability to grow is attributed by Ben-David to the specific setup of the US university, public or private, and in particular to its departmental structure. It appears evident that the departmental structure, as opposed to the German chair system, formed a necessary condition for the ability of the US higher education and science system to expand, but it was not sufficient. A range of other factors was necessary as well.

[20] Harvard University founded its Graduate School of Arts and Sciences in 1872; Johns Hopkins University, the first fully fledged new university with a graduate school, was founded in 1876.

[21] See e.g. Schwinges (2001), and in particular Turner (2001).

One of the basic factors that distinguished the US university from its German counterpart was its autonomy that extended beyond the three established 'freedoms'—the freedom to learn, the freedom to teach, and the freedom to do research—with implications regarding governance, management, staff and personnel recruitment, funding, and the admission of students.[22] The exercise of these extended freedoms, plus an early notion that teaching implied a close interaction of teacher and student, brought about a higher education system that differed markedly from that of the German university. Institutional diversity became pronounced, and decent faculty-student ratios were the norm. Hence, once the US research university entered the scene around 1870, diffusion of the research university concept was associated, practically from the beginning, with a disciplinary diversity as well as an institutional diversity, both of which were in a position to develop in parallel, in a permeable system of higher education. Compared to the German university this new system of higher education turned out to be far less constricting as far as the growth of science was concerned.

The fate of German science was not shared by American science. Indeed, US science had laid the groundwork in the later portions of the 19th century through its formation of research universities, molded as they were on the German model, but fused with the idea of the British college. US, like German, higher education spread by the first two processes indicated above (p. 16f), but it did not experience the constraining, strangling effect implicit in the German development. US higher education developed into a diversified system (Clark 1997; Trow 1997), with few but highly successful research universities.[23] These research universities, like all universities in the US, held onto their early teaching orientation in their organizational setup, and they retained—from their inception in the late 19th century until today—decent, practically non-varying, faculty-student ratios. The implication was that the US research university was not replicating the internal structure of the German university nor the status and role of the German professor. It opted quite naturally for a collegial, departmental structure (which did not have to obey the rule of one field, one faculty member). Ironically, perhaps a century later, the initial teaching orientation of US institutions proved decisive with regard to their research productivity (Herbst 2004).

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[22]The fourth freedom, initiated through the decision of the US Supreme Court Justice Felix Frankfurter in 1957 (in the Sweezy vs. New Hampshire case), namely the freedom of a university to select "who may be admitted to study", was not that important in the 19th century, and it may have had the effect of a *numerus clausus* used to exclude Jews who where highly overrepresented at the universities of that time (Paulsen 1902, 195f), at least in the German context. Frankfurther's edict became constructive in light of the various anti-discrimination laws. See in this respect also Oren (2000) and Karabel (2005).

[23]Research universities in the US constitute only roughly 2–3 % of all tertiary education institutions. In addition, there are selective undergraduate institutions, or colleges. In contrast, almost all universities in Europe aspire to be research universities, but fail to reach the necessary effectiveness.

The Missing Link

Ben-David had observed that the center of science had moved from Europe to the US, and it has remained there ever since.[24] In order to reflect on this, I shall try to spell out factors (beyond the forces that were already mentioned) that may be seen to have brought about and sustain this situation.

I shall start with the sustaining forces within Europe. The prospects are that we cannot expect a basic change in European science or higher education policies in the near future, but in order to assess such an observation we may speculate about its underlying cause. European nations, or Europe as such, may have learned that it does not pay to be first in science. While it was difficult to regain the coveted position of the premier science region of the world after World War II, this now emerges as a goal not worth pursuing. Europe seems quite content playing a second fiddle, in spite of all the declarations of the various commissions of the European Union (see pp. 203f). It appears more important to retain historically grown academic cultures than to adapt those to improve productivity or proficiency.[25]

Economic growth and prosperity are said to be linked to science (and higher education) in the form of a "linear model" (Hands 2001, 364): science → technology → improved or new products → social benefits or prosperity (or: higher education → knowhow → entrepreneurship and inventions → social benefits or prosperity). However, science, it appears, is not directly tied to prosperity. The relation between scientific development on the one hand and economic well-being or prosperity on the other is rather tenuous.[26] There are regions that profit from the existence of strong universities (Saxenian 1994; Moscovitch et al. 1997). In Israel, and perhaps also in Singapore and Switzerland, there are conscious and largely successful policies designed to link science and higher education on the one hand and economic development on the other. However, an *avant-garde* in scientific development, a center of science in Ben-David's terms, cannot easily be translated into socio-economic advantages. Indeed, because the relationship is tenuous, it may pay to jump on the bandwagon and play a second fiddle: the center invests and explores, and because information, findings or know-how travel that easily, because information is a non-rivaled—public—good, the periphery may be in a position to cash in on the development and the insight of others.[27]

[24] This observation is in large measures undisputed by leading scholars and major institutional rankings; see e.g.: Da Pozzo et al. (2001), CEST (2002, 2004), www.leidenranking.com and www.scimagoir.com.

[25] See footnote 19.

[26] Prosperity is the result of various factors and related to a range of layers of educational achievement within a society. Higher education forms just one of these layers, and scholars and scientists are a mere subset of people associated with higher education. Germany's present relative economic success does not appear to be related to its relatively week science achievement, and Britain's week economic performance does not appear to be related to the relatively strong performance of its major universities.

[27] A rigorous privatization program for information (and associated extended patenting practices) would do more harm than good: it would greatly stifle economic development.

Next, I should like to address the question of why it appears that the departmental structure and the diversified nature of the US university has become instrumental in fostering productivity—or ‘progress’. Progress, in the notion of Philip Kitcher (1993, Chaps. 4 and 5), is linked to content: to some form of ‘truth’; to the idea that scientific theories evolve so that the more recent versions have generally more explanatory power than those that came to be replaced; and that newer theories tend to incorporate older forms as special cases. Truth, in this reading, need not refer to a matching of some external phenomenon and corresponding explanatory, descriptive theories; the search for truth need not be confined to the natural sciences. Truth may also relate to problem solving, to engineering or the sciences of the artificial. In that case, truth can easily be conceptualized and tested[28]; and in a practical sense, truth may also refer to a good portion of the social sciences and the humanities.[29]

I should like to continue the discussion on Kitcher’s notion of progress, because it has bearing on our review of Ben-David’s ideas but, for brevity in this introductory note, I need to abandon the focus on content. Instead, I shall dwell on proxies of content, namely on scientometric indicators, knowing, of course, that a replacement of content by indicators is, at best, a crude approximation. A more elaborate, i.e. economically or structurally operationalized handling of the link that ties progress to content might actually call for an entirely new line of research.[30] Kitcher pursues an inquiry which is central to Ben-David’s legacy, namely the organization of cognitive labor (Kitcher 1993, Chap. 8). In this context, Kitcher raises questions regarding the role and function of authority, cooperation, entrepreneurship, prestige and credit, and regarding their effects on innovation and progress. He reasons

> “[. . .] that there are advantages for a scientific community in cognitive diversity. Intuitively, a community that is prepared to hedge its bets when a situation is unclear is likely to do better than a community that moves quickly to a state of uniform opinion” (p. 344).

And, in a different section within his treatise, he observes that

> “[s]ometimes in the history of science, fields split, merge, or give birth to hybrid progeny” (p. 91).

In other words, Kitcher’s notion of progress and Ben-David’s idea of growth are related, and they are both tied to scientific—or cognitive—diversity: diversity breeds growth or progress.

Both Ben-David and Kitcher maintain that diversity can be seen as a necessary condition for scientific growth or progress. Furthermore, various aspects native to the US science and higher education system appear to serve this end[31]: a diver-

[28]In the way engineering can conceptualize or test the load bearing of a bridge, or theoretical operations research can conceptualize or test the efficiency of an algorithm. Beware, however, that a test is never final: the notion, in antiquity, of a flat world was in line with tests available and did not contradict experience.

[29]Namely to those parts that can reasonably be conceptualized—or operationalized—and tested.

[30]Indeed, this entire anthology can be understood as such a call.

[31]Tendencies come into view that work against this ‘native’ tradition, with possibly deleterious effects which need not be spelled out here.

sified higher education system with relatively few, but effective, research-oriented universities; the departmental structure and the collegial culture on which research universities are based; the funding of scientists, rather than institutions, for research work (see Chap. 8); the substantial autonomy of institutions or institutional systems; the freedom to recruit and select, and an associated culture to include and serve, students, rather than to screen them out to protect standards; decent faculty-student ratios (and, by implication, also decent faculty-staff ratios); implicit policies guarding against an overpopulation of PhD's (and an associated lowering of academic standards), including the spread of signaling effects; et cetera. Hence, it can be hypothesized that what we have termed the 'native' aspects of US higher education and science[32] does serve diversity, and that diversity in turn serves growth and progress.

A broadened sociology of science, an economically or structurally operationalized system of theories, linking science institutions with scientific growth or progress, as alluded to above, has yet to be developed. But major elements of such a system of theories do exist in the works of classical sociology of science or the economic investigations on research and development. Economic issues regarding science cover a number of foci which could be dealt with here,[33] and they cover roughly three domains that pertain to (i) the individual researchers, (ii) research institutions or institutional systems, and (iii) societies or nations of which higher education or research systems are part. Not all these aspects appear equally relevant in the context of a reflection on Ben-David's research, and I shall concentrate my focus on the second domain, with only occasional forays into the first and the third.

Productivity Issues

The general focus of research which is loosely covered by an economics of science does not concentrate on the domain that is in focus here. My anecdotal impression is that most research pertains to domains (i) and (iii), and the domain (ii) is the leased researched. With regard to issues within (i), there are many studies covering the interplay between the research productivity of scholars and individual attributes like age, life cycle, gender, motherhood, ethnicity, and basic training, most of which have only scant systemic impact. Furthermore, many links between research productivity and attributes of individuals are pretty much self-evident. A good basic training early on in life translates into a decent research productivity later on, motherhood (or heavy teaching loads) may reduce research volume but not necessarily the quality

[32] To be found also, at least partially, in select non-US institutions or institutional systems.

[33] Paula E. Stephan (2012) refers e.g. to incentives and reward systems, to competition, to inequality, to academic salaries, to the relationship of salaries and productivity, to financial fruits of inventive activity, to patenting, to start-up companies, to the cost of equipment and infrastructure, to the support from industry, to nonprofit foundations, to self-funding, to fund allocation systems, to the educational market, to earnings of graduates, to the relationship between science and economic growth, and so on, and there are a host of publications which deal with each of these individual areas of interest.

of research, vital results in mathematics or physics are normally obtained during the early stages of a scientific career, et cetera.[34]

The other domain where one can observe relatively heavy research activity is the third. Here, studies link the *Zeitgeist* with research output: the Cold War with the heavy US federal support for basic and applied sciences; science funding, public or private, with economic prosperity; public support for higher education with achievements in science; the interlinking of higher education and society with progress or knowledge production; et cetera. Many of these studies are more or less historiographic, but their focus is on economies—or on policy sciences.[35] Other inquiries are of an econometric nature, linking input (manpower and capital) with output (patents, publications, citations). Exemplary in this respect are those of Zwi Griliches (and his associates). However, not all of these treatises are without their pitfalls, and some are easily misleading. There is a range of studies by international organizations that fall into this category in which nations are compared on the basis of a broad spectrum of indicators pertaining to educational achievement or research performance.[36]

Focusing on the second of the three domains mentioned above, I shall concentrate on a number of theses and associated corollaries. While organization concepts have been around for some time, economics started to look into the black box of organizations with the advent of the theory of games (von Neumann and Morgenstern) and the theory of communications (Wiener and Shannon). Early in the 1950s, Jakob Marschak (1955) and Roy Radner (1955) postulated elements of a theory of teams that were subsequently expanded (Marschak 1957; Marschak and Radner

[34]In an unpublished paper (Herbst 2000) I had claimed that gender equality in higher education is indicative of quality in higher education in general: quality implies equality, and equality implies quality (that is, gender issues have a systemic dimension). My argument here is that although gender issues have a systemic dimension, their impact (on institutional performance) is relatively difficult to measure. My own hunch is that as long as gender equality is not really implemented and 'lived' in an institution, the research environment—its setup, and governing or management structure (discussed as part of the second domain)—is suboptimal for everyone (i.e. not only for women).

[35]See in this respect also Burton Clark's concept of a "Triangle of Coordination" regarding state authority, market, and academic oligarchy (Clark 1983, Chap. 5) or Etzkowitz' and Leydesdorff's "Triple Helix" concept of university-industry-government relations (Etzkowitz and Leydesdorff 1997). Gibbons et al. (1994) postulated even a new mode of science ("Mode 2"). It is true that higher education has changed dramatically during the past decades (this is why Martin Trow is a co-author in Gibbons' book); but the claim that science moved from a Mode 1 to a Mode 2, i.e. from an academically and disciplinary focused science to a more practice-oriented, commercial, context-driven and trans-disciplinary oriented endeavor, is not only bold but perhaps also oversimplified: it appears that both modes were present—and interlinked—in modern times (see also Chaps. 4 and 5).

[36]The drawback of these studies is that the link to the institutions or the institutional systems get lost (or that normalization was performed in an inappropriate way) and that they compare averages that pertain to nations of grossly dissimilar weight, where the small countries (e.g. the Scandinavian nations, Israel, Switzerland) are likely to show up comparatively well. If one were to compare small countries with regions within the US, e.g. with California or the Boston metropolitan area, the results of such comparisons would most likely be very different.

1958a,b, 1959). This theory was to look at various members or ‘agents’ of a ‘team’ (e.g. a firm, an organization) who had different tasks (or roles to play) but worked for a common goal; and it was meant to suggest optimal informational structures when knowledge and decision-making powers were unevenly dispersed among team members (Arrow 1985). In the 1970s and 1980s, team theory was further developed in the context of systems or control theory (Basar and Bansal 1989).

The idea to look into the black box of organizations was quickly absorbed in a Germany concerned with *Wiederaufbau* and the reformation of its research apparatus (Krauch 2006).[37] However the focus there, following Marschak, was not on abstract firms or organizations but on the work of scientists (Bahrdt et al. 1960). Research was portrayed as ‘work’ that had passed historically through different stages[38]: the stage where research was basically produced by the individual researcher; the stage of a hierarchically organized and laboratory based research that was characterized by a division of labor (in the sense of a manufactory); and eventually the stage where research was to be collaborative, inter-disciplinary, and team based.[39]

Bahrdt et al. (1960) saw modern society as being confronted with many complex problems of vital importance, and they were looking for organizational forms within which such problems could be addressed. The sciences themselves diversified to address new problems (atomic energy and space technologies, operations research and management sciences, technological forecasting and assessment, environmental sciences, cybernetics, general systems theory, et cetera),[40] but the framework within which the problems were to be addressed remained untouched. They argued against hierarchically (or bureaucratically) structured research groups that were frequently the norm at the time (at least in Germany), they decried the corresponding pseudo-feudal work arrangements which bound together student apprentices, research assistants and principal investigators (see Chap. 6), and they favored heterogeneity and inter-disciplinary approaches (or complementary expertise among researchers).

Horst Rittel (1965) followed the lead of Marschak to focus on the organizational *interna* of research groups and their associated embedding in the wider context. In Bahrdt et al. (1960, 27–32) he already listed various features of team work[41]: teams

[37] The reformation efforts were short-lived.

[38] The emphasis here is slightly different from that of Ben-David: whereas Ben-David tied (modern) science (and research) to the emergence of a new role (i.e. that of the scientist), research (and science) is tied here to the labor associated with that role. Bahrdt et al. (1960, 19) also perceive, following Max Weber, a certain parallelization between bureaucracy and research (or science), and they see close connections between the emergence of the modern state bureaucracy and the “emancipation” of European science (in Ben-David’s sense).

[39] The ideas here were formulated before the notions of trans-disciplinarity became fashionable—and way before Gibbons et al. (1994) and the “Mode 2” concept.

[40] This was also a time when faith in progress, and the belief to rectify or solve societal problems through the use of science, was strong and firmly embedded in the community of scientists and politicians.

[41] This was formulated 60 years before the rise of social media like Facebook or Twitter.

are often more productive than the corresponding number of individuals combined; team judgement is frequently better than that of individuals; information is economically spread and fed back; inter-disciplinary teams profit from an extended toolbox which is at their disposal; teams engage naturally in organizational learning[42]; teams are subject to corrective social control and are usually more focused in their work than individuals; teams can more easily use and share scientific appliances and equipment; teams benefit from a collective power of imagination.

Rittel's list appears to pit the team concept against the work of individuals rather than against hierarchically organized research groups, but it was only meant to be a list of features propagating team work (historically the third stage of research work). Unfortunately, the research questions of decades past have remained pretty much in obscurity, in spite of their relevance today for higher education management and research funding.[43] In the following, I should like to take up this line of thought and to dwell on one aspect of group work that relates to productivity issues and, implicitly, to questions of scientific growth and progress (idea diversity, innovation, etc.).

Scale and Agglomeration Economies

Consider a research unit with associated input and output.[44] Two theses might be addressed: that research is characterized by (i) economies of scale and by (ii) agglomeration economies (Saxenian 1994; Cooke 2002; Fujita et al. 2001; Fujita and Thisse 2002). The first of these economies is present when larger units exhibit higher productivity than their smaller counterparts, and the second is present when a clustering of research units benefits individual units and enhances their productivity. Economies—or diseconomies—of scale are said to be associated with the micro levels of institutions, whereas agglomeration economies are seen to be tied to the meso or macro levels of the respective environment.

Research performance is dependent on a range of factors or circumstances pertaining to individuals or institutions. Such factors may differ depending on the aim of research. Applied research generally calls for working conditions that differ from those for 'pure' research or basic science; some research is dependent on large infrastructures; and crash programs similar to a "Manhattan Project" are altogether a different matter.

In the setting of a university, research takes place as an extended and intergenerational form of learning. Experienced scholars, faculty members, tutor junior

[42]This is my 'modern' interpretation of what Rittel wrote under the titles of "Addition der Informationsfelder" and "Verbesserung der Lernfähigkeit".

[43]Rittel himself had abandoned this research line to deal with other aspects which were closer to his assigned tasks in Berkeley and Stuttgart (Rittel 1992; Protzen and Harris 2010).

[44]Under a research unit we can imagine an individual, a research team, an academic department, a university, a country, or a supra-national entity. In the present context, I shall confine my remarks to the levels of a research team, the academic department, or the university.

scholars, doctoral or post-doctoral students, and the learning takes place in a team (Ehrenberg and Kuh 2009). Advanced research never has the form of a simple know-how transfer in one direction, from teacher to student. All in the team profit from working together, junior and senior members. Junior members profit from the experience and guidance of their doctoral parent or senior research associates, they profit from interacting with each other, and senior members profit from the seeming naiveté and the unconventional, unmediated questions of junior members, or from their know-how in new technologies and their possibly different disciplinary backgrounds.

Inter-generational research is a native form for universities in that these are charged to educate and train future professionals, scholars and faculty members. But it appears not at all clear if that form of research is also the most proficient. Not all research cultures adhere to an inter-generational model centered in research universities. In some countries dedicated research institutes (DRI) are the locus of research, and universities are often seen primarily as training institutions. In such cultures, research institutes (DRI) or academies are meant to assemble the more experienced researchers. Dedicated research institutes are less encumbered by the burden of teaching and they work, by their design, with more mature professionals. But they are also likely to suffer from inherent subordination problems, restricting the autonomy and creativity of a good portion of researchers, and the constant inflow of fresh blood and turnover of talent is comparatively constrained.

The question which of the two models presented is better suited to foster research, the inter-generational model of the research university, or the intra-generational model of the dedicated research institute or academy, is difficult to answer. Obviously, the aims of research, pure or applied, and the type, ‘big’ or ‘small’, play a role. Furthermore, there is the question of the extent to which the two models are exclusive and to what degree, and under what circumstances, an overlap appears possible and advisable. In countries where the second model has (or had) some credence (in the USSR or Russia, in Germany, France or Italy), there is a certain tendency to link research institutes (e.g. CNRS or Max-Planck-Institutes) with universities, and in countries where the first model is prominent (US, UK, Israel, Switzerland) dedicated research institutes exist. Lastly, where research is ‘big’ and ‘pure’, a sharing of a science infra-structure (CERN, for instance) is common.

If the question regarding inter-generational (that is, university-based) versus intra-generational (i.e. academy or dedicated research-institute-based) research were insignificant, differences in research organization would not impact on research productivity, and the observed differences in research organization could be seen as stylistic, brought about by the different histories of nations and higher education or research systems. On the other hand, if differences of research productivity can be observed (National Research Council 1995; CEST 2002),[45] they might be attributable, in part at least, to the way higher education and research is organized (Herbst et al. 2002; Herbst 2004). If such a link is hypothesized, ways have to be

[45] See also footnote 24.

found to attribute differences in research productivity to the organization of research (Hurley 1997).

The remainder of this section shall focus on this question. Specifically, the role of economies of scale and agglomeration economies shall receive attention, and other factors, mentioned partially above, shall be ignored. The question shall be addressed as to what extent economies of scales and agglomeration economies are discernible. The question is relevant in a management context because research units have a size which is not regulated by 'natural' forces and only imperfectly regulated by market forces, and it is difficult to conceptualize 'optimality'. In fact, the forces that regulate and effect the size of research units—the appointment of faculty, tenure policies, grantsmanship, funding and ranking cultures, et cetera—are shaped by local mores and may have more to do with a guild system than with the fostering of research. Specifically, we shall focus on the following issues:

- economies of scale at the level of nuclear research groups;
- intra-departmental agglomeration economies;
- inter-departmental agglomeration economies as they pertain to the university as a whole.

There are only few studies that address these issues, but available data indicate that all three economies have their impact (National Research Council 1995; Ostriker et al. 2011). In light of these studies,[46] economies of scale at the level of nuclear research groups show an optimum which is generally reached with a group not exceeding 5–10 members[47]; larger groups tend to suffer from diseconomies.[48] Intra-departmental agglomeration economies are clearly visible for good sized departments, comprising roughly one to three dozen faculty members[49]; and inter-departmental agglomeration economies, as they pertain to the university as a whole, are visible as well: good departments profit from other good departments in various ways.

If one contrasts these findings with the picture of European, and specifically Humboldtian higher education institutions (Herbst et al. 2002; Herbst 2004, 2005, 2012), we obtain the following impression: US nuclear research groups tend to be smaller than research groups at corresponding European (i.e. Humboldtian) institutions[50]; US departments tend to be larger than their corresponding European counterparts[51]; and US research universities tend to play the role of an intellectual center

[46]Which refer to US research universities.

[47]Doctoral and post-doctoral students, plus the principal investigator. Optimal group size is dependent on the research field.

[48]Various factors are responsible for this phenomenon (Herbst et al. 2002). Larger groups may also have members who are 'active' as researchers, and those who are not, affecting (negatively) average output.

[49]That is the size that allows for proper inter-personal communication among faculty; smaller departments do not reach a critical mass; larger departments may profit from specialization and subdivision.

[50]Size of the group is defined by the number of researchers.

[51]Departmental size is defined by the number of principal investigators.

much more naturally.[52] The implication is that US institutions are characterized by higher research productivity[53] and by a higher thematic research diversity which is tied to growth (Ben-David) and progress (Kitcher).

That is the backdrop of the primary legacy of Ben-David yet to be developed: we need a sociology of science and higher education studies which address the link that ties the morphology of the institutions of science to their performance. A focus on research networks (see Chap. 9) ought to be seen as complementary to, not as a replacement for, such a course. In order to follow this path, it is necessary to develop a deeper understanding of the various science systems.[54] To negate Ben-David's seminal contributions to the study of science because of his purported antiquated views of science and society is shortsighted, as Ilana Löwy has pointed out (Chap. 4), and amounts to *das Kind mit den Bad ausschütten* ("empty the baby out with the bath water").

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[52]Larger research groups coupled with smaller departments (Scheme H) versus smaller research groups coupled with larger departments (Scheme D) are based (roughly) on the same number of researchers. One could claim, therefore, that the two setups are associated with the same research productivity (publications or citations per researchers, etc.). But this is not the case: diseconomies (associated with larger research groups) are coupled with reduced agglomeration economies (of smaller departments); and productivity advantages (associated with smaller research groups) are coupled with increased agglomeration economies (of larger departments and the greater diversification of the university). Hence, Scheme D tends to outperform Scheme H (Scheme D is portrayed here as the ideal-type of the US research university; but it can also be found in research universities of other nations, e.g. in Canada, the UK, and Israel).

[53]The notion of 'research productivity' needs to be operationalized properly. I have mentioned "publications or citations per researcher" as possible measures, but occasionally one finds also the measure "publications or citations per principal investigator", a possibly improper normalization in the context of research groups of unequal size, with misleading incentives in favor of large research groups.

[54]As Merton has already observed more than half a century before (see p. 192), there is a clear lack of research focusing on the workings of science and higher education systems, particularly in Europe. Introspective sciences, i.e. the sciences of science (meta-sciences), insofar as they would focus—in the tradition of Joseph Ben-David—on the working logic of science as a system, remain quite undeveloped. Few scientists show that focus, the corresponding scientific orientations are less quantitative and empirical than they could be, and current research has a tendency to dwell on (perhaps even irrelevant) details rather than on central aspects.

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Part II
Role and Ethos

Chapter 3
Ben-David's Critique of the Sociology of Knowledge and His Politics of Freedom

Yaron Ezrahi

Abstract This chapter focuses on Ben-David's normative notion of science and its relation to an 'open', rationality-focused society. Like a range of his contemporaries, Ben-David believed in the enlightenment function of science and in the associated benefits accrued to society. In the 1950s and 1960s, there was a social contract in place which accorded universities and research centers a high degree of autonomy, and academia and the respective societies were both profiting from this consensual arrangement.

This situation changed slowly during the subsequent years: the role of science and scientists changed, and the reflection on science transformed itself as well. Against this backdrop Ben-David's idealization of the autonomy of science had become somewhat anachronistic; but the science community is still called upon to share Ben-David's persistent passionate concern to preserve the integrity of science and the cultural foundations of the politics of freedom in contemporary society.

For Joseph Ben-David, sociology of science was neither work nor occupation. In the deepest sense of the word it was a vocation, a calling. In some respects it was Joseph's prolonged war against Fascism: his devotional enterprise of defending, as he saw it, the fragile cultural foundations of liberal democracy. Ben-David held that in some societies fascism and totalitarianism have largely been the results of the failure of the Enlightenment program. More precisely, these societies failed to accept and to culturally integrate the ethos of science and to legitimate the role of the scientist, not only as an autonomous man of knowledge but also in support of his role as a model of the virtues of democratic citizens as truth seekers and independent, judicious, rational people. Ben-David shared this view with his contemporary sociologists like Talcott Parsons and Edward Shils, with political theorist Allen Bloom and the writer Saul Bellow—and with other thinkers on the University of Chicago's Committee of Social Thought. For Ben-David the rise of modern science was both a cultural revolution and the evolution of core norms and practices for the non-violent

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settling of social and political conflicts. Consistent with the vision of the Enlightenment he passionately believed in the importance of science in the liberal-democratic political imagination and the commitment to a self-governing polity that generates uncoerced agreement by rational debate (Ezrahi 1990).

My discussion of Ben-David's conception of science and its relation to society and politics consists of three brief parts: First I shall refer to the main elements of Ben-David's sociological conception of science as a component of the liberal democratic order. Then I will discuss the question of whether the liberal democratic conception of freedom depends, as Ben-David maintained, on science as a model of non-violent resolution of political conflicts and why he thought that, unlike the sociology of science, the sociology of knowledge can subvert this latent role. Finally I shall discuss the strengths and weaknesses of Ben-David's conception of the relations of science and politics and its implication for the idea and practice of freedom in the modern liberal, democratic state.

Ben-David's Sociological Conception of Science

Ben-David's conception of the role of science in the defense of freedom has some affinity to Winston Smith, the hero of George Orwell's novel, "1984". In the novel Winston is confronted with the command "reject the evidence of your eyes and ears"; "it was their final most essential command", continues the narrator. Winston's

> "heart sank as he thought of the enormous power arrayed against him [. . .] The solid world exists, its laws do not change. Stones are hard, water is wet, objects unsupported fall towards the earth's center [. . .] With the feeling that he was [. . .] setting forth an important axiom, he wrote: 'Freedom is the freedom to say that two plus two make four. If that is granted, all else follows' " (Orwell 1949, 184–185).

Towards the end of the novel, brainwashed Winston is driven to deny this equation.

The existence of an objective world of nature and of compelling, irrefutable truths is here the weapon of the common man, the means with which he defends himself against ideological brainwashing by a totalitarian regime. Ben-David viewed science in a similar light, a basis of common sense epistemology that guarantees free thought against the politics of unreason. Aspects of this faith have been known at least since the middle of the 17th century. The goal of a fellow of the Royal Society of London like John Wilkins, or of a much respected foreign visitor to the Royal Society like Amos Comenius, was to advance a universal designative language that would uphold agreements on truths across geographical, social, cultural and linguistic boundaries. This ideal became an important element of the ethos of science to which Ben-David subscribed. Hence his opposition to the perspectives of thinkers and social scientists like Karl Marx, Karl Mannheim, Émile Durkheim and certainly Michel Foucault, all of whom tended to discern connections between the content of scientific knowledge and the macro-social location of the scientists. The manifest or tacit links of these connections to political power, ideologies and interests, appeared to Ben-David as leading to the politicization and relativization of scientific knowledge and to the erosion of the foundation of voluntary rational political culture. And indeed, if scientific knowledge is not extra-social and extra-political, how can it set limits to ideology and politics? This is why Ben-David thought that while it is alright to trace the class and institutional affiliations of scientists, particularly in investigating the social conditions congenial for their ability to achieve autonomy, the sociology of knowledge is quite another matter. Tracing knowledge to socio-political conditions is actually to challenge the fundamental liberal-democratic ethos about the separation of knowledge and society, the claims of science and partisan interests, and the autonomy of reason in relation to social and political forces. Ben-David insisted on separating the evolution of the social role of the scientists from the evolution of scientific ideas and theories. The former allows degrees of social determination whereas the latter is somehow autonomously determined by processes of interaction between ideas, theories and experiments of free thinking individuals.

It is from such a perspective that Ben-David insisted, in an 1981 essay focused on Mannheim and Durkheim, that

> "[t]he sociology of knowledge tradition assumed that all ideas, irrespective of their truth, were socially conditioned. According to the Durkheimian version of this theory, the basic categories of thought, such as conceptions of space, time, and causality, are rooted in language which reflects primarily social relationships [. . .] In the Mannheimian version of sociology of knowledge, the determining conditions are the perspectives of different groups, [. . .] Neither Durkheim nor Mannheim considered that their sociology implied scientific relativism" (Ben-David 1981, 42).

Ben-David explained that

> "The reason for the rejection of these theories by sociologists of science was that the empirical evidence on covariation between social base and the structure of knowledge was never satisfactorily established and because none of the theories of sociology of knowledge contained a satisfactory explanation of how, by means of what mechanisms, knowledge is determined by the social base" (*ibid*, p. 43).

Ben-David concluded his criticism of this kind of sociology of knowledge by complaining that according to this outlook

> "[t]here is no difference [. . .] between science, myth or ideology. Arguments and evidence only make sense within [. . .] partly verbalized and partly nonverbalized traditions evolved by particular groups" (*ibid*, p. 44).

He was especially concerned about the dangers of a sociology of science, inspired by philosophy of science, which "den[ies] the existence of pure observational language and asserts that 'facts' are determined by theory" (*ibid*, p. 44).

Durkheim would have vigorously rejected the allegation that his sociology puts myth and science on the same footing. In his often neglected 1913 lecture, published later as an essay entitled "Pragmatism and Sociology", he asserted, indeed, that in some respects science is no less a 'collective representation', a group-symbolic construct, than mythology; but unlike mythology, a scientific perspective on the world is more progressive because it arises in a kind of social structure which allows the rise of individualism which science, in turn, supports. He claimed further that, whereas mythological thought unites individual minds in "a single collective mind", science serves social thought and communications without undermining the separate integrity of individual minds. According to Durkheim, science achieves this result by substituting the unity of minds—of the kind realized in mythological thought—by the unity of reality as the object of many separate minds. Guaranteeing the unity of the world as an external object outside the thinking subjects enables science to "turn minds towards impersonal truths" (Durkheim 1983, 86–98; Ezrahi 1990, 173–180) without undermining the integrity of individuals as discrete minds or discrete thinking subjects. Durkheim can claim, therefore, that contrary to the kind of objections raised by Ben-David, it is not necessary to postulate socially undetermined external reality and socially autonomous thought to allow science to function as a building block of liberal democratic society. It is sufficient that reality as a collective representation resists modification by the simple effort of the individual will: "The impersonal truth developed by science [observed Durkheim] can leave room for everyone's individuality" (Ezrahi 1990, 175; Durkheim 1983, 88–91).

Science as a Model?

For both Durkheim and Ben-David, then, the affirmation of a hard factual reality as an object of many discrete minds is necessary to secure the foundation of autonomous liberal individualism and its significance in the foundation of a rationally guided society. But whereas Durkheim, consistent with his assumption about the primacy of the group over the individual, concentrates on a kind of a collective that can facilitate the evolution of the individual, Ben-David—like Robert Merton, Max Weber, and consistent with the tradition of methodological individualism, by contrast—assumes the primacy of the individual relative to the group and is concerned with the potential of the group to obliterate the individual. Hence the persistent tensions within and between the works of Durkheim and Ben-David concerning their respective conceptions of science and the social order. In the case of Durkheim it is the tension between individualism as a condition of science and a collective representation and his commitment to liberal democratic order. In the case of Ben-David it is between his commitment to an empirical sociology of science, his conception of scientific knowledge 'uncontaminated' by ideology, politics, philosophy and social epistemology, and his commitment to a conception of socially unconditioned scientific truths that can nevertheless constrain social and political behavior in a liberal society.

Without analyzing these tensions and their implications in detail I would like to argue, first, that Durkheim's sociology of science seems to me analytically cogent but oblivious to the potential relativizing effects of any public perception of science as socially conditioned, a form of a collective representation. Second, I contend that while Ben-David's argument about the total social and political autonomy of science as a human enterprise is untenable, even naïve, his concern that the social perception of the social and political conditionality of science can help undermine its authority is well founded and historically confirmed. While the social genealogy of individualism, and hence of science, is a very convincing hypothesis, the liberal democratic order has been largely shaped by different social ideas or popular images. Durkheim refers to these as collective representations of natural individual rights and of the individual as prior to, and existing independently of, society. To the extent that such socially diffused ideas of modern individualism become a collective representation, they acquire the power to be institutionalized and become a building block of a liberal-democratic order. When collective imaginaries of the social and the political order become hegemonic they generate performative scripts that help create institutions and behavioral regularities that approximate them. What is relevant to such processes is not the relative ontological status of individual and society nor which came first, but the socio-political effects of either, or any combination of the two when they become regulatory hegemonic collective imaginaries in a particular socio-cultural context (Ezrahi 2012).

Hence, Ben-David's concern, but not his theoretical perspective, highlights the fact that one needs to distinguish between the validity of a sociological theory and its potential effects as a source of popular ideas that influence the formation and de-formation of hegemonic collective representations. Ben-David's argument against Durkheim would have been more endurable had he suggested that, as a popular idea or in its vulgar versions, Durkheim's valid theory on the rise of individualism and science could be enlisted in support of reactionary communitarianism, and to weaken liberal individualism as a collective representation and a building block of the ethics and practice of modern science and liberalism (Merton 1973). One needs to subscribe to the—indefensible—belief that the production of pure objective knowledge is dependent upon rational, autonomous individualism; and upon a normative structure of science that is devoid of any ideological dimension to warrant a strictly, or narrowly, empirical sociology of science that negates the possibility of a sociology of knowledge. This case is only a particular example of the perennial question of whether or not the anticipated or unanticipated contextual implications and effects of social, sociological, economic or political theories do influence the very selection and constructions of these theories, and whether such influence forecloses the possibility of a conceptually cogent approach guided by strictly intellectual considerations of social phenomena like science and politics—or necessarily constitute a 'fatal' constraint on free theorizing.

The Relation of Science and Politics

I would like to argue that already during the 1960s, the insistence of sociologists like Shils, Merton and Ben-David on the potential of scientific rationality to enable unforced agreement or to constrain arbitrary power has begun to be anachronistic. While the polarities of science and ideology, rationality and irrationality, voluntary agreement and involuntary agreements had some substance in a universe framed by the European and the American Enlightenment, beginning with the rise of the mass democracies in the late 20th century such dichotomies do not make much sense. In our time, arbitrary governance is kept in check not by the authorities of science and universal reason but by economic and political stability. Moreover, contemporary politics in democratic states have been profoundly transformed by the fragmentation of public agendas, the rise of single issue politics, the impact of the mass media, the decline of political parties and the theatricalization of the political arena. These processes have obviously been uncongenial for the role of knowledge and rational arguments in public affairs (Ezrahi 2012). Paradoxically, political fragmentation and incoherent public policies have appeared more compatible with the democratic decentralization of power than large scale coherent reforms. As a matter of fact, such dispersal of powers—the power of contemporary world-economic interdependencies, of global public opinion mobilized by contemporary mass media, and the contemporary structural constraints on the centralization of political power even in authoritarian regimes—are probably more effective than reason or science ever were in diminishing the prospects of Fascism.

Aside from these conditions, Ben-David's idealization of the autonomy of science has appeared anachronistic the more science and its technological applications have become relevant to, and entangled in, controversies about the choices and execution of legislation, technologies and public policies. What politicizes science is not so much the perception of its social origins and conditions but often the redistributive material and political effects of its input into public affairs. Ben-David's attempts to shelter science from politics were doomed from the beginning, although the difference between low and high degrees of politicization, of scientific knowledge, research and institutions, may correspond to the differences between democratic and authoritarian regimes.

Highlighting many of the qualifications of Ben-David's vision of autonomous science, and his fear of the potential of the sociology of knowledge to undermine it, still leaves us with his persistent passionate concern to preserve the integrity of science and the cultural foundations of the politics of freedom in contemporary society. Driven by his personal experience with European—and particularly Hungarian—Fascism, Ben-David hoped to strengthen liberal democracy by his sociology of the role of the scientist in modern society and the study of the social conditions of the scientific community. It is fair to ask: what could be an appropriate response to these concerns in our time? I have no clear answer to this question. But certainly, any progress on these issues will require the analytical powers, the learning and the tenacity of contemporary scholars who will assume the difficult task to carry Ben-David's project further.

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Chapter 4
The Scientists' Role and Medical Innovations

Ilana Löwy

Abstract The chapter focuses on medicine and argues against the hasty dismissal of much of the classic sociology of science by a new generation of scholars. Ben-David tied the diffusion of disciplinary thought and innovation to his concept of the scientist's role. Bacteriology and psychoanalysis served as examples. In both cases, a 'role-hybridization' was said to be instrumental in the move from the original disciplinary field to the new focus. The chapter explores this diffusion process on the basis of two examples: screening for cancer; and prenatal diagnosis. In both innovations, the impetus to develop these novelties did not originate with a specific group of scholars or professionals—and specifically not in their intent to transcend a constricting 'role', as Ben-David might have suggested. Rather the innovations came into being as the result of an interplay of various stakeholders.

Why scientists do what they do? This interrogation, grounded in the double affirmation that scientists are professionals—and therefore can be studied using concepts and tools borrowed from the sociology of professions—and that they have a unique social role, was at the very center of Ben-David's sociology of science (Ben-David 1984/1971). The concept of "scientist's role in society" was popular in the 1970s and the early 1980s. Its subsequent disappearance may be related to the rise of new approaches to history, sociology and anthropology of sciences. Researchers associated with these new approaches argued that sociologists who studied science in the 1950s and 60s propagated an idealized—and for some a biased and self-serving—understanding of their object of study. They presented scientific research as a superior achievement of human spirit, an activity which invariably promotes rationality, a better understanding of the natural world, and an impressive increase in human well-being and human freedom.

New developments in history, sociology and anthropology of science radically destabilized an earlier view of scientific research. Researchers in these domains displayed the complexity and multifunctionality of the activity called science—or

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rather the sciences—and the diversity and heterogeneity of the group of people gathered under the label 'scientists'. By consequence, concepts such as "scientists' role in society" and "scientific ethos" came to be seen as conceptually simplistic and methodologically outdated. The hasty dismissal of these concepts, my text proposes, was premature. Researchers who studied science in the last forty years greatly increased the understanding of the things scientists do, the ways they do them, and the consequences of their activity, but they seldom asked why scientists act in specific ways—besides a general assumption that they, like other professionals, are interested in status, power, and money.[1] Today, too, we do not have good answers to the interrogation at the center of Ben-David's sociology of science: the reasons for the genesis and development of specific directions of scientific research.

The term "scientist's role" appeared for the first time in Ben-David's paper "Roles and innovation in medicine" of 1960 (Ben-David 1960). This article examined the rise of two new medical disciplines: bacteriology and psychoanalysis. Institutionalization of the sciences in the nineteenth century and the stabilization of investigation patterns, Ben-David argued, made radical innovation in established scientific disciplines more difficult. New ways to perceive scientific problems frequently originated in a need to solve practical questions. Such a need favored the emergence of a hybrid role of scientist-cum-practitioner, accountable to his original scientific community, but also to an external, practice-oriented reference group.

Both Pasteur and Freud, the founders of bacteriology and psychoanalysis, respectively, were socialized as fundamental scientists, and all their life adhered to the scientific ethos. Their interest in the solution of practical problems ("what can be done about disease?", "how should one deal with neurosis?") led them, however, to rely on reference groups outside their original scientific community. The result was the application of the scientific method to a new range of problems, the development of a hybrid social role of scientist-cum-practitioner, the enlargement of the scope of questions seen as legitimate subjects of scientific inquiry, and intensive efforts to diffuse new scientific practices. The success of the hybridization of roles, concepts and practices, their fruitful implantation in appropriate 'ecological niches', and the mutual shaping of new specialties and their environment, reflected, Ben-David added, general rules which govern scientific growth and, at the same time, unique historical developments.

Scientists studied by Ben-David, one should add, were not presented as selfless idealists interested only in revealing of secrets of nature or the increase of common good. They strived to be faithful to norms and values of the group in which they were socialized, but also looked for recognition, including financial one. One way to achieve these goals was to provide multiple reference frames for their research and to legitimate it in more than one way. Ideally, different reference frames should be complementary and should reinforce each other. This is not always the

[1]Researchers associated with the 'first wave' of "sociology of scientific knowledge" (SSK) did attempt to provide 'interest-grounded' explanations of scientific activities, but their analyses were often limited to efforts to link knowledge and facts produced by a given scientist to social interests of a group to which he belonged.

case, and occasionally attempts to frame one's work in different ways may backfire. For example, Louis Pasteur claimed that he had developed his process of brewing beer in order to help the French beer industry and to display the superiority of French scientists over the Germans'. He also patented his invention, securing himself a steady source of income. The newspaper *Le Figaro*, dismayed by the fact that a civil servant—Pasteur was at that time professor at the *École Normale Supérieure*—increased his personal fortune through research made with public money, called Pasteur explanation "an illegitimate union of patriotism and mercantilism" (Bayet 1986).[2] The dense networks that link today Pasteur's heirs, the present day biomedical researchers with the pharmaceutical industry, can be similarly analyzed in terms of co-existence of several reference frameworks.[3]

Ben-David initially claimed that he aspired to develop a fully generalizable sociological knowledge, not to investigate specific historical developments. He later recognized (for example, in the new 1984 introduction to the second edition of *Scientist's Role in Society*) that unique historical events are also appropriate subjects for sociological inquiry (Ben-David 1984). Ben-David affirmed, nevertheless, that he remained faithful to the sociologist's ideal of producing nomological, empirically verifiable, generalized statements, and was interested in social conditions that favor scientific innovation in general, not in the ways such conditions affect the content of particular innovations (Ben-David 1984). However, Ludwik Fleck's advice, that when studying scientists, one should pay more attention to what they do than to what they say they do, may be extended to social scientists too (Fleck 1929). Ben-David produced highly original historical studies which provided new insights about the general conditions that made medical innovations possible and, at the same time, illuminate the reasons for development of specific directions of scientific inquiry.

Science Studies and the Disappearance of Scientists' Role

Writing shortly after Ben-David's death in 1986, Gad Freundenthal attempted to reconcile Ben-David's ideas with those of promoters of sociology of scientific knowledge (SSK). In spite of appearances, Freudenthal (1987) argued, the two approaches are not radically different and it is possible to construct a fruitful synthesis of both. In one of his last papers, a discussion with Timothy Lenoir on the rise of physiology in 19th century Germany, Ben-David indeed affirmed that his approach—and the one inspired by the new sociology of science—may be seen as complementary research programs, one (i.e. his own) focussing on structural elements and the other

[2] My translation. Ben-David did not study this aspect of Pasteur's career; *École Normale Supérieure* is an elitist French higher education institution, founded to train high school teachers in the public school system, and today mainly training university teachers and researchers.

[3] On the close relationships between academic researchers and the pharmaceutical industry, and their consequences on the evaluation of therapies, see e.g., Angell (2004), Wilson (2009), Pietrantonj et al. (2009).

on contingent events. Both are equally important. If one wishes to understand the trajectory of a given ship, Ben-David proposed, one needs to study laws of hydrodynamics and principles of construction of vessels, but also the weather conditions and sea currents encountered by the studied ship in its travels. The same is true if one aspires to understand the development of scientific research in a given time and place (Ben-David 1986). Freudenthal (1987) was probably right when he proposed that the process of selection and integration that characterizes all scientific activity could have lead to an incorporation of distinct traditions in sociology of science of the 1970s and 80s into a single, synthetic view. Historical trends, however, are difficult to predict. The British "strong programme" in sociology of science and the US tradition in this domain were never fully integrated, and both lost their original impetus in the 1990s. They were replaced by different questions and approaches, loosely gathered under the term "science studies" or "social and cultural studies of science".

New studies of science and medicine, the anthropologist of science Sharon Traveek proposed, replaced the singular 'science' with the plural 'sciences' and with a new focus on multiple objects and practices. According to the 'old' view, scientific research is objective, neutral, and grounded in a single scientific method, based upon codified skepticism. Scientific reasoning proceeds by deduction and induction; and scientific knowledge is amassed progressively and cumulatively. The correct application of the scientific method led to better understanding and control of nature, while the application of scientific discoveries is the primary reason for the improvements in the quality and the duration of human life during the past two hundred years. The 'new' view of science proposes by contrast that there is no single, homogenous entity called science, but multiple, highly variable scientific practices. Each community of scientists collectively establishes the rules which define which experimental data should be taken as facts, which theories should be considered important, and how debates should be closed and consensus reached. Such rules vary greatly among groups of practitioners in different places and disciplines. Moreover, science and technology are social and cultural endeavors, which, like all human endeavors, can be conducted in different ways and can have a wide range of positive and negative consequences (Traveek 1996).

New approaches to the investigation of the sciences developed from the 1970s on by historians, sociologists, anthropologists, political scientists, and jurists, Traveek explains, greatly enriched our understanding of the ways science works. However, at least some variants of science studies replaced one orthodoxy about the nature of scientists' activities by another. While researchers in this domain loudly proclaim the importance of the plurality of patterns of scientific inquiry, in practice many seem to believe that there is a single corpus of accepted views on scientific practices and one 'correct' way to study scientists' activities. Science studies displayed the heterogeneity and complexity of the scientific enterprise. At the same time, as Sharon Traveek and others had pointed out, they made other aspects of this enterprise invisible (Pestre 2004). Among the latter, most of the elements shape the scientist's role such as it was defined by Ben-David: causal explanations, structures and institutions, economical and political variables, and values and norms shared by scientists as a group.

The disappearance of some topics of investigation and their replacement by others may be seen as an unavoidable consequence of scientific change. A shift in focus of a scientific inquiry frequently decreases the visibility of some elements and increases the visibility of others. Such a change may, however, be problematic when it leads to the disappearance of a central element of the studied question. One of the striking phenomena in recent studies of science is a growing invisibility of scientists as a group. The striving to show that there is no single, universally valid scientific method, and that science is but one human activity among many, did not support interest in the specificity of the scientific enterprise. A focus on the heterogeneity of scientific practices did not privilege a search for traits shared by all the researchers or for norms and values that inform on their actions. Studies of highly labile networks of actors and 'actants' do not favor investigations focused on the shared ethos of scientific communities, and on the intersection of rules that govern such communities with norms promoted by other social groups. Scientists described in investigations inspired by science studies often either display a highly individualized, idiosyncratic behavior or, alternatively, are presented as anonymous and, one may assume, quasi-interchangeable agents.

There are important exceptions to this rather hasty generalization. Today, too, biographers of scientists attempt to uncover the reasons for their heroes' choices and articulate developments on the micro, meso and macro level.[4] Researchers who study scientific communities of the past, or those who follow the rise of new domains of scientific inquiry, similarly are interested in the totality of elements which may account for the observed changes in scientists' activity. However, such studies are usually limited to interest in studied individuals or groups and do not investigate the conditions that make science, scientific practices or scientific change possible. One notable exception is Lorraine Daston's well known—but, alas, less frequently used—text *The Moral Economy of Science*. This text examines the notion of "scientist's ethos" in the light of recent efforts to historicize the constitutive elements of modern scientific practice such as the striving for precision, replication or objectivity (Daston 1995). Daston, like Ben-David, is interested in specific forms of moral, emotional and æsthetic elements adopted by the 'tribe' of scientists, and is attuned to scientists' emotions and values. In *The Moral Economy of Science*, Daston does not link, however, the development of scientific ethos to changes in larger social structures. Her scientists move in a world of noble, bourgeois or protestant values, such as honor, punctiliousness or introspection and adapt these values to their specific needs, but—at least in that text—they do not seem to be concerned by institutions and laws, hegemony and subordination, financial constraints and political developments. One of the strengths of Ben-David's approach was his ambitious attempt to bridge between developments on a micro and macro scale, and to provide means to investigate at the same time the universality of the scientific enterprise and its irreducible historicity and contingency.

[4] A typical example may be Darwin's biographies, e.g., Browne (1995, 2002), Desmond and Moore (2009).

Practical Problems and Scientific Knowledge

An important aspect of Ben-David's approach was his interest in the role of practical questions in stimulation of new directions of scientific inquiry. The pressure to provide solutions to practical problems, Ben-David proposed in his pioneering 1960 study, may favor innovations in science and medicine. Once successfully established, usually through mutual shaping of an innovation and its "ecological niche", an approach initially driven by a practical need may lead to radical changes in knowledge and social practices. A world before Pasteur and Freud, that is, without microorganisms and unconsciousness, was a very different place from a world with these concepts. The change brought by description of invisible organism and unconscious thoughts may be illustrated by the shocked reactions of people socialized in standards of Western hygiene when they visit places where such hygiene does not exist, or by the difficulty of people, who interpret human behavior in psychological and psychoanalytical terms, to conduct a meaningful discussion with religious fundamentalists who judge human behavior in terms of sin or redemption. A hybridization of roles of a scientist and a practitioner, one may propose following Ben-David, may have effects that go far beyond developments within the sciences. Accordingly, scientific domains directly linked with practical applications, may be different from those devoid of such direct links.

In his book *Trust in Numbers*, the historian of science Theodore Porter (1995) argued that some scientific disciplines, such as e.g. Assyriology or high energy physics, function mainly as a closed *Gemeinschaft* (Community) (Tönnies 2005/1887), relatively isolated from external pressure. Such an isolation does not mean that a given scientific discipline does not influence—and is not influenced by—cultural, economic and political variables. High energy physics has numerous links with military and civil uses of atomic energy, while Assyriology may play multiple roles in the highly volatile Middle East politics. However, in domains that function according to the *Gemeinschaft* mode, the production and validation of knowledge is relatively free from external pressures. External intervention is mainly exercised through the control of resources. This is, to be sure, an important factor, because it defines the scale and scope of research in a given area. High energy physicists need expensive instruments, and Assyriologists the possibility to conduct costly excavations. Nevertheless, members of these scientific communities are, as a rule, able to decide what counts as established 'facts' in their domain, which questions are interesting, what is the right methods to answer these questions, how new knowledge is validated, and how it is integrated into an existing disciplinary corpus.

Other scientific communities, Porter proposes, especially those linked directly with practical preoccupations, are closer to the *Gesellschaft* model, and are less insulated from external influences. For example, the growing tendency to standardize and regulate medical practices was driven, to an important extend, by the public's mistrust of profit-seeking doctors and industrialists (Marks 1997). The increasing role of *Gesellschaft*-type relationships in the production of scientific knowledge led to important changes in scientists' perception and self-perception. Such changes existed already when Ben-David had written *The Scientist's Role in Society*, but they were amplified in the last half century. When politicians and science administrators strive to develop a quantitative and presumably objective evaluation of scientists' productivity, including the humanities and social sciences, and insist on adequate economic returns for investments in the sciences, interest in the scientists' role in society—that is, in the mutual shaping of societal expectations and pressures and scientists' values and actions—can provide a valuable entry point to rethink recent changes in the scientists' tasks and status and reactions to these changes.

An interest in elements that affect decisions of scientists can also help to bridge studies of science, interested in local events, and those who follow developments on a macro scale. The recent focus on "mediating devices" in the sciences—experimental systems and scientific instruments, metrology and standardization, classificatory systems, rules and regulations—provided a fruitful way to link different levels of inquiry. Studies of mediating devices may also help to understand how scientific knowledge became universal in spite of the great diversity of local practices. However, such studies were usually focused on specific products or tools of scientific activity—instruments, techniques and inscriptions, techniques and instruments, publications and textbooks—not on people who produce them. They tended, therefore, to 'black-box' intentions and aspirations, and seldom fully pay attention to the ways scientists' values and preferences are shaped by economic, legal and institutional constraints and available technical solutions, or broad societal and cultural considerations.

What will change if, following Ben-David, a historical or sociological investigation will take as its starting point the reason scientists—as individuals, groups or members of institutions—elect to act in specific ways? Let's look at the history of scientific and technical innovations. Recent studies of innovations often focus on the innovation itself. Historians, anthropologists and sociologists who study "biographies of things" follow events that led to the development of a given innovation, its consolidation, diffusion (straightforward or difficult), sometimes its demise. Such studies often produce rich and fine-grained description of the innovation's trajectory. However, an approach focused on the biography of an innovation tends to see the innovation itself as self-evident. It can provide important insight why a given innovation succeeds and what the consequences of this success are, but is less well adapted to explain why this innovation came into being in the first place. It is also less well adapted to a critical study of scientific and technological change.

By contrast, when the choices of people who developed and promoted an innovation are taken as the starting point of a study, the innovation itself is not taken for granted any more. New scientific and technological developments are neither entirely predetermined nor fully contingent. They come into being when specific groups of stakeholders are able to realize their goals through their manufacture and diffusion. A focus on the reasons for stakeholders' decisions can open new ways to study the dynamic of scientific and technological change. Since people do things, it is important to look carefully who does what, what the existing power relationships are, whose interventions really matter, what are the structural constrains on scientific activities and how do these change with time. Such an approach can avoid some of the pitfalls of studies interested solely in human actors and non-human actants, directly involved in the production and diffusion of a given innovation, and provide a more accurate understanding of the genesis of this innovation and its consequences.

Diagnostic Innovations in Context

Screening for Cancer The history of two medical innovations, i.e. screening for malignancies and prenatal diagnosis, illustrate complex intersections between the trajectory of an innovation, stakeholders values and priorities, and broad societal changes. The rise of screening for malignant tumors is directly linked to efforts to assess the efficacy of treatments for tumors, and the rise of cancer charities and cancer organizations. Both developments were in turn related to the high costs of radiotherapy. Radiotherapy of cancer was developed in the early twentieth century and consolidated in the 1920s and 30s. This therapeutic approach offered the hope of cure of previously incurable tumors, and of alleviating the suffering of patients who could not be permanently cured. This was, however, a costly solution: radiation therapy employed radium, a rare and pricy element, while efficient X-ray treatment was conducted with high voltage X-rays machines, an expensive equipment. The need to use these therapies favored specialization in oncology and the concentration of patients and experts in a small number of centers, able to purchase powerful X-ray machines and purified radium (Pinell 2002/1992). Moreover, some cancer specialists argued that the expense was not justified, because radiotherapy was no more efficient or safe than surgery. Other experts promoted the opposite view. In the 1920s, the striving to evaluate the efficacy of costly interventions favored the development of cancer statistics (Moscucci 2007). The accumulation of quantitive data led in turn to shifts in the perception of cancer therapies.

In the early twentieth century, many surgeons noticed that operations for localized tumors tend to be more successful than those for extended and disseminated ones. On the other hand, malignant tumors are highly variable: some patients operated for small tumors died nevertheless rapidly from a generalized disease, and others, operated for larger tumors, did well. Collection of data on long term effects of therapy, coupled with efforts to standardize diagnosis and the staging of malignant tumors, helped to dissociate tumor's size from the presence of disseminated disease. Aggregate data clearly indicated that localized tumors could sometimes be cured, and that the chances for success were roughly correlated with the tumor's size. By contrast, a disseminated cancer was invariably lethal (Lane-Claypon 1924, 1927). In the early twentieth century, the development of new surgical techniques and of radiotherapy led to hopes of a rapid improvement of rates of cures of malignant tumors. Reliable statistics of outcomes of cancer treatments put an end to these hopes; they indicated that the great majority of cancer patients, including those treated by best experts who employed the most advanced techniques, did not survive. These disappointing results were attributed to the fact that the patients arrived "too late". The only realistic hope to reduce cancer mortality, many specialists explained at that point, was to promote an early detection of malignancies (Bloodgood undated, ca. 1916).

Cancer experts aspired to overturn the popular notion that cancer is always a fatal disease. Their goal was, however, to persuade people to consult immediately after noticing suspicious symptoms, not to promote mass screenings of healthy populations. The rise of such screening is linked to the rapid development of cancer organizations. In the late nineteenth and the early 20th century, cancer charities provided services to indigent cancer sufferers, and to patients rejected by their family because of the severity or unpleasantness of their disease. Charities provided these patients with an equivalent of today's hospice care. However, from mid 1910s on, cancer organizations begun to collect money to finance the equipment for radiation therapy. This new activity transformed traditional charities, initially focused on palliative treatment and "good death", into organizations that worked in close alliance with cancer experts. A growing interest in the promotion of cures led them to espouse the early detection cause, and to adopt education about cancer as one of the main axes of their activity.

Cancer organizations energetically promoted the slogan, "if detected early, cancer can be cured". If one reads this phrase carefully, it merely states that while some localized malignant tumors are curable, all the disseminated ones are deadly. This was not, however, the usual interpretation of this slogan. It strongly hinted that a patient who knows what early manifestations of cancer are, and who promptly consults a competent doctor when suspicious symptoms became visible, will not die from cancer. It also indirectly implied that patients dying from cancer may be at least partly responsible for their fate.[5] Such an interpretation was supported by a double meaning of the term 'early': early in the natural history of a given tumor, and early in relation to first symptoms of the disease. In some cases—for example in a slow growing skin cancer—these two meanings are indeed close, and the first perceptible signs of a tumor indicate the presence of an early stage of natural history of this tumor. In other cases, however, the two meanings of 'early' are dissimilar. A cancerous growth (say, suspicious lump in the breast, a swelling of a salivary gland) can be small because it was detected early in its development—and in this case it may be easier to cure than a tumor detected after it had spread. But it may also be small because it belongs to a slow growing variety and has a good prognosis for this reason, not because the patient saw a doctor immediately after a suspicious symptoms was noticed.

Activities of cancer charities increased the visibility of cancer as a social problem, and favored the transformation of early detection of malignant tumors and precancerous conditions into a public health issue (Koss 1989; Strax 1989). Public health policies were grounded in the principle that healthy people should be regularly screened for the presence of early cancerous growths or premalignant changes in the tissues (Löwy 2007). The rise of mass screening for cancer in the second

[5] Breast cancer—an accessible and visible tumor—was especially important target for such view. See: Aronowitz (2001), Gardner (2006), Aronowitz (2007).

half of the twentieth century promoted in turn the development of new visualization techniques such as mammography, colposcopy and colonoscopy, and the rise of new medical practices. The extent of the diffusion of these material and social innovations was not uniform.[6] Nevertheless, in the early twenty-first century, screening for cancer (especially for female cancers, breast and uterus) became a routine practice and a self-evident public health intervention.

Prenatal Diagnosis Developments that contributed to the rise of screening for malignancies—the high price of radium and powerful X-ray apparatus, controversies on efficacy of radiotherapy versus surgery, and the growing importance of cancer organizations—were directly or indirectly related to the disease of cancer. The main drive for the development of techniques of prenatal diagnosis was an event disconnected from such diagnosis: the legalization of abortion. Decriminalization of abortion was not related to the question of the 'quality' of an unborn child. Its aim was to allow women to control their bodies and free them from the burden of unwanted maternities. Women fought for a right to refuse the birth of 'a child', not of 'this child'. The legalization of abortion made possible, however, the use of already existing biological and medical approaches to predict the fate of an unborn child. Families of children with severe diseases or disabilities and obstetricians and pediatricians who wanted to alleviate the plight of these families provided a 'push' for the rise of new medical specialties, and new medical technologies, while the liberalization of abortion opened spaces for new scientific, medical and industrial activities.

The development of prenatal diagnosis originated in an encounter, at the 'right' historical moment, between three distinct techniques: amniocentesis, ultrasonogram, and the visualization of human chromosomes (Cowan 2008). The development of obstetrical ultrasound was an offshoot of efforts to adapt a military technology—ultrasound—to medical uses. The goal of the first ultrasound tests performed on pregnant women was to see if the woman carried one or more fetuses, verify the size of the fetus in order to predict potential complications during childbirth, or evaluate the age of pregnancy. The technique was initially much too crude for a diagnosis of fetal malformations (Blume 1992; Tansey and Christie 1998). Amniocentesis (earlier called amniotic tap) was developed to alleviate pregnancy-related problems. Some women suffered from excess of amniotic fluid and doctors learned in the 1930s to insert a needle in the abdomen in order to aspire some of the fluid and relieve the pressure, dangerous for the fetus. They also learned to inject physiological solution to women who had insufficient amount of amniotic fluid, again, a condition that put the fetus in danger. In the mid-1950s, the sampling of amniotic fluid was introduced as a diagnostic technique that makes possible the detection of mother-fetus Rhesus incompatibility. When such incompatibility was suspected (often because a previous child was sick or died), optical examination of the amniotic fluid—a method developed by the British obstetrician, Douglas Bevis—allowed to

[6]For example, people in the US are screened for cancer three times more often that in Western Europe. See: Howard et al. (2009).

evaluate the extend of damage to the fetus. If the test revealed important destruction of fetal red blood cells, obstetricians could induce an early birth or prepare an exchange transfusion immediately after the child was born (Zallen et al. 2004). With the increase in the number of amniotic taps, doctors started to use ultrasonogram to visualize the inserted needle and to limit the risk of accidentally hurting the fetus.

In the late 1950s, scientists also learned to study human chromosomes. Until that time, biologist studied chromosomes of laboratory animals, but had no reliable way to investigate human chromosomes. Until 1956 they even did not know that humans have 46, and not 48, chromosomes. That year, the development of a method to visualize human chromosomes by Albert Levan and Joe-Hin Tjio opened the way to the description of inborn anomalies linked with abnormal number of chromosomes (aneuploidy). In 1959, researchers had shown that the Down syndrome was the consequence of the presence of three chromosomes 21, and that Turner and Klinefelter syndromes were linked to the presence of an abnormal number of sex chromosomes (respectively, 45X0 and 47XXY). Inborn anomalies attributed previously to endocrine disorders were redefined as errors of cell division (Harper 2006).

These observations potentially opened a possibility to diagnose aneuploidy before the birth through the analysis of fetal cells in the amniotic fluid. Such an analysis would have been, however, pointless if nothing could be done to alleviate the newborn child's condition (by contrast, the precipitation of birth of fetuses who suffered from Rhesus incompatibility greatly reduced the number of deaths of infants born from Rh-sensitized women). An additional and indispensable condition for widespread diffusion of prenatal diagnosis was the legalization—or at least, a wider tolerance—of abortion. The first prenatal tests were made on women who had affected children, and who decided to abstain from a future pregnancy, or those who made the same decision because of the presence of hereditary disease in the family. The possibility of a prenatal diagnosis followed by termination if the fetus inherited the familiar disease gave these women the courage to became pregnant (Tansey and Christie 2003). Prenatal testing was initially the answer to a demand of women and couples who had a traumatic family history. During a second stage, it became a 'screening' directed towards well defined risk groups (older mothers, women with family history of hereditary disease), and only recently, prenatal screening was extended (in industrialized countries) to near all the pregnant women.

Medical innovations, such as screening for malignancies and prenatal diagnosis, did not arise because a group of professionals decided to develop a new medical technology, then found allies who supported these activities and successfully 'translated' the interests of other social groups into their own. They came into being as a result of a complex, and at least partly contingent, interplay between developments within and outside the studied scientific domain: scientists' motivations, the pressure of practical problems, technological, social and political change. And they were frequently affected by events quite remote from the trajectory of the investigated innovation. Their history indicates that it may be important to take into account not only the trajectory of a given innovation, but all the events that affected choices of people who developed, promoted and diffused this innovation.

New Role for the "Scientist's Role in Society"

Ben-David views of what science is and what scientists—and, in particular, academic scientists—do reflect faithfully his time and place: Israeli and North American social sciences of the 1960s and early 70s. Some elements of his historical analysis, such as an exclusive focus on Western science, or absence of interest in the exclusion of women and colonized people, are seen as problematic today. On the other hand, Ben-David's approach creatively links analysis of beliefs, interests, motivations and decisions of academic/scientific researchers with broad societal changes. It can, therefore, provide innovative ways to connect the local and the global, and to interrogate values incorporated in instruments, techniques and practices. Such an approach can be applied, among other things, to forward the understanding of the exclusion of women, people of color, lower classes, or non-Western populations from the academe, and from the production of 'legitimate' knowledge. The dismissal of Ben-David's heritage in the name of more progressive ideas may deprive the defenders of these ideas of efficient tools to promote them.

A renewed interest in the scientist's role(s) in society can be enriched by elements brought to the fore by sociologists, anthropologists and historians of science in the last forty years, and insights developed by gender and post-colonial studies. It can pay attention to intersections between science, culture, economy and politics, follow more closely the regulation of scientific and technological activities, investigate dynamics of domination and subordination, and include actors and elements missing from Ben-David's original analyses. In its more inclusive—and thus, hopefully, more robust—version, the scientist's role may become a 'dispositive' (or 'apparatus' in Agamben's translation of Foucault's original term) (Agamben 2009), which articulates self-identities and status, rewards and publications, instruments, techniques and training, economy and moral economy of science, institutional structures and administrative decisions. Its study may favor a renewed interest in individual and collective ethos of scientific research while preserving important lessons about the complexity and heterogeneity of scientific practices and the porosity of boundaries of science.

Interest in the scientist's role in society can also promote more reflexive studies of science. Social scientists of Merton's, Ben-David's or Shils's generation viewed science as an intrinsically progressive enterprise, the promoter of greater well-being of human kind, a bastion of democratic values, and a shield against dangers of totalitarian regimes. Their own research, they believed, was faithful to such an ideal of science, a view which allowed them to be proud of their role as social scientists.[7] Many members of the post-68 generation of sociologists of science rebelled against what they saw as unjust use of scientific knowledge and the hypocrisy of scientists who described themselves as the producers of value free, 'pure' knowledge, while

[7] Some critics of this position argued that social scientists who uncritically praised science as a superior kind of human activity were not very different from male scientists who provided scientific evidence of women's inferiority, or Western scientists who had 'scientifically' proven the superiority of Western culture.

actively contributing to military or colonial enterprises. They attempted to correct such misleading view of science, and some strived to combine the role of social scientist with the one of political activist.[8]

The importance of activist trends in science studies diminished, however, with the growing institutionalization of this domain and the turn towards more descriptive approaches. Researchers who "followed scientists around" explained that they see their task as the production of accurate description that display the complexity of social and cognitive dynamics and usages. Accordingly, they carefully avoided taking sides, judging the protagonists, or attributing responsibility.[9] They also refrained from revealing their own values and standpoints. Scholars who study the sciences in the early twentieth first century frequently dissociate their reflections on science(s) in general, from their understanding of their tasks and values as academic researchers. They do not think that they have a greater obligation to be reflexive about their research practices than their colleagues who study geological formations, Greek manuscripts, or plant viruses. The paucity of reflexivity in science studies may be also seen one of the consequences of the "science wars" of the 1990s.[10] Finally, it can be related to the difficulty of researchers in this area to claim at the same time that scientists are not entitled to special recognition and to believe that their own studies are truly important and should be adequately rewarded.

It is ironic that some science studies scholars—a discipline that in the 1970s criticized the previous view of natural sciences as a "point of view from nowhere"—promote an akin view of their own studies.[11] The development of a historical sociology of sciences may provide an antidote for such an 'a-perspectival' view. We need more studies of science as a profession and a vocation, and more research on political, economic and administrative variables that favor the production, consolidation and dissemination of scientific knowledge within and outside institutions of higher education. We also need, as Lorraine Daston proposed, studies of cultural origins and development of scientific rationality which takes into account moral, emotional and æsthetic elements of the scientific enterprise, and the ways they unfold in each historical context (Daston 1995). And, finally, we need to rethink the unique role, that is, self-representations, duties and responsibilities of historians, sociologists, political scientists and anthropologists who specialize in the study of science and, therefore, acquire a more systematic—although not necessarily superior—understanding of this unique human endeavor.

[8]This is a simplified view of the origins of science studies. Jon Agar had shown that this direction of studies originated in two contradictory trends, one towards more communitarianism and another towards greater individualism. See: Agar (2008).

[9]Pestre (2004), following Boltanski and Chiapello (2004), also argues that trends put to the fore by science studies in the 1980s and 90s, closely follow—and thus indirectly legitimate—new trends valorized by capitalist management: networking practices, agency and autonomy of actors, creativity, mobility and adaptability.

[10]The high points of the latter were the Sokal affair and publication of the books by Gross and Levitt (1994), Ross (1996), Sokal and Bricmont (1997, 1998).

[11]The notion of "situated knowledge" was developed by feminist critique of science; see: Haraway (1988), Harding (1996), Keller and Longino (1996).

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Chapter 5
The Ongoing Tension: Clinical Practice and Clinical Research

George Weisz

Abstract Ben-David had studied the medical sciences on a number of occasions and traced their impact on the development of the sciences and professions in general. The chapter exemplifies a range of aspects which Ben-David had addressed: the interplay of teaching and research; the relative position of basic research versus applied research; the role of the sciences and professions within higher education; and the professional ethos regarding research and service.

Medicine stood at the beginning of modern science with its propensity to specialize. This brought the medical profession into an internal conflict which foreshadowed tensions subsequently: the curative aspect of medicine, the necessities to base practice on evidence, the comprehensiveness of medicine versus various specializations, interests of an evolving pharmaceutical industry, aims of public health, or the foci and incentive structures of health insurance programs. The chapter focuses on some of these tensions as they developed at the interface between practice and research orientation and as they affect medical schools.

Joseph Ben-David wrote frequently about medical researchers. They intrigued him because they played multiple sociological roles and were frequently participants in the hybrid science that he considered a major source of innovation. Medical practice, he believed, could itself be a source of creative inspiration. In an article published in 1960, he wrote: "This analysis of the beginnings of bacteriology and psychoanalysis lends general support to the proposition that contact with practice may be important in reorienting research toward the investigation of new and fruitful problems" (Ben-David 1960a). But he was also acutely aware that this co-existence of roles was difficult and oftentimes problematic. In 1966 Ben-David published a relatively modest article "Socialization and Career Patterns as Determinants of Productivity of Medical Researchers" (Ben-David 1991) based on studies of Israeli medical researchers.[1]

[1] The paper was presented at the Sixth World Congress of Sociology, held in Evian, September 4–1, 1966.

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Here he highlighted the tensions between the functions of clinical practice and clinical research:

> "But even the trained researcher usually works in a hospital, the main job of which is to cure. Consequently, the occupational role of the clinical researcher contains both the elements of practice and of scientific work. These two components of the role are governed by different sets of norms and values and link the doctor to two different systems of professional communication. This results in an inner conflict within the role image of the clinical researcher and a considerable strain in the role pattern. Yet this role pattern seems to be more efficient than the role patterns in other fields of applied science" (Ben-David 1991, 71).

He suggested that the roles were so different that a process of 'resocialization' was required by any M.D. entering the research field:

> "[. . .] an important aspect of the resocialization consists of linking the beginning researcher into a network of scientific communication, thus inducing him into an internal scientific community and exempting him to some extent from the standards and norms of the professional community of local medical practitioners" (Ben-David 1991, 72–73).

In fact Ben-David and his collaborator found the post-doctoral training was most effective when taken in a basic research environment. In his conclusions, Ben-David moved from empirical data based on the Israeli case to broader generalizations:

> "In large scientific centers there may arise more continuous schemes of training, and sizable groups of clinical researchers in each field may be found to form scientific subcommunities of their own. It is nevertheless our feeling that essentially the problem of hybrid nature of clinical research is general. There is in all cases an element of institutionally generated conflict through turning out practicing professionals and then putting them to work in research" (Ben-David 1991, 88).

Ben-David's insight has broad implications. The practice/research dichotomy has been the source of profound tensions at the core of medical education during the past two hundred years and has shaped both the training of physicians and medical practice. In this essay I would like to explore a few of the many facets of this tension, presenting my material as a series of case studies. I will begin more or less at the beginning with formation of the world's first large-scale medical research community in Paris during the early years of the 19th century. Among the questions that historian have explored is the mechanism by which doctors came to see at least some of their patients as research subjects.

Case 1: Creating Patient-Subjects

Unlike historians of German science, historians of science in France rarely speak of "a Great Transition" that created professional science in that country. This is because the process occurred gradually, starting in the eighteenth century and accelerating after the French Revolution, but in the absence of any single institutional innovation comparable with the rise of the German research university. Ben-David (1970), in a classic work of historical sociology, explicitly denied that much in the way of institutional change took place in post-Revolutionary Paris, claiming that the "great upsurge of French science following the Revolution was only indirectly related to the new institutions of higher education established between 1794 and 1800, and those institutions did not constitute a beginning of organized patterns of scientific work. They were rather the culmination of eighteenth century patterns of scientific work". This explains why French science, according to Ben-David and many others, began after 1830 to decline from its position of international scientific leadership (Ben-David 1984/1971). No one bothers to discuss medicine in this context. This is, I suspect, partly because it lacked the epistemological status of the physical sciences usually considered to be at the center of this shift, and also because medical research was 'professionalized' in clinics rather than universities or laboratories and remained subsidized to a considerable degree by private medical practice. It would take us well beyond the scope of this essay to discuss these issues in detail. I will, however, make three points. First, it is certainly the case that developments in nineteenth-century French science built on eighteenth-century attitudes and institutional patterns, not least in their openness to specialized research. Second, medicine was not just part of the milieu of amateur science from which 'real' disciplines such as physics and chemistry emerged; it, too, went through a comparable process of professionalization and discipline formation. Third, however one chooses to evaluate the overall institutional system devoted to science and technology in France, Paris medicine brought into being a new institutional form that was in its way as revolutionary as the German research university, even if it proved to be less enduring.

Early in the nineteenth century, Paris became a center of knowledge production of unprecedented size and scope, based on a network of interconnected institutions and individuals. The Faculty of Medicine, the Sorbonne, the School of Pharmacy, the *Collège de France*, and the *Muséum d'Histoire Naturelle*, as well as the municipal hospital system, shared students, professors, and junior staff; all became part of a common career structure for the elite that I have described elsewhere (Weisz 1995). Around these institutions revolved a flourishing world of medical societies, private medical teaching, and medical periodicals that observed closely and often criticized harshly the elite of official medicine. Only those with formal teaching positions can be considered fully 'professional researchers' in the modern sense (although many of these also had flourishing private practices). And even for these, as well the much larger number of individuals who did not hold posts providing substantial salaries, medical practice not only supplied the data of clinical research but also frequently subsidized it financially. Nonetheless, these limitations in no way diminish either the novelty or the vigor of this new type of research community.

A research community—numbering many hundreds of individuals—was unique to Paris during the first half of the 19th century. The Paris Faculty of Medicine, the largest medical school in the world, had more than two dozen full professors and many junior personnel. The Parisian hospital system employed several hundred doctors and surgeons. To these one must add all the ambitious students and graduates who were seeking to make their mark in the world of academic medicine. In this competitive world, nepotism thrived and some nonentities managed to achieve notable success; but it was nonetheless deemed imperative to produce new knowledge.

Critics argued that many of the structural characteristics of the institutional system were counterproductive and harmful. But no one disputed that the goal was to advance knowledge. And few disputed that what was needed was 'positive' knowledge, based on careful empirical observation of many different clinical cases and postmortem dissections. Some might criticize mere empiricism and argue for the importance of theory in making sense of empirical observation, but no one of any stature suggested that empirical observation was less than central. As a consequence, many hundreds of individuals were, at all levels of the system, seeking to make or consolidate elite careers through various kinds of clinical research and teaching. One of the most important consequences of this shift to new forms of practice/research, I have argued elsewhere (Weisz 2006), was the spread of medical specialties which allowed individual clinicians to see the large numbers of cases that were now a requirement of rigorous clinical research.

This institutional context goes some way toward explaining the spread of medical research within institutions of medical education in Paris as well as the transformation of patients into research subjects. But it does not explain the transformation in doctor-patient relations that accompanied this shift. Foucault (1975/1963) has famously described this change as the birth of the "medical gaze", the process of objectifying the patient so that diagnosis and treatment could take place in new ways. Foucault describes rather than explains this shift and he also situates it rather earlier than is justified by the historical record. Furthermore he neglects a key aspect of the transition: the need to reconcile the physician's commitment to the patient's well-being, the traditional approach to medical ethics (though rarely formalized in the early 19th century) with a parallel concern to advance medical science. The apparent tension between these two ideals would become a formative element in the birth of contemporary bioethics a century and a half later but it troubled nineteenth-century physicians relatively little. Unless a physician did something truly egregious to patients, transmitting microorganisms or cancer cells to healthy patients for instance, there was little public or professional outrage.[2]

A group of British sociologists writing in the 1970s attempted to provide an answer to a related question: how and why did the patient and her symptoms lose their privileged position at the centre of medicine, to be replaced by anatomical lesions or the results of laboratory tests (Waddington 1973; Jewson 1974, 1976)? The answer they provided is based on social class and can be applied as well to the closely related question of how patients became research subjects. According to this explanation, it was the medicalization of large urban hospitals that made a new sort of research possible. Not only did it provide doctors with large numbers of patients and bodies for post-mortem dissection, a prerequisite for the new style of clinical research, but medicalizing institutions housing the poor created a fundamental disequilibrium in the doctor-patient relationship. Unlike the patronage relationships

[2]The most important study of this subject in the American context, Lederer (1995), overestimates in my view the degree of consensus that existed within the medical community about what was permissible and what was not.

between affluent clients and physicians, or even the relative equality between paying patients and doctors, poor hospital patients had few options and little leverage in deciding what was done to them. The poor certainly merited and frequently enjoyed physician commitment to their well-being. But social inequalities made such commitment incomplete and flexible. Since hospital patients were receiving free treatment paid for by society, making their bodies available for teaching and research purposes was seen by many as a fair bargain that allowed these patients to give back some of what they were receiving from society. One consequence was that patients' bodies replaced oral accounts of illness as the basis of clinical observation and judgment. A corollary to this proposition was that where such social inequalities did not exist, clinical research was difficult to introduce. The lack of such institutions in late 18th-century Britain, it was posited, and the widespread survival of patronage relations in which wealthy patients were the dominant agents explains the relative absence of high level research in that country during this period. This argument is not totally convincing on empirical grounds (hospital patients were not necessarily passive, and private patients could be made subservient by serious illness or an authoritative physician). Nor is it clear that there was a significant discrepancy between clinical research in Paris and other great cities of Europe. But this explanation has the merit of at least seeking to answer a critical question; how could the commitment of the physician to his patient leave room for research practices that privileged the future good of society as much if not more than the immediate interests of the patient?

There are of course other possible lines of argument. One that seems particularly convincing to me is the relatively thin line between 'normal', acceptable therapeutic experimentation which was in the patients' own best interest and experimentation whose only goal was to advance science. The experience of most doctors and patients was that many therapies worked some of the time. This was hardly surprising since every patient and indeed every case of a disease was thought to be somewhat unique. Even after 1840 when clinical counting of therapeutic results became common, there was still enormous variation in clinical results. Without a clear concept of statistical efficacy to define acceptable practices (not to mention lack of standardized practices), physicians had enormous margin to maneuver, trying new therapies or adapting old ones to new conditions. As long as one could make a plausible case that an experimental procedure was done for therapeutic purposes, there was little cause for outrage and no clear contradiction between the physician's dual roles as a healer and researcher. As we now know, this situation began to change radically in the mid-1960s.

Case 2: Research as a Source of Professional Conflict

Professional hierarchies are not necessarily based on research roles. In the UK, professional power flowed traditionally from control of key institutions. During the course of the 19th century, membership in the Royal College of Physicians and the Royal College of Surgeons gradually evolved into the domination of voluntary hospitals where medical education took place. Hospital practice and teaching did not necessarily involve research or disciplinary specialization and the British elite resisted such trends until the early 20th century. But elsewhere research and specialization became increasingly associated with institutions of medical education. It began with the German research university in the 1850s and 60s; French institutions attempted to keep up with major reforms of higher education from the late 1870s on. In the USA medical elites struggled during the second half of the century to develop local research. Everywhere, intra-professional conflict between elites and organized practitioners revolved frequently around issues of research expertise.

The American academic medical elite, at the periphery of international medical science, was particularly enthusiastic about research and specialization in medical schools (though there were bitter conflicts about specialized medical practice). Nonetheless the process created significant tensions within American medicine. The AMA sought during the 19th century to be the single, unifying body representing the medical profession as well as the central locus of medical knowledge in America. In 1860, as part of its drive to introduce more scientific discussions at meetings, it created six sections devoted to specialized disciplines. While morning sessions were given over to general business and medical education, afternoons and evenings were taken up by the 'scientific' work of the sections. Nonetheless, the scientific status of the AMA was not high and meetings were dominated by professional and ethical issues (JAMA 1902; Fishbein 1947, 1092).

The scientific role of the AMA was directly called into question by the rapid proliferation of specialty societies. In 1864 the American Ophthalmological Society was formed, joining the Association of Superintendents of Asylums, which had been in existence for close to two decades. Superintendents and ophthalmologists were soon joined by many other specialties in organizing societies, which characteristically restricted membership to individuals perceived as having contributed to medical knowledge in the specialty. Leaders of the AMA bemoaned the proliferation of these societies but nonetheless continued seeking the support of specialists who increasingly replaced general practitioners as presidents of annual meetings. More significantly, the AMA's system of sections gradually evolved into a parallel form of representation for specialists. As specialties grew, sections divided and subdivided. In 1885 there were seven sections, and fifteen years later there were thirteen. Before 1885, an association-wide committee of nomination chose the officers of the sections, but thereafter sections elected their own officers. In this way, the AMA came to provide an alternative form of representation for specialties that, as the association's representatives never tired of pointing out, was not exclusive and restrictive in membership, as specialist societies were, but was instead open to non-academic specialists and general practitioners (Hibbert 1894, 860). Indeed, many of the papers read in these sections during the latter decades of the nineteenth century seem to have been aimed chiefly at educating GPs in specific skills and teaching them when to consult a specialist.

A brief controversy was ignited by the coming together in 1888 of twelve national specialty societies in an annual Congress of American Physicians and Surgeons. This followed on the heels of several unpleasant conflicts between the AMA leadership and the notables of academic medicine, many of whom were specialists. During the early 1880s there was much disagreement about the clause in the AMA's Code of Ethics that prohibited doctors from consulting with homeopaths. Many elite physicians opposed this prohibition on the grounds that scientific expertise rather than professional codes should determine proper practice. A serious dispute broke out when the Medical Society of New York State enacted a code of ethics without a consultation clause; this caused a split in the society, and for the next decades two medical societies coexisted in New York State. Many members of the AMA saw the campaign for freedom of consultation as a self-interested attempt by predominantly urban specialists to increase their fees. In 1885 another dispute erupted over the organization of the International Medical Congress in Philadelphia. The original organizing committee, made up of leading academics and specialists, was dismissed and replaced by a committee more closely identified with the AMA and its support for the Code of Ethics (Warner 1999, 52–69).

In response, those most closely associated with the fight against the Code of Ethics organized in 1886 the Association of American Physicians, an exclusive body representing the scientific elite of medicine (Harvey 1986). Two years later the annual Congress of American Physicians and Surgeons met for the first time. This was a restricted event controlled by extremely restrictive specialty societies that claimed to be devoted to medical knowledge rather than to medical politics or medical ethics. Both the Association and the Congress were perceived as direct attacks on the AMA. One editorial in the Journal of the AMA condemned specialist societies for their "disintegrating influence" that was "antagonistic to any general and harmonious organization of the whole profession"; it went on to attack the new Congress for seeking "to ultimately displace and supersede" the AMA. In contrast to specialist societies, which encouraged "class differences" and their attendant bickering and rivalries, the sections of the AMA were presented as a means of accommodating specialists and researchers while also maintaining unity and homogeneity within the profession (JAMA 1888). But the Congress, which never became more than an annual meeting of the member societies, limited itself to scientific issues. Its members also feared the excessive proliferation of specialties, and its rules stipulated that the admission of any new specialist societies required the unanimous agreement of all member societies. The Congress's existence in fact encouraged efforts to increase the number of specialty sections within the AMA, to improve the way these functioned, to make them more autonomous, and, increasingly, to transform them into the dominant units within the organization (Konold 1962, 37–41).

After 1890, relations between elite specialists and the AMA were not without conflict. The specialty societies and the Congress of American Physicians and Surgeons offered specialist researchers an attractive alternative to the sections of the AMA as well as the opportunity to express hostility to the AMA's attempts to prohibit contacts with irregular practitioners. But by 1896 an editorial in the JAMA proclaimed: "The American Medical Association has become what the Congress of American Physicians and Surgeons sought to be, a veritable confederation of medical bodies devoted to independent lines of thought and practice [...]" (JAMA 1896; Taylor 1896). The specialty sections of the AMA continued into the twentieth century to be characterized as the true associations of specialists precisely because they were inclusive. The AMA's claim to represent specialists and researchers would continue to have serious consequences as the twentieth century advanced. The radical reform of American medical education in early 20th century was certainly the most visible and successful result of the ability of these two groups to work together and make compromises.

A number of theoretical points emerge from this case study. The first is one that Ben-David made several times in his writings: researchers, even clinical researchers, see the world differently than ordinary medical practitioners and their concerns and interests frequently conflict. The second is that research expertise can be a form of professional power but this power is not absolute because medical practitioners can and do organize themselves to counterbalance the influence of research elites. The third point is that what some view as a matter of scientific expertise may be perceived by others as a matter of ethics or virtue, as the controversy about homeopathic consultations demonstrates. Both medical expertise and medical virtue can be tacit and informal or can be formalized in codes. Codes of ethics formalized medical virtue in the 19th century in much the same way that clinical practice guidelines now formalize recognized scientific competence. In the next two sections we will examine attempts to standardize competence and efforts to codify virtue.

Case 3: Clinical Research and the Standardization of Competence

One of the ongoing tensions between medical research and medical practice has to do with the movement of the results of the former to the latter. There is now a distinct area of research, Translational Research, devoted to bringing the results of the life sciences to medical practice. More recently there have been calls for (and efforts to implement) "practice-based research" that is more closely attuned to real clinical needs (Westfall et al. 2007). The issue is particularly urgent when it comes to clinical research, where application should be easier; frequently however, this is not the case and the consequence is practice variation from one locale and one practitioner to the next. This was not always a cause for concern. Until the second half of the 20th century, a certain degree of variation in medical practice was considered acceptable or at least unproblematic. But since then a regulatory upheaval (if not revolution) has taken place.

Until the 1970s, medical actions were indirectly regulated through the training and credentials guaranteed by both the organized profession and state authorities. Armed with these credentials, individual physicians were assumed to be competent enough to determine the appropriate medical procedures. As innovations appeared they were debated in the medical literature and individual practitioners adopted them or not. Although the opinions of experts expressed informally or formally provided some guidance for such decisions, they were frequently diffused narrowly and had no formal status. In contrast, the regulation of quality now explicitly targets medical practice itself by attempting to modify physician behavior. Like the regulation of credentials that it supplements rather than replaces, numerous groups and institutions are involved in this process. Although there is a long history associated with this shift, the transformational developments took place at a specific time (the 1960s and 1970s) and place (in the United States). The outcome of this process has been an international plethora of clinical practice guidelines and a growing literature on how doctors can be convinced or made to follow them. Although guidelines have become associated with the evidence-based medicine movement, they in fact preceded it.

This emergence of a culture of guidelines is not just a consequence of the growing pressure of third-party players to control costs, as suggested by some (Fowkes and Roberts 1984), or defensive measures by medical professions to preserve autonomy, as has been argued by others (Castel 2002; Timmermans and Kolker 2004). Far more complex and long term developments have been at play in the transformation of practice regulation. These emerged well before the current era because outside the traditional private relationship between doctor and patient, large-scale institutional settings for biomedical practice existed beyond the authority of physicians. There, the standardization of classification categories, measures, and procedures was perceived as a requirement for a variety of purposes, including the evaluation of outcomes, large-scale organizational activity, and, later, third-party payment. In the nineteenth century, public health was one such domain. Then, in the early twentieth century, public health standards entered the world of clinical medicine as preventive public health expanded to the sphere of therapeutics through such mechanisms as sexually transmitted disease, tuberculosis, and cancer-control programs. Hospitals, which grew at a prodigious rate at the end of the nineteenth century, also generated demands for standardized organizational structures, practices, and data collection.

After World War II, all aspects of the medical enterprise expanded dramatically, especially in the United States, which was by then the world's richest nation and the most profligate spender on health care. Everywhere but in the United States, national health insurance systems were established or significantly extended. Hospitals were expanded and modernized. The research sector grew significantly as well, especially in the United States, where government funding rose to unprecedented heights (Fox 1996). This expansion of both public domains of medical practice and biomedical research, each with their attendant multiplication of standards and protocols, made the standardization of medical procedures appear both feasible and imperative.

The expansion of biomedical research had a variety of consequences. First, it vastly augmented the already large number of technological and pharmaceutical innovations with which doctors and growing numbers of administrators had to cope, and it amplified the pressure for collective forms of evaluation. Second, many large domains of research became sufficiently collaborative to generate standards and protocols. In particular, the spread of multi-center research required standardized categories and practices that allowed for the aggregation of data. This happened first in several biomedical domains whose interactions among many researchers, complex technologies, clinics, and laboratories necessitated some form of negotiated conventions (Cambrosio et al. 2006). Cancer treatment was a notable case in which research and clinical practice were closely associated. Here, chemotherapy was increasingly the result of research protocols that had become routine practices.

Funding agencies demanding comparability of results forced researchers in other specialties to follow this path as well. Indeed, one of the motivations for the development of the Diagnostic and Statistical Manual of Mental Disorders (DSM) III was to establish psychiatric disease categories stable enough to be the subjects of rigorous and fundable research (Healy 1997). Third, randomized clinical trials (RCTs) gradually became a 'gold standard' for evaluating therapies (Marks 1997). Despite the controversies continuing to surround them, RCTs were widely believed to tell us, in many cases, what best practice was. The logical conclusion for many has been that practices differing from those validated by RCTs are mistaken deviations from correct clinical procedure, and that the perceived solution is to diffuse knowledge of correct practices in various ways, including practice guidelines.

The expansion of biomedical research had yet another consequence: it brought into the open some of the ethical dilemmas long associated with research on humans, and it also created new ones as technology expanded the frontiers of the possible. A substantial minority of the 'guidelines' published during the 1960s and 1970s dealt with issues ordinarily categorized as 'bioethical' concerns, including the standardization of informed consent requirements. The products of medical research created a variety of ethically complex conditions and practices in clinical medicine—such as brain death, life-sustaining technologies, and in vitro fertilization—which seemed to require ethical guidance to supplement their very complex technical guidelines (Rothman 1991). These guidelines then became integral parts of the protocols defining these activities. Today, the very work of ethics is itself becoming subject to special guidelines and evidence-based research.

If the consensus necessary to produce guidelines was made possible by new techniques like randomized clinical trials or the Delphi method developed at the Rand Corporation to generate forecasts from experts, it was made necessary by perhaps the most significant development of these postwar years: the increasing role of governments in every aspect of health care. Through the national health insurance systems that were established during the postwar years in much of the Western world, including the partial system developed in the United States in the mid-1960s, medical practice became integrated into the public political arena and transformed into an object of intense media scrutiny. Public accountability became a critical issue (Wiener 2000). In this context, guidelines produced by experts are attractive for many reasons. All health insurance systems must decide what counts as diseases and medical procedures, which of these are to be paid for, and who should be paid for doing them. This is not an exercise in finding truth. Rather, it requires negotiations in which many actors make claims in the name of various values and rights. What is important in the end is that an act can be placed into one category or another to be dispatched accordingly. Such pigeonholing is frequently performed by administrative fiat or negotiation. But this leaves politicians vulnerable to public criticism. The invocation of expert guidelines to support these decisions helps depoliticize such issues (Jasanoff 1990; Nelkin 1995, 444–456).

An equally important rationale was the perceived need in nearly all Western nations to impose rational direction and coordination on an array of institutions—hospitals, dispensaries, medical schools, local medical assistance or insurance programs—that had been created incrementally and almost haphazardly over long periods of time and that were increasing in both size and technological-functional complexity. This need was in part, but only in part, linked to budgetary considerations. It also was associated in many European countries with the belief by administrators and politicians that medical professions were too powerful and needed to be brought down a peg or two (Hassenteufel 1997). Less often emphasized but no less critical was the fact that these different kinds of medical institutions were now lumped together in large administrative structures, becoming parts of 'systems' that did not seem to function in any obviously comprehensible way. They appeared to require reorganization based on 'rational' principles and some guarantee of 'quality' (Robelet 2002). Not only did all sorts of practices require standardization, but administrators also required information about health 'systems' in order to exercise organizational control. This information then had to be available in standard quantified form, allowing for understanding and evaluating activities along various axes. In turn, this created health organization research, which received a fraction of the funding for biomedical research (Gray et al. 2003) and deployed quite different forms of disciplinary expertise, including economics and organizational science (Benamouzig 2005). But health services research engendered further efforts to ensure comparability through the standardization of categories, methods of data collection, and practices under study that do not differ in kind from similar efforts in the biomedical research sector.

Clinical guidelines are hardly unique; health care now is inundated with guidelines of every sort. Long-term health planning; the establishment of new institutions or services; specialist training; the evaluation of medications, procedures, and technologies; laboratory testing; utilization management; and ethics review are only some of the activities that have produced an extensive guideline literature. And the list keeps getting longer. What all these activities have in common with cancer treatment protocols or multi-site clinical trials is their technical complexity and need to coordinate large numbers of people and things. In addition to the growing scope and complexity of institutions and activities to be regulated, expectations have changed as well. As health care has become a public good financed by public monies, domains once characterized by individual judgment and idiosyncrasy have become increasingly subject to demands for transparency and regulation (and, it goes without saying, cost control). And every effort to regulate increasingly unwieldy health care systems seems to produce complex mechanisms that require even more guidelines or conventions in order to function.

Case 4: Standardizing Virtue in the Medical School

The tensions between research and practice have long been internalized in the functioning of institutions of medical education. Such tensions have intensified in the decades since World War II, characterized by massive investments in bio-medical research. Professional rewards of all sorts in medical schools now depend on research productivity and success in obtaining research funds. Revenue from clinical practice in academic centers is frequently used to supplement biomedical research funding. Sometimes the relationship has been synergistic with targeted research producing clinical innovations that became important money-makers for the faculty and are in turn channeled into further research (Bowman et al. 2007). But in many cases, practice simply finances research. Medical education itself is overshadowed in major medical centers by research priorities (which are also far more attractive to private donors). In the words of Ken Ludmerer (2000), a physician-historian: "By the end of the 1990s, education was by far the most endangered part of the medical school's traditional mission. Amid the pressures of research, graduate medical education, and the provision of increased patient care, the education of medical students has become merely a passing concern." That being said, medical school faculty have usually cared deeply about sending good doctors out into the world and reforms of curriculum and teaching methods have been a regular occurrence. These processes attract only intermittent if any interest from leading academic researchers who leave this task to clinician-educators who receive substantially inferior rewards and status. This has prompted calls for revamping promotion and tenure systems to take account of the different career trajectories of physician-educators (Fleming et al. 2005).

The tension between doing research (and training young researchers) and producing good medical practitioners is only one aspect of an ongoing problem. Another facet has to do with what should be taught. In the early 20th century, it was knowledge of medical science that seemed essential. Of course like all generalizations, this one is too broad and it is easy to find counterexamples; the British medical elite until well into the 20th century valued gentlemanly virtues and practical experience as much if not more than scientific knowledge, as my colleague Chris Lawrence has demonstrated (Lawrance 1985). And just as the narrowness of specialization and scientific reductionism in medicine has engendered diverse efforts to introduce "holism" or "synthesis" or the "biopsychosocial approach", there have been regular reactions in medical schools

> "[...] triggered by still another *prise de conscience* about a too-great emphasis on the biological and technical aspects of medicine at the expense of what have variously and alternatively been called its psychological, social, cultural, interpersonal, behavioral, environmental, ethical, moral, and or humanistic components [...] The most recurrent pattern of all has been to inject designated new courses into the curriculum, as if they were intellectual magic bullets that could remedy the perceived [...] imbalance in medical training. Over the course of the last three decades [...] North American medical schools have moved in seriatim from psychiatry, to psychosomatic medicine, to social and behavioral science, to community medicine, to bioethics, to the humanities in their search for such formulaic solutions" (Fox 1990, 125–157).

Perhaps the most bizarre of these efforts is the one currently agitating medical educators, the "professionalism movement". At one level it can be seen as one of three current movements (the others are the evidenced-based medicine/guidelines movement and the patient protection movement) that seek to improve the quality of medical care. And certainly it is a response to many of the same social, economic and cultural pressures discussed above in Case 3 that have produced these latter two movements. But it is unique in a variety of ways. Unlike evidence-based medicine that originated in departments of clinical epidemiology and presented itself as a paradigm shift in medical knowledge (or at least that part of it dealing with evaluation of efficacy), professionalism has won broad support among medical educators. Without questioning the cognitive basis of medical education or clinical research, it seeks a renewal in basic medical values or more accurately a return to traditional values; more controversially, it aims to produce not just proper behavior among physicians but virtue.

While professionalism has a much narrower base than the evidence-based medicine, it has nonetheless spawned an impressive literature. As of mid-April 2009, the Web of Science listed 1,084 articles on the subject, the vast majority published after 1999. (There are also a notable number of former deans of medical schools among the authors of these papers. The individual with the largest number of publications on this subject, according to Web of Science, is a former dean of McGill's medical faculty.) Professionalism is a difficult subject to write about, because, while there is a general consensus that medical students and residents need to be educated into internalizing certain kinds of behaviors, values and virtues, there is little agreement about what these actually are or how to go about inculcating them. Probably the most influential statement of the goals of professionalism was the Charter on Medical Professionalism published in 2002 simultaneously in the *Annals of Internal Medicine* and *The Lancet* (American Board of Internal Medicine (ABIM) 2002). It was produced by three internal medicine institutions, two American and one European, which had joined together in 1999 to launch the Medical Professionalism Project.

The charter begins: "Changes in the health care delivery systems in virtually all industrialized countries threaten the very nature and values of medical professionalism [. . .] We share the view that medicine's commitment to the patient is being challenged by external forces of change within our societies." The response to this threat must be a "renewed sense of professionalism, one that is activist in reforming health care systems". The document thus assumes that medicine has always been governed by a collective commitment to the patient and a sense of professionalism. It then moves on to a brief Preamble:

> "Professionalism is the basis of medicine's contract with society. It demands placing the interests of patients above those of the physician, setting and maintaining standards of competence and integrity, and providing expert advice to society on matters of health. The principles and responsibilities of medical professionalism must be clearly understood by both the profession and society. Essential to this contract is public trust in physicians, which depends on the integrity of both individual physicians and the whole profession."

One finds here the assumption that medicine's (by which is meant physician's) relationship with society is governed by a social contract; this means practically that doctors have both rights and obligations and those rights depend on the fulfillment of obligations so that public trust in physicians can be maintained. Aside from making the welfare of patients primary and visible, the key to this trust is 'integrity', a word that appears twice is this short declaration. The document then goes on to state the three fundamental principles of renewed professionalism:

- *Principle of primacy of patient welfare:* This principle is based on a dedication to serving the interest of the patient. Altruism contributes to the trust that is central to the physician-patient relationship. Market forces, societal pressures, and administrative exigencies must not compromise this principle.

Altruism or dedication to the other is central here. What it means practically is that the physicians' understanding of patients' welfare should trump "market forces, societal pressures, and administrative exigencies".

- *Principle of patient autonomy.*
- *Principle of social justice:* The medical profession must promote justice in the health care system, including the fair distribution of health care resources. Physicians should work actively to eliminate discrimination in health care, whether based on race, gender, socioeconomic status, ethnicity, religion, or any other social category.

The last is perhaps the most 'modern' aspect of this charter extending medical virtue from the individual doctor-patient relationship into the domain of collective social justice. It is the principle that progressive members of the movement are proudest of.

The charter then gives a list of personal responsibilities to be manifested by the physician: commitment to professional competence; commitment to honesty with patients; commitment to a just distribution of finite resources; commitment to scientific knowledge; commitment to professional responsibilities; commitment to maintaining trust by managing conflicts of interest. The last is particularly salient because "medical professionals and their organizations have many opportunities to compromise their professional responsibilities by pursuing private gain or personal advantage. Such compromises are especially threatening in the pursuit of personal or organizational interactions with for-profit industries, including medical equipment manufacturers, insurance companies, and pharmaceutical firms". The charter concludes:

> "To maintain the fidelity of medicine's social contract during this turbulent time, we believe that physicians must reaffirm their active dedication to the principles of professionalism, which entails not only their personal commitment to the welfare of their patients but also collective efforts to improve the health care system for the welfare of society. This Charter on Medical Professionalism is intended to encourage such dedication and to promote an action agenda for the profession of medicine that is universal in scope and purpose."

It is easy to dismiss this document as yet another example of the medical profession's centuries-long struggles to expand or defend its privileges and powers by invoking commitments to competence and the societal good. Certainly, the large literature that has emerged while usually sympathetic to the wider aims of the movement has been clear-eyed about the ambiguity, abstraction and occasional contradictory quality of the values being defended. One particularly lucid but sympathetic participant in these debates refers to this rhetoric as the 'nostalgic' view of professionalism (Hafferty and Levinson 2008). But I want to emphasize a number of other points. First, all this rhetoric has had a real effect on medical education. Most major medical schools have introduced courses or programs that try to implement the tenets of professionalism, however they are understood. At my own university, McGill, a course component called “Physicianship” tries to teach many aspects of professionalism as described in the charter throughout the entire four years of the medical undergraduate program. (It has also incorporated existing courses, like my own course Medicine and Society, as well as Bioethics taught by department colleagues, which have no relationship to the overall theme but provide an illusion of continuity and coherence.) The second point is that unlike the rest of the curriculum focusing on knowledge and technical skills, these new orientations attempt to foster values in a way that goes well beyond introducing the Hippocratic Oath for graduates as occurred during the interwar years or White Coat ceremonies as occurred more recently. And this brings me to my third and in some ways most interesting point: despite the abstractness of the values being promoted, efforts are being made to train teachers in the field of medical education in order to make it a somewhat less amateur undertaking (Steinert et al. 2006). Similarly, the evaluation of outcomes in teaching has gained ground:

> “Many tools, incorporating quantitative and/or qualitative approaches are now available to assess professionalism; its foundational components of communication and ethics and its central principles of excellence, humanism accountability and altruism. These include standardized clinical encounters, high-fidelity simulations, portfolios, reflection, observations over short-defined periods, critical incidents and longitudinal observations, multisource assessments including peer assessments, written examinations, and measures of conscientious behavior. The selection of a tool should depend upon the purpose of the assessment and the measure’s reliability, validity and practicality ” (Sullivan and Arnold 2008; Stern 2006).

It should be evident that I, like many others, have serious reservations about ‘professionalism’ which has always struck me as, at the very least, deeply and astonishingly naïve.[3] (Although sociologists of the medical profession like Elliot Freidson are frequently cited, the critical bite in their work is usually completely missing in the professionalism literature.) But one thing I cannot deny is the good will and idealism of many of those seeking to reform medical teaching. There is genuine will to improve the quality of medical practice. To put it in Ben-Davidian terms, a new societal role may be emerging, that of the (semi)-professional medical educator. And in terms of the subject of this essay, the tension between clinical practice and research, the movement has already had some impact. Certainly it has not radically altered the balance of power within medical schools. As my own, like many others, becomes increasingly subject to corporate models of organization, biomedical research has become even more dominant because of its capacity to attract status and revenue. But there is now a critical mass of clinician-teachers in many faculties who have been made visible and validated by the professionalism movement; these are now capable of serving as role models for medical students. And to the extent that they succeed in creating a new sort of evaluative research of teaching outcomes, there may be emerging yet another potential bridge (as well as a possible source of tension) between clinical practice and research.

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[3] In the interests of full disclosure, I confess that I argued energetically against the new course on the ground that it would be perceived as indoctrination by students. I also doubted that ‘altruism’ could be taught.

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Part III
Center and Periphery

Chapter 6
Faded Grandeur: Disciplinary Differentiation, Interdisciplinarity and Renewal in the German Academic System

Richard Münch

Abstract The chapter traces the productivity of German, or European, science *vis-à-vis* US science: the early dominance of European science had been inverted. This has given rise to an ineffective European focus on performance-measurements, rankings, performance contracts or financial incentive schemes.

The analysis of the German university is linked to the humanities. Because of their standing, and because of their ties to an educated elite, a homology between humanities and the social stratum carrying it is postulated. After WWII, this homology was pushed to the fringes while a diversified disciplinary spectrum failed to develop. Various disciplines are forced to follow a doctrine "large is beautiful". However, top-down reform efforts, initiated at European or national levels, remain ineffective; and higher education remains deficient as long as structural deficits are perpetuated.

After a long period of crisis, the German educational system is still shaken. Reform protagonists are convinced that the worldwide admiration of the German system, dating back to the 19th century, has long been lost; reform opponents are afraid that even the last remnants of the former grandeur will be sacrificed to the worship of PISA and Bologna.[1] Both extremes hide a grain of truth. More than 100 years have passed since American students and scholars started to visit Germany, to be inspired by science and a research environment found there. In the 20th century, the US, having emulated the German university to some extent, has risen to a scientific hegemonic power; it attracts the most ambitious academic offspring from all over the world and draws creative strength from this permanent supply of young talent. Hence, for some time now, reformers have viewed the US research university as a model.

[1]The Programme for International Student Assessment (PISA) and the Bologna Process, designed to create a European space of academic education.

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What explains the success of the US university? We may presume that the conditions there are not better than elsewhere and the scientific hegemony of the United States is merely due to the size of the American publication market; or to the dominant role of the English language; or to the recruitment and steady influx of young talent and top researchers from all over the world, facilitated by the sheer affluence of Harvard, Yale & Co. These factors may indeed partially explain why Germany and other European countries in recent decades have fallen behind. Nevertheless, there are major structural features which distinguish the American academic system from that of Germany, but also from those found in Britain and France, and these structural features appear pertinent in an attempt to explain the performance of scientists in the US as compared to those within Europe. However, such factors appear not properly understood by the various actors or policy makers who try to improve the standing of the current German academic system. The most prominent strategy employed is the Excellence Initiative, launched recently by Federal and State Governments and aimed to foster research at German universities. This chapter tries to point out that the Excellence Initiative does the opposite of what it is intended to do: it aggravates the structural weaknesses of the German academic system.

In his pathbreaking work on the "Scientist's Role in Society", Joseph Ben-David (1984/1971) pointed out the major structural achievements of the American academic system in the 20th century, along with the failure of the German system to adjust its originally favorable structures to the challenges of the 20th century.[2] As we shall see, his insights have lost nothing of their relevance (Herbst et al. 2002; Herbst 2004). We will first look at the international standing of the German academic system, particularly in comparison to the United States, but also in comparison to the United Kingdom and to France. Secondly, we will sketch an outline of the origin, development and basic structural features of the German system as compared to that found in the United States. Thirdly, we will explain the achievements and failures of the German system, using the example of the humanities (i.e. *Geisteswissenschaften*). Throughout, we shall attempt to show how the Excellence Initiative fails in reforming the system, but in fact maintains its defaults and weakens its traditional strengths.

The International Standing of the German Academic System

During the first part of the 20th century, i.e. the period from 1901 to 1948, of the Nobel prizes[3] in physics, chemistry and medicine, 100 were awarded to researchers working at institutions in France, Germany or the United Kingdom, and 43 to researchers at institutions in the United States. During the period of 1949 to 2008, with economics having been included since 1969, the corresponding ratio was 86 for the three European countries combined and 264 for the United States. In the

[2] See also Ben-David (1977a, 93–126).

[3] Counted by to the number of researchers receiving the prize.

earlier period, scientists of the three European countries won 2.3 times more prizes than their colleagues in the US, while in the later period 3.1 times more prizes were awarded to researchers working in the US. The ratio rose continuously in favor of the US.[4]

It is remarkable to see that the most recent growth of the United States' share in Nobel prizes was accompanied by a declining share in the world total publications output in science and engineering. While the share of the US publications dropped from 34.2 to 28.9 percent between 1995 and 2005, the corresponding drop for the European Union was only from 34.7 to 33.1 percent (National Science Board 2008, 5–28, Table 5–12). The European Union's reduced decline was influenced by the accession of ten new member states in 2004, though their contribution was small. However, while the relative share of US publications—and their reception—is declining in an expanding pool of publications, US publications are still well received: in 1995, the US share in citations was about 50 percent, i.e. about 15 percent above the publications' output share; in 2005, it was around 41 percent, i.e. about 12 percent above the publications' share. The corresponding figures for the European Union were about 31 and 32 percent, respectively, i.e. about 3 and 1 percent below the publications' share (National Science Board 2008, 5–49, Table 5–33).

Interestingly, the share of publications originating within the UK shrunk more than the corresponding share of German publications, namely from 8.1 to 6.4 percent and 6.7 to 6.2 percent, respectively. When it comes to Nobel prizes, scientists within the UK were much more successful in the two decades after World War II than afterwards.[5] This does not, however, reflect in any way the newer British focus on the performance-based allocation of research funds among universities, introduced through the Research Assessment Exercise (RAE) in 1986. This and other evidence (Herbst 2007, Appendix D) speaks against the expectation of a significant enhancement in research performance due to the German Excellence Initiative, molded to some extent on the British RAE.

The coordinated attempts of Federal and State Governments to enhance research at German universities, as advanced by the Excellence Initiative, do not focus on an elimination of structural disadvantages but contribute, in contrast, to their continuation. Under the pressure of various international university rankings of dubious quality, as those published by the Shanghai Jiao Teng University (Billaut et al. 2010) and the Times Higher Education Supplement, the aim is to push up in the ladder of rankings a small number of select 'elite' universities, simply by concentrating more research funds on them (Trow 1994).[6] This strategy ignores the evidence that the

[4]Between 1949 and 1963, the ratio was 42 to 22; between 1964 and 1978, it was 57 to 27; between 1979 and 1993, the ratio stood at 70 to 16; and between 1994 and 2008, it stood at 100 to 21 (The Nobel Foundation 2010).

[5]While scientists of the UK won 33 Nobel prizes between 1949 and 1978, as compared to 11 Nobel prizes awarded to German researchers during the same period, the number dropped in the period between 1979 and 2008 within the UK to 13 Nobelists, while German scientists garnered 15 awards (The Nobel Foundation 2010).

[6]It is misleading to take the top ranking-positions of British universities, such as Cambridge, Oxford or London, as a confirmation for the effectiveness of the funding system.

success of American science is not primarily due to richness or a more profuse funding of higher education institutions; rather, it is due to a diversified, permeable system of institutions, few of which rate as research universities (Clark 1997; Trow 1997)[7]; to the role and the capacity of such research institutions in order to compete for outstanding scientists and teachers; to the role and the capacity of researchers to compete for research funds; to institutional structures favoring a collegial culture as well as disciplinary differentiation and renewal; and, finally, to the upholding of a Humboldtian ideal, the unity of teaching and research.

The overall performance of British science—and even more so of French science—does in no way support a top-down strategy characteristic of the German Excellence Initiative. The broadly spread distribution of Nobel prizes among institutions in the United States, the 'product' of quite a different funding logic, supports the notion that the Excellence Initiative is the wrong way to improve the German academic system. What can be learned from the American model, furthermore, is the importance of keeping research and teaching integrated within the university.[8] The Humboldtian idea—or the German practice—of integrating teaching with research was one of the factors which made the German university the leading model in the 19th century; and it was primarily this practice which was emulated by the US research university in the later parts of the 19th century and which served, at the beginning of the 20th century, as the catalyst for the subsequent ascent of American universities. Germany, on the other hand, was unable to renew its university system along the lines of the so-called Humboldtian ideals and tended, with the formation of the *Kaiser-Wilhelm-Gesellschaft* in 1911 and—after World War II—the subsequent *Max-Planck-Gesellschaft* and other organizations, to separate research activities from teaching. While the Excellence Initiative aims at a closer unity of teaching and research within the confines of the university, isolated research centers are being supported rather than a general integration of research and teaching at German research universities.

Historical research shows that the idea of the unity of teaching and research, commonly associated with the name of Wilhelm von Humboldt and his organization plan for the University of Berlin, founded in 1810 (von Humboldt 1964a), is a construction of the late 19th and early 20th century and has become a national myth (Bruch 1997; Ash 1997, 2008; Langewiesche 2010). Humboldt's own texts on higher education policy of 1809–10 were published not before the 1890s. The practice of uniting research and teaching began way before the foundation of the University of Berlin in the 1780s with Göttingen acting as a forerunner, and was widespread in the German universities of the 19th century without Berlin serving as a model. All these historical corrections do not, however, change the fact that the integration of teaching and research was an invention that flourished in German universities of the 19th century and made them a model for the entire academic world,

[7] That is, less than 200 out of a total of more than 4,000 higher education institutions.

[8] In the United States, 81 percent of the Nobel prizes were awarded to scientists doing research at universities; in Britain, the corresponding percentage was 72 %; in Germany, it was only 61 %, and in France the figure was only 36 % (The Nobel Foundation 2010).

particularly in the US. The failure of the German university to be guided by its own ideal-type is, however, attributable to the German university itself: the structures emerging from this model, the implicit rules and regulations which define and guide its culture, do not favor disciplinary differentiation, inter-disciplinarity and renewal to the same degree as the institutional setup of the US academic system.

The German University as a Socially and Cognitively Closed Institution

The founders of the 'newer' German universities included philosophers, historians, literati and linguists who made their idea of "education through science" *Bildung durch Wissenschaft*)—doing research in the process of learning, in solitude and freedom, and teaching in the process of research—the sacred core of the university (Schelsky 1963, 79–91). This core should be protected by the corporation of professors and the academic self-administration of autonomous universities against all societal expectations. The necessary material prerequisites for the autonomy of the university, funding and legal protection, were to be ensured by the state. In return, the universities were expected to educate the future public servants alongside their own academic offspring, forming the foundation of a 'culture state'. It is only on the social basis of an alliance between the educated middle-class élite and the state of an 'enlightened' absolutism that something could mature in such purity as the *Geisteswissenschaften* in Germany, quite apart from the 'arts and humanities' in the Anglo-Saxon countries and the *letters* in France.

The realm of a self-reproducing educated middle-class, maintained by the state via education at universities, existed only in Germany in such a closed form. In England, in particular, there was a far more lively exchange between the educated middle-class and the commercial middle-class. Until the most recent past, this situation had been mirrored by the fact that in England studies of history, language or literature naturally prepared for executive positions in business: it was seen as necessary to gain an understanding of such disciplines and foster cultural traditions whose acquisition was to characterize the habitus of the gentleman. History, arts and humanities were, and are, no canonized and methodically disciplined *Geisteswissenschaften*, but disciplines which find it easy to open themselves to stimuli from other subjects and to serve practical expectations of training for executive positions in state administration or business. German *Geisteswissenschaften* differ fundamentally from this picture; they supported the foundation of a social closure of the educated middle-classes through a hermeneutical methods canon, a cognitive closure. To defend this position, they relied on the autonomy of the university and the academic self-administration of the professorate, both guaranteed by the state.

The growing demand for a vocational, useful university education which accompanied the industrialization process in the last third of the 19th century, could be fought off more effectively than in other countries—or was transferred to newly established technical schools and colleges. The *Gymnasium* was protected against practical claims by the new *Realgymnasium*, and the universities were shielded through the formation of new technical schools. Nevertheless, the advancement of the natural sciences within universities could not be prevented: these flourished in the laboratories of outstanding researchers and teachers, attracting young scientists from all over the world. The organization of teaching by academic chairs, and the research in associated institutes presided over by the respective chair holders, made the maintenance of an inner disciplinary core possible. In this framework, only established professors were permitted as chair holders. The various chairs were linked to disciplinary core areas, and because chair holders had to master their entire core area, both in teaching and research, they needed staff to meet these tasks. However, new research areas usually emerge at the margins of disciplines, and these 'marginal' research areas were relegated or left, as a rule, to associate staff or 'private' lecturers with fewer corporative rights.

This structural constellation made it possible to maintain the inner disciplinary core in its purity for a long time, and to protect it from irritation caused by neighboring disciplines or practical demands. This effect did not only manifest itself in the humanities, but also in the natural sciences where the chair principle—in association with the corporate self-administration of the professoriate—impeded the development of new fields within the basic sciences, at least within the same institution, as well as that of applied research. The 'closed' university transferred the development of new fields increasingly to the institutes of the Kaiser Wilhelm Society and its successor institution, the institutes of the Max Planck Society. Applied research was transferred to industry as well as to the institutes of the Fraunhofer Society, the Helmholtz Society and to the Leibniz Society (Ben-David 1984/1971, 108–138). Therefore, right from its foundation years at the end of the 18th and the beginning of the 19th century, the modern German university practiced an unusually high degree of social and cognitive closure, kept afloat by its autonomy and corporate self-administration. The need to open the university in the course of the 20th century in social terms to ever greater numbers of students, particularly in the 1970s (Trow 1970), produced a tension between the maintenance of its 'sacred' disciplinary core and its adaptation to new societal demands beyond the long dominating civil service, a tension which is still visible today. The struggle for an appropriate implementation of the Bologna process is a current symptom of this historically deeply rooted tension.

Disciplinary Stability at the Expense of Differentiation, Interdisciplinarity and Renewal

Alongside the massively increased vocational requirements for education to prepare for jobs, ever stronger disciplinary struggles have emerged in the bordering areas between disciplines. Under the far more open conditions in the Anglo-Saxon context, new research areas have sprung up at the intersection of established fields and professional fields were included in university curricula. Germany is falling behind:

the oligarchy of chairs, coupled with the corporate self-administration of the professoriate, protected the core areas against interdisciplinary intrusions and kept them 'pure' for a longer time. At the same time, these core areas and their implicit funding schemes acted as strongholds against a comparatively weak propensity to expand or to renew. In certain fields, renewal did receive some stimuli within Germany, but renewal could never be firmly institutionalized due to inherent—'cultural'—constraints of the German university (Ben-David and Zloczower 1962).

What has been widely extended and disseminated within the US only exists in a very precarious situation in Germany, at the margins of the disciplines, with little chances to blossom-out properly. Some examples of such newly created disciplines are psycho-linguistics, neuro-linguistics, behavioral economics, neuro-economics, historical sociology, and administrative science. Examples covering wide and firmly established research fields, none of which have been widely institutionalized in Germany, are gender studies, studies in law, studies in religion, studies on ethnicity, educational research, migration studies, cultural studies, development studies, and European studies.

Disciplinary Differentiation, Interdisciplinarity and Renewal in the US

If we compare the situation in Germany with the American university, we will discover five essential structural differences that gave the latter far more leeway to a steady renewal and to expand in research and teaching. These are: (i) the integration of research and teaching on the graduate level of PhD-students with a regular curriculum above the undergraduate level (i.e. the graduate school); (ii) a collegiate culture (with sensible faculty-student and faculty-staff ratios) and the forgoing of strong hierarchies; (iii) the organization of teaching in larger departments, with autonomous faculty members, and without chairholders ruling over permanent academic staff or assistants; (iv) the organization of research in flexible research teams and interdisciplinary—interdepartmental—centers; and (v) the guaranteed freedom of research and teaching of faculty members—but not the corporate right of self-administration and university management (Ben-David 1984/1971, 139–168; Parsons and Platt 1973). This means that professors are not engaged in permanent 'territorial' struggles, but can focus on their duties, their research and teaching activities; while the executive tasks remain in the hands of a professionalized university management.

These structural conditions made it possible for new disciplines and new research areas to emerge and to gain equal status with established core areas. Likewise, it was possible to develop a wealth of undergraduate curricula at the intersection of established disciplines, based on disciplinary differentiation and interdisciplinary research, attracting a variety of interests. Hence, undergraduate students in the social sciences do not have only a choice between sociology, psychology, political science or economics, but can focus also on communication or media studies, or on international relations. In the US, graduate studies of various kinds and orientation prepare students for non-academic employment as well as for academic careers: academic or professional post-graduate studies, and studies at the PhD-level. All this is done within the context of a graduate school, and within a departmental program, where often dozens of faculty can address a broad spectrum of subjects or research areas, but where no single professor voices a German full professor's heroic claim to represent an entire core area of his discipline. A well-equipped American academic department may have the same number of staff or PhD-students as their German counterpart, but because teaching and research loads are spread over a larger number of faculty, the respective individual responsibilities are within sensible bounds, research teams directed by a principal investigator are smaller and work in a more intimate fashion, horizontal cooperation affecting the potential for disciplinary differentiation and inter-disciplinary work is common, and per caput productivity is enhanced (Herbst et al. 2002).

Disciplinary Differentiation, Interdisciplinarity and Renewal in Germany

Today, the Bologna process and the newly established Excellence Initiative are meant to take German universities onto a new path: of cognitive and social opening; and of horizontal and vertical differentiation through inter-disciplinary research cooperations. At the same time, the corporate self-administration of the professorial guild is increasingly constrained and partially replaced by a professionalized university management, including a university council as a monitoring body. The latter doubtlessly means interference with the handed-down ruling structures. However, the widespread new institutional controls through target agreements (*Zielvereinbarungen*) and performance indicators, designed along the principles of New Public Management, might replace creative researchers and teachers—who are guided by an inner vocation and enthusiasm—with opportunistic faculty members who try to meet targets (or to gather 'points'), to the detriment of science and education.

Contrary to the professed aims of recent reform measures, the oligarchic structures which governed research and teaching in the past have widely remained untouched. The implication is that staff is 'used' (or abused), without providing academic career opportunities, and without a prospect for the creation of new research fields or new disciplinary orientations and the associated new faculty positions which would result. Teaching lacks a foundation for disciplinary differentiation and new multi-disciplinary curricula. This shortcoming is enhanced by the fact that a growing part of teaching in peripheral areas is relegated to assistants or adjunct faculty. In Germany, the Excellence Initiative has increased the number of staff members without proper career perspectives by approximately 4,000: funds invested in this context, two billion Euro, are largely consumed by passed-on oligarchical structures, without any chances for a new development.

The Excellence Initiative accentuates the hierarchical organization of research at German universities. Of the 4,085 new positions funded in this way (until Spring 2009), 92 percent were created for doctoral or postdoctoral research assistants working under the direction of principal investigators. Of the 326 new professorial positions, 99 were established at the 'junior' level (Hornbostel and Sondermann 2009). Because these funds are available for a five-year period only, the newly financed faculty positions will have to be replaced by positions earmarked in the regular university budget, implying that there will be no improvement in faculty-staff-ratios (and, by implication, in faculty-student-ratios). In 2007, across all disciplines at German universities, the roughly 39 thousand professorial positions amounted to 21 percent of the regular full-time positions for scientists, or 14 percent of the regular full-time and part-time staff (including visiting professors or emeriti). The fields of philologies and humanities are only slightly less hierarchical (Statistisches Bundesamt 2009, 123–150). Decades after Ben-David's comparative analysis of the German chair system and the American department system (Ben-David 1984/1971, 108–168), reform measures are on the wrong track: the Excellence Initiative will not change the oligarchic nature of the German university and will fail to promote a capacity for renewal.

In its efforts to catch up with the US, the German university falls prey to a strategy which makes its old strengths disappear—without there being any prospect for a rise to new grandeur. The regime of professorial corporatism, responsible for its lacking capacity to change, is called into question by the conversion to an entrepreneurial university (Clark 1998). At the same time, the old oligarchic organization of research and teaching remains untouched. The combination of variety and competition, part of federal pluralism, is being superimposed by trends towards the formation of oligopolies and monopolies. The turn to New-Public-Management-regimes involves new forms of governance and central administration. Obviously, the German universities are subject to substantial changes, but the changes as such will not bring about the hoped for increase in achievement. The chosen processes of transformation increasingly subject science to the laws of an 'academic capitalism', in the service for capital accumulation, where education and knowledge are not seen as public goods (Readings 1997; Slaughter and Leslie 1997; Slaughter and Rhoades 2004; Geiger 2004).

The Humanities Under the Old Regime

In 2007, Germany celebrated the "Year of the Humanities". A comprehensive public relations campaign was meant to recall that the humanities have a right to exist, even in a world dominated by other concerns. The fact alone that such a campaign was considered necessary is proof of the extent to which the humanities have lost their historically evolved—'natural'—legitimacy. In a world inundated by global benchmarking, societal institutions and practices are called to demonstrate that they contribute to a nations's competitiveness. Accordingly, the humanities too were pulled into the maelstrom of academic capitalism.

In defense of the humanities, it is pointed out that their purpose transcends the categories of competitiveness and profitability; they are said to have a value of their own and should be judged according to their own criteria. Their supporters think of all those values and criteria, which made the humanities in Germany—more so than in any other country of the world—disciplines based on their own truth and methodologies. Whereas in the UK and in France language, literature and history are understood primarily as a pursuit of tradition and part of the élite formation beyond the narrow field of the teaching profession (in their form as *lettres*, arts, *histoire* or history), the humanities became a veritable science in Germany during the 19th century. This science could meet its own high claims as a pure doctrine only by being freed from all practical requirements. The university in its Humboldtian orientation, guided by "solitude and freedom" *Einsamkeit und Freiheit*), by the unity of teaching and research, by the community of teachers and students, and by education through science, was exactly the right place for this (Schelsky 1963).

The humanities, in this German tradition, were based on a largely closed educated class of teachers, Protestant priests and professors, and on the comprehensive state-support granted to schools, universities, theatres, operas, orchestras and museums. The culture state was to represent no less than the reality of the ethical idea (*Idee der Sittlichkeit*) in Hegel's sense. On this social basis, the philologies, as the embodiment of the humanities and as a specifically German product of the 19th century, could flourish and find their epistemological justification in Dilthey (1968/1883); they, therefore, owed their unique position to a homologous cognitive and social closure.

However, trends towards opening accompanied the development of the humanities right from the start. The fighting off of vocational interests or instrumental usefulness, has run through the history of the respective disciplines in Germany up to today. This defense was necessary because the humanities expanded and claimed new turf, and not because they had shrunk or were pushed aside. This development is still underway today, and is overlooked in the common complaints about the precarious situation of the various foci of the humanities and their role in culture and higher education, dominated by other disciplinary endeavors (i.e. natural and health sciences, engineering and technology, and economics and business).

A first wave of defense struggles goes hand in hand with Germany's massive industrialization between 1870 and 1914 (Ringer 1990/1969). This was also a period of an expanding secondary and high-school education. This does not mean, however, that the numbers of pupils at the classical *Gymnasium*, or the numbers of students at the universities, were declining; secondary schools and universities were merely supplemented by educational institutions with different aims, focussing on vocational—i.e. technical, practically usable and economically useful—skills and qualities. Possessing the monopoly for the education of teachers at the classical *Gymnasium* gave—and still gives—the universities their secure social basis to protect the humanities against 'alien' vocational requirements.

Social Opening and Cognitive Closure

The foundation of the homologous cognitive and social closure experienced its first weakness during the massive educational expansion in the 1970s. The great number of new universities gave the humanities a boost unknown so far. Along with the number of students, the number of faculty and assistants grew substantially during only one decade. A categorization of the Federal Statistics Office shows that after this first expansion at the beginning of the 1970s, between 1974 and 2005 the number of students in philologies and humanities (*Kulturwissenschaften*) rose from 76 to 421 thousand (by a factor of 5.5), while it grew merely from 137 to 326 thousand in engineering sciences (by a factor of 2.4) (Statistisches Bundesamt 1976, 107) (Statistisches Bundesamt 2007, 144). Ordinary staff in the philologies and humanities, however, only increased from 18 to 20 thousand during the same period, which meant a dramatic deterioration of the faculty-student-ratio. Unavoidably, this involved a dissolution of the community of faculty and students (Statistisches Bundesamt 1976, 111; Statistisches Bundesamt 2007, 149).

As a result of this expansion, many new students could not enroll in fields of their choice, in particular education, and they pursued studies in other disciplines, primarily within the humanities. With this development, the social closure of the humanities was removed, yet the social opening was in contradiction with its cognitive closure. In so far as this contradiction could not be resolved through a cognitive opening of the field, it had to be paid for with high drop-out rates and high initial unemployment of graduates, and the pressure increased to offer courses focusing on practical skills in the humanities as well. Some pioneering universities introduced new undergraduate or graduate studies—such as literary translation, teaching literature, German as a foreign language, journalism, intercultural communication, creative writing or cultural management—, but they always had to accept being accused to betray their heritage. Cultural sciences (*Kulturwissenschaften*) were discovered as an expanding sector: area studies with a focus on Japan, China, Eastern Asia or Islamic regions replaced the classical philological subjects in Japanese studies, Sinology, Indology and Islam studies; combined with this development was a turn favoring the training of communication skills: after all, in the setting of broadcasting, an expert on Islam is not expected to present exegeses of religious texts, but solid information about political events and their gestation.

The new undergraduate and graduate studies without any disciplinary identity, with hardly any resemblance to the 'original' humanities, are in no way a colonization of fields where such practical requirements and constraints of utilization did not exist before; they merely mean the logical consequence of a development which started with the massive expansion of higher education after the 1970s. The closed social world of the educated classes has now been replaced with an open social world of suitably trained service providers. The new cultural workers have long stopped regarding themselves as the gate keepers of the Grail of education, of a classical cultural good—in contrast to high school teachers and the philologists' association. Instead, they work as web designers, personality coaches, or managers of cultural institutions, municipalities or public relations (Koppetsch 2006).

Coping with the Crisis

With this turn to the present and to practical aims, the humanities entered the former domains of the social sciences which, as a consequence, involved their "social-scientification" (Wehler 1973). However, at the same time they gave up part of their cognitive closure—thus, the very core of their identity. Simultaneously, beyond the teaching profession, a growing labor market emerged for graduates in the new humanities. It is little surprising that such graduates are less interested in Goethe and the like, but more in marketing, personnel management and public relations. Nevertheless, the complaint that today's students of German language and literature cannot be excited by literature, and are not used to reading, has become standard and a companion of this fundamental change: the *doxa* (i.e. the ruling doctrine) of the field had become an orthodoxy which had to fight off strong heterodox (i.e. deviant) trends and alternative models of orientation.

The emergence of the term Cultural Sciences, which took place during the 1980s, can be identified as the replacement of the humanities' orthodoxy with a new paradigm (Jaeger et al. 2004). Cultural studies, formed in the Anglo-Saxon world, represent an important disciplinary complement of classical humanities (Fiske 1988; Chen and Morley 1996; du Gay et al. 1997). The broadening of disciplinary foci, however, also brought about a certain banality of topics: it has become possible to deal with the punk or hip-hop scene (outside of the framework of ethnographic or musicological studies), with management or negotiation styles, as well as with 'high-minded' literature. The profanation of the humanities was accompanied by a corresponding loss of societal appreciation: the humanities and the related 'priesthood', studying 'holy' texts, were taken down from their élitist pedestal to the solid ground of a discipline which furnishes skills and necessary know-how for the various activities of our daily existence. Once on this track, there will be no stopping. Social-scientification have long followed the trend of "natural-scientification". Linguistics started this process by borrowing approaches from the natural sciences. The latest development in this respect is the spread of neuro-linguistics and its instrumental commercialization: after all, neuro-linguistics can be successfully applied in speech therapy. This development does no longer concern hermeneutics as an interpretation of texts following Dilthey's tradition, but serves to exploit society's human capital.

Eventually, the new cultural sciences have increasingly succeeded in obtaining their slice of funds from the German Research Foundation (DFG) and in extending their research potential through the Volkswagen and the Fritz Thyssen foundations. Between 1972 and 2006, the annual support volume provided by the DFG for humanities and social sciences rose from 39 to 200 million Euro (i.e. by a factor of 5.1), while it increased from 46 to 308 in engineering sciences (i.e. by a factor of 6.7) (Statistisches Bundesamt 1974, 112; Statistisches Bundesamt 2007, 159). Today, humanities and social-sciences are recipients of approximately 15 percent of the German research funds available, and the Excellence Initiative provided almost 17.5 percent of its funds for that sector, i.e. around 350 million out of a total of two billion Euro. Therefore, the humanities might be underfunded in Germany, like other sciences, but they are not being 'displaced' due to the funding process.

The New Regime

Research in the 'new' humanities has nothing in common any more with traditional scholarship or the meticulous philological work on long-term editorial projects. On the way to expansion, the humanities submit to the laws of research management having broken free (Gibbons et al. 1994, 90–111). "Little science" has become "big science" (de Solla Price 1963). Cooperation is indispensable now, even if no knowledge is gained and it involves greater expense of time due to the necessary coordination. In such interdisciplinary networks, the focus on clear-cut questions, research programs and methods is frequently lost. Scholars become research managers who are mainly involved in creating follow-up job opportunities for their staff. Doctoral students work in large PhD-factories, and assistants work in large research centers under the dictate of a painstakingly coordinated program. Large-scale research perpetuates itself and generates its own extended demand for staff: nobody has an overview any longer over a partial scientific field, let alone the know-how to master it.

The flourishing of large research centers in the cultural sciences is accompanied by the generous funding—on the part of the DFG and by an associated bureaucratic overhead—for tasks like project management, coordination and public relations: "Now, however, [. . .] we need [. . .] new, salaried communicators for the excellence clusters and their central monitoring—a kind of hitherto unknown, academically trained mid-level faculty" (Schloemann 2007).[9] Increasingly, funds flow into the management of research activities instead of the research itself. A shrinking innovation potential is the consequence; the broader support of creative researchers, working on their own or in small teams, appears to be the more promising strategy.

The developments just described are often said to be based on a functional necessity, by claiming that collaborative research is required in the disciplines under consideration and in order to address complex problems. Nevertheless, there is no proper evidence in a range of fields which would support this supposed necessity. In the humanities and social sciences (i.e. for English or American studies, history, education, sociology, business administration and economics) for which data were gathered by the *Centrum für Hochschulentwicklung* (CHE) (Berghoff et al. 2005, 2006; Münch 2007, 433–439), no significant positive relationship could be established between external funding (input) and publications (output) per researcher (faculty and staff) employed. Hence, the proliferation of centers engaged in large-scale research (outside the traditional fields for "big science" like particle physics) cannot be explained on the basis of such relationships, and it may be due to the outlined mechanisms of a circular accumulation of economic and symbolic capital broken free. Economic capital, in the form of a collaborative research centre, for instance, is considered a mark of quality and, hence, a symbolic capital which can, in turn, be reinvested to accumulate economic capital: this is the form of a 'capitalism' in the cultural sciences.

[9] My translation.

'Excellence clusters' established in the framework of the recent German Excellence Initiative form the preliminary peak of this development. Whether or not large-scale research centers foster creativity or an open, broad discourse, is of secondary importance (if at all). The affluence of 'excellence clusters' becomes self-perpetuating: guest scholars can be invited who lend the cluster the splendor it could not achieve on its own. The absorptive power of such clusters devours everything that would bloom without the imposed cooperation and coordination requirements. An associated consequence of this development is the systematic over-researching of subject matters. The parallel with overgrazing of a common pasture by cattle or sheep cannot be overlooked (Hardin 1968). In view of all the things that have been researched already in the field of "cultural foundations of social integration" or the "normative foundations of social order", it is foreseeable that such excellence clusters, which receive a 6.5 million Euro support per year, will not generate any revolutionary new findings. Achievements cannot be forced by large investments and, within a proper habitat, it is left to chance where a breakthrough will happen (Merton and Barber 2004). Managers of large-scale research enterprises are more hindered than spurred on by their bureaucratic duties to generate 'spectacular' findings, and there appear to exist no significant scale economies in the humanities and the cultural sciences as they are known from industrial mass production.

The old philological orthodoxy leads now a shadowy existence. The fact, that not all scholars in the humanities submit to the new *doxa*, yields the strange phenomenon that the German Science Council (*Wissenschaftsrat*) and the DFG, on the one hand, have worked out funding guidelines for humanities oriented toward the old scholar model, using keywords such as "free time" and *opus magnum* (Wissenschaftsrat 2006; DFG 2007); on the other hand, these funding agencies fuel the new model of large-scale research in the cultural sciences to an even larger extent within the framework of the Excellence Initiative. In view of the current state of affairs, we have to assume that the old orthodoxy will not succeed in leaving its niche, since it simply lacks the foundations (i.e. the former social and cognitive closure) to attack the new paradigm from within a marginalized position.

The founder of big science in the humanities was Theodor Mommsen (1817–1903) (Rebenich 2004). For Mommsen, this kind of planned and organized research meant long-term philological editorial projects, as they were established by the Prussian Academy of Sciences. Large-scale interdisciplinary research, involving collaborative research centers (*Sonderforschungsbereiche*)[10] working on a common theme for up to 12 years, is a new kind of collaborative research. This is all the more true for the clusters of the Excellence Initiative. However, because there is little disciplinary differentiation within the German hierarchical system dominated by a small number of chair-holders, the number of principle investigators who work at the intersection of different disciplines is frequently too small. Hence, large-scale joint projects are very often only symbolic constructions with no real synergetic effects:

[10]Introduced by the German Research Foundation (DFG) in 1968.

they are designed mainly as a funding scheme for the professors involved and their respective university, and as a device to extend power and influence.

Many talented prospective scholars working under the direction of a chair-holder have little chances to advance to the professorial level and their gift is wasted, at least within the framework of higher education, and cannot be used as a source of renewal. Right from the start, this has been the major failure of the German university as well as that of non-university research institutes as they were envisioned by leading scholars like Theodor Mommsen or Adolf von Harnack (1851–1930). Harnack authored the founding idea behind the Kaiser Wilhelm Society, founded in 1911, when he acted as its first president. The Kaiser Wilhelm Society started a history of expanding research outside the realm of the university, divorced from teaching, that is, far removed from Humboldt's idea of integrating research and teaching. Harnack's idea of investing in the charisma of a single scholar, appointed as director to a lifetime—i.e. tenured—position, and leading a bunch of young scientists to the forefront of research, is still alive as the core principle of the Max Planck Society that replaced, in 1948, the Kaiser Wilhelm Society after World War II. This philosophy is still celebrated as the "Harnack Principle", proudly presented on the website of the Max Planck Society (Planck 2010). The belief in the extraordinary qualities of a director of research is one of the most retarding elements in the German research system: even the best researchers will eventually become an obstacle to renewal if they are forced to preside over a large staff. Implicitly, such systems prevent independent research of scholars of similar merit, young or mature, and reduce the natural variety for new ideas. It is remarkable to see how one hundred years of failed investment in the advancement of research are still celebrated as a success story; this can only be explained by the lock-in effects of historical paths (Pierson 2004). The achievements of the American university department, in contrast, based on a community of equals, tells a completely different success story.

If we compare an American interdisciplinary research center—or institute—with the German collaborative research center or 'excellence cluster', we see different institutional settings. The far more accentuated differentiation of disciplines within US departments makes it possible to unite specialists from within departments, or scholars and professionals from various departments who work in the same field or who can build such a field on firm grounds. They research together with some doctoral and post-doctoral students who, in turn, have a fair hope to make a scientific career and become faculty members themselves in precisely that field, because positions exist in their specialities in widely differentiated departments. Apart from doctoral and post-doctoral fellowships, these centers or institutes do not need large funding, at least not as far as humanities or social sciences are concerned, and they exist in loose associations with their members as long as a common research focus exists. In contrast, the German center employs far more permanent staff, often senior, subaltern scientists, requiring a permanent stream of basic funds which should be used, instead, for an enlarged spectrum of faculty positions.

Conclusion

The heyday of the sciences in Germany in the 19th century began with the integration of research and teaching in the university. This rise of the sciences occurred in a setting of social and cognitive closure which separated the university from practical demands and allowed it to focus on the fundamental questions and an inner core of the various disciplines. In this way, the German university achieved a leading position in the world: it became the model for the successful advancement of science.

At the beginning of the 20th century, American scholars who had visited, or studied at, German universities began to emulate the German idea of integrating research and teaching and introduced the science laboratory and graduate studies in the form of a graduate school. However, the German idea of the unity of teaching and research was not simply copied but fused with the earlier ideal-type of the British college with its focus on teaching and the more intimate faculty-student relations. Not the chair was to form the building block for the new research university, but the department based on a collegial culture. This eventually allowed for a much broader disciplinary differentiation, for the establishment of interdisciplinary research centers at the intersection of a variety of disciplines and sub-disciplines, and for a greater renewal potential in the form of establishing new academic or professional fields and specialities.

This institutional setting proved to contain the crucial competitive advantage of the sciences, humanities and professional fields in the United States. The German university, on the other hand, abandoned its own professed ideal: with the foundation of the Kaiser Wilhelm Society in 1911, the path was prepared to support and expand research with minimal institutional ties to a teaching institution: the German system saw no need for—and missed the chance to maintain—the integration of research and teaching by introducing something like graduate schools throughout the 20th century. The successor institutions of the Kaiser Wilhelm Society—the Max Planck Society as well as a whole series of other non-university research institutes that have been grouped together under the roof of the Leibniz Society, the Helmholtz Society and the Fraunhofer Society—are recipients of no less than about 40 percent of the public monies spent today on research. With the stagnation of block grants and the concomitant expansion of third-party funding beginning with the 1980s, a further separation of teaching and research can be observed (Schimank 1995).

All these changes have never affected status, role and dominant position of chairholders or directors, working either within universities or non-university research institutes: in both cases, they command over a range of permanent positions, filled with senior staff who do not enjoy scientific freedom, apart from doctoral and postdoctoral students with a temporary employment status. Both the difficulties to manage institutes of such size, and the fact that senior staff is more often chosen for their subserviency than for their brilliance, makes this form of institute deficient. While the German university eventually—after 1968—opened itself to a broader spectrum of students in social terms, it was unable to open itself in cognitive terms, both with regard to applied research and professions outside the common spectrum, as well as with regard to establishing new research fields and sub-disciplines located at the intersection of current disciplinary orientations.

In the wake of New Public Management and corresponding neo-liberal reforms, the turn away from block grants and toward third-party funding has aggravated the structural deficits of the German academic system. The most recent programs within the framework of the Excellence Initiative continue along this path. Though there are elements which indeed aim at overcoming some of the weaknesses of the system—such as the establishment of graduate schools and the improved cooperation of universities with non-university research institutes—the discretionary resources of the corresponding programs are largely being absorbed within the existing structures and prop them up: the oligarchic structure of the German university has become even more accentuated than before. Apparently, no swaths can be cleared to replace the chair-structure by a departmental structure. Under these conditions, even the new graduate schools will not be in a position to change the domination of chair-holders. Furthermore, the new cooperations between universities and non-university research institutes do not attempt to integrate research and teaching, and they may even constitute an advanced form of separating research within the university from teaching.[11] Ultimately, with the increasing concentration of research funds on a very small number of research centers and the inauguration—or better: declaration—of 'elite' universities, Germany might lose its last competitive advantage (vis-à-vis e.g. Britain or France), namely the comparatively large number of universities in a position to recruit productive scientists. However, no prospects are in sight for disciplinary differentiation, inter-disciplinarity and renewal that would bring the German university closer to its relatively effective counterpart in the United States.

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[11] On the suggestion of the Science Council, the newly established research islands are to be supplemented with new teaching professorships.

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Chapter 7
The Scion and Its Tree: The Hebrew University of Jerusalem and Its German Epistemological and Organizational Origins

Shaul Katz

Abstract The chapter focuses on the transfer of German science models that affected the newly established Hebrew University of Jerusalem (HUJ). The chapter traces the preparatory phase and the early decades of the HUJ, examining three constituting parameters of development: German-ness, Jewish-ness, and local-ness—in mathematics, natural sciences, oriental studies, Jewish studies, and medical research.

The international diffusion of the European research university included the export of models or ideal-types of higher education and forms of research.[1] While all universities, at least since the beginning of the nineteenth century, resemble one another, those that emulated the German model differed according to their particular expression of 'German-ness'. This chapter sets out to examine the extent to which the Hebrew University of Jerusalem, at least during the initial phase of its operation in the second half of the 1920s, when it was made up of institutes rather than faculties, was a result of the diffusion of organizational models and research traditions created in nineteenth-century Germany and hence a scion of the German academic tree.[2]

[1] See Shils and Roberts (2004) and, in particular, the short section therein entitled "Palestine" (pp. 190–191); for a contemporary comparative outlook see Bentwich (1939). It should be noted that both collections place the Hebrew University of Jerusalem 'outside' the framework of Europe.

[2] Shils and Roberts (2004) say that "The Hebrew University of Jerusalem [. . .] adhered to no single national model although British provincial and German influences dominated [. . .]" (pp. 190–191). Compare also footnote 73 below on page 138.

The present chapter is an expansion of a similar theme presented in an earlier, Hebrew, version (Katz 1997). My many thank go to Mrs. Ruthie Rossing of Jerusalem for her English copyediting.

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German Jews and Nineteenth and Early Twentieth-Century Science

The involvement of nineteenth-century European Jewry with scholarship manifested itself on both the individual and the collective levels. On the individual level, as Jews were increasingly admitted to European universities, the number of prominent scholars and scientists of Jewish origin grew (Charpa and Deichman 2007, 3–36; Richarz 1974). Some of them became intellectual leaders in their respective academic fields and proponents of an academic ideology professing the importance of 'science for its own sake'. The high proportion of prominent Jewish scholars and scientists was perceived by some as evidence of a Jewish aptitude for creative, abstract work or, alternatively, of an illegitimate penetration into universities and research institutes—or both. A widespread popular explanation (especially among Jews) for the phenomenon of Jewish excellence in science and secular scholarship was that the intellectual virtues (allegedly) engendered by their early *yeshiva* experience had prepared Jews for the abstract, analytic thinking required for academic study. In some quarters of the academic arena and beyond, the same alleged Jewish distinction in abstract thinking nourished antisemitic critiques of the 'decadence' of Jewish mathematics' or 'Jewish physics', ultimately eventuating in the Nazi persecution of Jewish academics (Rowe 1986).

On the collective level—paralleling many nineteenth-century European movements for a national, cultural and social renaissance (whether reformist or radical in nature)—there emerged two Jewish movements that put their faith in 'enlightenment' and modern education as well as in the intellectual and instrumental value of modern science. The first, especially in Eastern Europe, was the Jewish *haskalah* (enlightenment) movement that aimed to regenerate the collective life of the Jewish communities (in part, with the goal of making Jews 'productive'). This was pursued by means of educational reform and the publication of popular Yiddish- and Hebrew-language journals and books, written mainly by amateurs, which advanced current scholarly and scientific views. Arguing, *inter alia*, that individual participation and distinction in the modern scientific enterprise was a legitimate and noble activity that harked back to the traditional ideal of *torah lishmah* (learning for its own sake), prominent figures among the proponents of *haskalah* were widely successful in promoting a belief in the emancipating force of science and technical knowledge.[3]

At about the same time, Jewish intellectuals in Western and Central Europe began adapting contemporary academic and humanistic scholarship to the study of traditional Jewish texts and Jewish history. This new *Wissenschaft des Judentums* ('Science of Judaism'), at first advanced by scholars with the aid of Jewish academic societies and journals, later led to the establishment of several academically

[3]On the *haskalah* see, e.g., Feiner (2003) and Sorkin (1987). For an analysis of relevant publications of popular science in Hebrew, see Soffer (2004) and Shavit and Reinharz (2011); for Yiddish, see Métraux (2007). See also Zalkin (2005).

oriented rabbinical seminaries which were intended, within the various Jewish religious movements that had emerged, to replace or supplement the traditional *yeshiva*. The *Wissenschaft des Judentums* enterprise also sought to reduce the cultural distance separating the Jewish minority from the gentile civil society within which it was embedded and to facilitate Jewish integration. A central component of this 'Science of Judaism' was its insistence that the academic study of traditional texts and Jewish history, cultivated in the respective local majority language, was the best means to guarantee both the preservation and the regeneration of Jewish—ethnic-religious—minority identity in a changing, and perhaps even perplexing, world.[4]

The Zionist movement, originating in late nineteenth-century Europe, took a different approach to Jewish minority existence and the collective reconstruction of the Jewish people. Its proposed solution, mass immigration to Palestine (*Eretz Israel*, then still under Ottoman rule), had the dual aim of creating a Jewish majority population in its own sovereign territory and of reviving Jewish-Hebrew culture in that space. Zionism, too, turned to enlightenment, i.e., to scientific research and technological development as the main instruments for the realization of its vision.[5] The World Zionist Organization (WZO), founded in 1897, advanced two major programs. The first (echoing earlier *haskalah* projects to make Jews 'productive') aimed to create settlements in both urban and rural, agricultural settings—the *kibbutz* being the best-known example of the latter; hence the Zionist interest in founding medical and agricultural research stations along with academic colleges to train engineers and agronomists.

The WZO's second major program sought the renaissance of Jewish-Hebrew culture by, among other ventures, creating a Hebrew national educational system epitomized by the establishment of the Hebrew University of Jerusalem. It is noteworthy that the Zionist vision for this endeavor incorporated a strong commitment to pure or basic science, 'science for its own sake'. This emphasis may be explained by the enlistment of a number of Jewish scientists and scholars, mainly members of the German scientific elite, as supporters of the Hebrew University project. Envisioned as a Hebrew-language research university, the Jewish national university was to propagate and excel in various sciences as well as in academic Jewish studies.[6]

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[4]There exist an abundance of sources regarding the origin and the nature of חכמת ישראל (Wisdom of Israel/*Wissenschaft des Judentums*/Science of Judaism/Jewish Studies). For an historical perspective and contemporary discussions, see Meyer (1967), in particular the sixth chapter on "Leopold Zunz and the Scientific Ideal" (pp. 144–182); and Goodman et al. (2002). For a comparative outlook on the history of the two seminars mentioned below and similar institutes, and on the origin of the Institute of Jewish Studies in Jerusalem, see Schwartz (1997).

[5]The best example is the Zionist utopia envisaged by Theodor Herzl (1902).

[6]Most of the speakers at the Hebrew University's April 1925 inauguration ceremony, such as 'national poet' Chaim N. Bialik, confirmed their vision and hope that the new university would excel in pure science, which they equated with 'Torah for its own sake'. See Hebrew University, *The Inauguration*, April 1, 1925, small format. See also Judah L. Magnes on the virtues of "Torah for its own sake [. . .] Torah is not a 'spade to dig with' ", in Magnes (1936b).

Three phenomena made the vision of establishing a Hebrew University in Jerusalem seemingly attainable: (i) Jewish participation in the Western academic elite, suggesting that leading Jewish academics could be recruited to create and staff the required faculties of the planned institution; (ii) organizational and epistemological traditions supporting scientific scrutiny of the Jewish heritage as a kind of national, scholarly enterprise; and (iii) broad popular Jewish public support for establishing a Jewish university manifesting a commitment to pure science.

The Hebrew University Project: History, Goals and Initial Implementation

The idea of establishing a Jewish university, as an expression of nineteenth-century Jewish nationalism, had been raised before the WZO already at the outset of its activity in the late 1890s. In 1913, this idea became the project for the 'Hebrew University of Jerusalem'. Activities on behalf of the university took on an operational cast after the British conquest of Palestine from the Ottomans, in the final stages of World War I. With this came a growing awareness on the part of the principal activists, headed by chemist Chaim Weizmann (1874–1952),[7] that the plan to establish a 'complete university'—as it was spoken of at the time—must temporarily be set aside. Preference was given instead to a modular plan for an institution based, first and foremost, on research institutes in which there would be no systematic instruction for undergraduates. An operational memorandum written in 1920—the "Preparations Report" —presented the principles that were to guide the institution in its first years:

> "The guiding principle must be to emphasize the quality even at the expense of size and quantity. In the circumstances it was agreed that it was most appropriate to start the Science Departments as research institutes and not as Teaching Faculties. Such institutes, even if small, could reach a high standard in spite of the limited funds and restricted housing accommodation which could be provided at the beginning in Jerusalem without involving fresh building operations [. . .] They will, however, help to create the necessary scientific atmosphere and establish the reputation of our University, thus ensuring it against the low scientific standard and a correspondingly low reputation like that from which new universities in backward countries, especially in the East, generally suffer [. . .] They will easily be transformable and will gradually develop into teaching faculties".[8]

Each research institute in the natural sciences was to have an academic staff consisting of "six regular instructors (two professors, two lecturers, and two assistants)"—but that lay in the future. For the moment, in order to get the institutes off the ground,

[7]First President of the State of Israel, 1949–1952.

[8]"Report [on] Preparations for the University in Jerusalem (to be submitted to the Annual Conference of the Zionist Organization)" (henceforth, Preparations Report), English version, pp. 1–2, Item 2^0 34 2998 in the National Library of Israel Catalogue. The possibility that it might be easier to establish not whole faculties but single institutes had already been raised in the 1913 plan for the Hebrew University (Reinharz 1985, concluding chapter).

the university's founders made do with a single head for each institute, who was to participate actively in its establishment, including the selection of other staff members.[9]

The decision to start the Hebrew University with research institutes left its founders with no clear model to emulate, certainly not a German one, for the typical German university was a State university in which the various faculties within which instruction took place comprised the principal organizational units. The connection between the university and its research institutes, on the other hand, was a partial one, since the funding for the initial capital investment and the running costs of such institutes often came directly from the government or from third parties (public and private bodies outside the university). In contrast, the authors of the Preparations Report believed that each institute should have a public board of its own that would concern itself, among other things, with the provision of the necessary resources. The university's founders therefore sought to establish a new organizational model for the institutes in the making. As they wrote:

> "We are aiming at combining those elements in the continental system of the State Universities with those elements in the English system of Independent Universities governed by Trustees as shall be found to fit in most naturally with conditions in Palestine".[10]

Hence a hybrid model was proposed and implemented for the fledgling university's institutional, organizational framework. The examination of 'German-ness' of the Hebrew University might better be focused on its research setup, at least during the initial period when the institute-based structure was meant to be its temporary organizational framework.[11]

As already noted, those who were actively working toward the university's establishment, as well as their supporters, anticipated that the institution would attract the leading Jewish scholars and scientists of the period, whose stature would make the "Jerusalem university", as it was then called, both a "spiritual center" for the Jewish people and a "beacon for the universities of Europe".[12]

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[9] Preparations Report, p. 3.

[10] Preparations Report, p. 5; see also Shils and Roberts (2004).

[11] The various scholars who have analyzed the university's organizational structure—and the apportioning of administrative and academic authority on the level of the institution as a whole—and pointed in this context to its 'German' qualities were concerned mainly with periods subsequent to that under discussion here. See e.g. Ben-David (1986); see also Ben-David (1991a), a collection of Ben-David's scientific contributions.

[12] Its leading lights would be "the greatest experts in their fields, such as Ehrlich in bacteriology, Bergson in philosophy, Einstein in physics, von Wassermann in serology, Freud in psychiatry and psychology, Oppenheim in neurology, Loeb in biology, and so on" (cited from the Hebrew daily, *Do'ar HaYom*, March 1, 1922; at that time, Ehrlich had already been deceased for several years).

The members of the first "Academic Council [were] Prof. A. Einstein (chairman), Berlin; Prof. Besredka, Paris; Dr. M. Buber, Happenheim; Prof. Z. Chajes, Vienna; Prof. Ehrmann, Berlin; Prof. J.N. Epstein, Jerusalem; Prof. A. Fodor, Jerusalem; Prof. S. Freud, Vienna; Prof. Hadamard, Paris; Prof. J. Horovitz, Frankfurt and Jerusalem; Prof. J. Klausner, Jerusalem; Prof. S. Klein, Jerusalem; Prof. I. Kligler, Jerusalem; Prof. E. Landau, Göttingen; Prof. L.S. Ornstein, Utrecht; Prof. O. Warburg, Tel Aviv; Dr. Ch. Weizmann, London"; see Hebrew University Yearbook (1925–26, 10).

But expectations are one thing, and their realization another. Indeed, the Hebrew University project garnered the support of many members of the Jewish academic elite, some of whom agreed to join its Board of Governors or its Academic Council, which functioned for about a decade as the 'Senate-in-the-making", with Albert Einstein as the first chairperson. But only a few of these scholars actually moved to Jerusalem upon the university's establishment, or they remained on its staff for a short period.[13]

The rise of the Nazis in Germany in the early 1930s, and the enactment of "racial laws" in Fascist Italy towards the end of that decade, only somewhat changed this picture. However, those who arrived during the second half of the 1930s joined a more fully formed, comprehensive university framework, certainly from an organizational point of view. The first faculty, the Faculty of Humanities (*Geisteswissenschafliche Fakultät*), had already been established in 1928, while the university's second faculty, the Faculty of Science (*Mathematisch-Naturwissenschaftliche Fakultät*), was established in 1935–36, the same year in which the Academic Council was replaced by the locally based Senate. It was this latter body that chose the institution's first academic head—the Rector—from among the professors in Jerusalem. However, the Board of Governors retained its status as the highest governing body of the Hebrew University with Dr. Chaim Weizmann as its chairman.

During the period of the institutes, with the Board of Governors and the Academic Council and its various committees and chairpersons physically located outside Jerusalem, the university was effectively and imperiously run by Dr. Judah L. Magnes (1877–1948), an American social activist rabbi who had already been living in Jerusalem for several years. To his credit redounded the successful collection of the initial donations that facilitated the opening of the Institute of Jewish Studies, which practically and symbolically preceded the opening of the university as a whole. It was the Board of Governors that appointed Magnes to the post of 'Chancellor' (*Kanzler* in German), which was far from being merely an honorary position, as the English term might imply, or that of a bureaucrat appointed by the government to administer the university, as the German one would. The Hebrew University had absolutely no connection with the British administration in Palestine; the Mandatory Government recognized it as an "Institution of Public Utility" and refrained from impinging on its affairs.

Adopted Academic Traditions

From the beginning of the nineteenth century through the period under discussion, the German university system, as already implied, was considered the most developed in the world and served as a model for other university systems (Ben-David

[13]Einstein himself exemplifies this phenomenon. His convoluted relationships with the Hebrew University are studied in depth in Rosenkranz (2011).

and Zloczower 1962; Schwinges 2001). Moreover, it was in Germany or in other countries within the German cultural domain, such as Switzerland, Austria, or Bohemia (later part of Czechoslovakia) that the majority of the founders and senior academic staff members of the Hebrew University had received their academic training.[14] However, the personal background of the staff is not the only factor in this discussion. In order to examine the extent to which the Hebrew University in its early years may be said to be a scion of the German academic system—a copy or a significant representation of the German original, transplanted into a new territory and milieu—a set of criteria for probing this 'German-ness' as a parcel of different epistemological and organizational models shall be offered.

However, these same criteria are also meant to facilitate the identification of proposed alternatives to the original German models, as well as changes introduced as a result of their integration into a new national surrounding in an extra-European territory in which, from a European point of view, both the natural and the human environments were new and different. In what follows, then, the new university's character will be examined along three parameters: its 'German-ness', its 'Jewishness', and its 'local-ness'.

Given the organizational structure described above, these parameters must be considered within the context of the research institutes along two axes:

- the epistemological ideals and research agendas that guided the actual research activities carried out in the various institutes during the university's formative years; and
- the organizational structure of the institutes and, particularly, their hierarchical configuration and practices.

The institution's 'German-ness' may be determined by the degree to which it adhered to the prevailing views in the German academic world regarding the intellectual and practical value of academic research, and by the degree to which the scholarly work pursued in Jerusalem kept to the research agenda, goals and priorities characteristic of German scholarship of the day. A central criterion in this regard is the institutes' adherence to the ideal of 'pure' science which, in its most extreme form, meant rejection of—and even contempt for—'applied' science.

Akin to the idea of pure science is what we would today call 'basic' research (OECD 2002). Within the same discipline, scholars who subscribe to either of these ideas may have similar priorities and research goals. However, those who define themselves as being engaged in basic research will justify research because the new knowledge they discover may someday prove useful to applied research and, ultimately, will benefit human life; they may formulate their research programs in

[14]In 1929, the staff members who had been trained in the German university system included seven of the eight full professors (two of whom had the rank of Visiting Director); none of the associate professors; 11 of the 14 lecturers; and 13 of the 18 instructors or assistants. See the discussion of the Institutes of Oriental Studies and of Jewish Studies below (pp. 117 and 131). For a prosopographic analysis of members of the staff of the Hebrew University at that time and later, see the comprehensive study by Telkes-Klein (2004).

response to findings emerging from real-world problems or applied research. Those who see themselves engaged in—or committed to—pure science, on the other hand, will claim that their work is of intrinsic value and needs no extrinsic justification; they will formulate their research programs only in response to questions emerging from within their own scientific disciplines.

A further criterion for the institutes' German-ness is the extent to which they adopted the organizational models that characterized research institutes in the German university system.

The institutes' 'Jewish-ness' may be assessed by the extent to which they adhered to the scholarly ideals and research agendas that characterized the principal currents of Jewish studies in the German milieu, that is, the *Wissenschaft des Judentums* tradition. Another parameter is the extent to which such studies in Jerusalem responded to Jewish national-Zionist aspirations and reflected the minority status of Jews living in a context devoid of political sovereignty, that is, from the Zionist point of view, the Diaspora. Pertinent as well was the degree to which the institutes adhered to the organizational mold of institutions of Jewish studies established in Germany.[15]

The university's 'local-ness' is appraised on the basis of scholarly and organizational responses to the emergence of a new type of Jewish national expression, Zionism, which demanded a kind of cultural revolution and, eventually, political sovereignty. Zionism also demanded the formation of a new relationship with *Eretz Israel* (the Land of Israel), its landscapes, and its history. The local character of the institution is further defined by the degree to which research embodied a commitment to the Zionist project and to the country's inhabitants as well as to those of the neighboring regions.

A basic criterion that shall be used in determining the extent of the institution's German, Jewish, or local character is that of the research materials chosen by the University's scholars and scientists. A distinction is proposed among three types of research materials—as defined from a European (or European Jewish) point of view—that were the foci of the research carried out in the Hebrew University in the period under discussion:

- 'universal materials', such as mathematical entities, molecular structures and processes, or Greek and Latin texts ('classics');
- 'Jewish materials', mainly texts written in Jewish languages, such as biblical and Talmudic scripts, Yemenite Jewish poetry, or Hebrew literature from the period of the *haskalah*;

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[15] In both the Institute of Jewish Studies and the Institute of Oriental Studies, a striking number of academic staff members both held doctorates from German universities and had been trained or active in the German-Jewish academic system; this included two out of three professors and three out of seven lecturers in the Institute of Jewish Studies (three lecturers did not even hold doctorates when they joined the staff, but two of them had been trained in German-Jewish academic institutes). Menachem Milson (1996) has written of the Institute of Oriental Studies, "This founding generation of the Institute [of Oriental Studies] may be characterized by the fact that all of them but [one] were born in Europe; all but [one] were graduates of German universities; and all but [one] had a strong background in Jewish studies".

- ‘local materials’, on the one hand components of the land’s natural environment, such as the minerals in the Dead Sea, the flora of Palestine, or the country’s agricultural pests; and, on the other, the country’s archaeological and ethnographic resources. The ethnographic materials could be approached in two ways; for some researchers they were of interest, like many of the other materials listed here, for ‘Palestinological’ purposes—that is, for their potential to enable the reconstruction of daily life in the past, particularly biblical periods; in contrast to this past-oriented approach, researchers in the social sciences were interested in studying the ethnic variety of present-day Palestine (Abuhav 2004).

The above categories should not be taken as mutually exclusive.

An operational criterion for determining the degree of the university’s German-ness, Jewish-ness, or local-ness is the type of academic journal in which results of studies were published, that is, the readership to which research findings were directed. In this context one should distinguish among disciplinary academic journals (such as chemistry, botany, or zoology); journals as platforms for professionally oriented research communities (such as medicine or agriculture); and science journals with a broad spectrum of foci (along the lines of *Nature* or *Science*, which publish reports on discoveries assumed to be of interest to a wider scientific community) (Katz and Ben-David 1975).

German Models of University and Other Research Institutes

There were two models of university research institutes in Germany. The most widespread, the ‘single-disciplinary’ model, was headed by a single (*ordentlicher*, full) professor representing a particular scientific discipline within the university. The heart of the institute consisted of its research facilities and/or library, which were meant to serve the needs of the institute’s head and his immediate subordinates and for training the associated research students. The second model, the ‘dual’ or ‘multi-disciplinary’ model, was headed by two or more professors representing different areas of specialization within a single broad scholarly field. The institute was jointly directed by its professors, each with his own group of subordinates and research students, and he determined the content of their research. In either model, following the prevailing German tradition, subordinates had very limited research autonomy, and as long as they remained in that status, they did not share in the academic ideal of what was viewed in the German system as *Lehr- und Lernfreiheit* (the freedom to teach, and the freedom to learn). The criterion proposed here for determining the degree of the presence or absence of research autonomy is the way in which the director of the institute and his subordinates were presented as authors of the publications emerging from the research in which they shared.

Apart from the university research institutes, Germany had a broad variety of other research institutes that were not directly connected with academic instruction. The institutes of the *Kaiser-Wilhelm-Gesellschaft*, for example, were intended to provide especially distinguished scholars with the conditions for unhindered research, free of teaching commitments.[16] Other research institutes were aimed at furthering knowledge in particular fields of the natural sciences or the humanities and functioned in connection with universities, museums, or libraries.[17] Still other institutes were established for the purpose of applied research in the public or private sectors (Pfetsch 1970).

Alongside the 'metropolitan' research system that operated in the 'center' of the German cultural setting were two additional categories of research organizations and institutes that operated outside the center. The first category included those that operated on the periphery of that setting, with the intention of eventually being integrated into the center. The relevant example here is the organizational infrastructure within which the disciplines of the *Wissenschaft des Judentums* were developed. The second category included organizations and institutes whose *raison d'être* was to operate in extra-European contexts, combining imperial with scientific interests.

The Research Institutes of the Hebrew University: Continuity and Change

After the Hebrew University of Jerusalem was officially opened in the spring of 1925, and for several years thereafter, it operated as an organizational umbrella for a few research units which were considered institutes or departments within the university-in-formation. These were: the Institute of Jewish Studies, the School of Oriental Studies, the Institute of Chemistry, and the Institute of Microbiology with two departments for medical research (Department of Parasitology and Department of Hygiene). A few years later the Hebrew University had been expanded by two additional institutes, the Institute of Palestine Natural History and the Einstein Institute of Mathematics. All this was in keeping with the concept of the Preparations Report (see p. 106) that the university would operate in a way that recalled—as Weizmann

[16] On institutes and seminaries within the German universities see McClelland (1980, 280–287). On the *Kaiser-Wilhelm-Gesellschaft* see: Die Gesellschaft (1961). The Hebrew University's founders were familiar with the *Kaiser-Wilhelm-Gesellschaft* and others. Einstein was the Director of its Physics Institute in Berlin from 1917 until he left Germany. The institutes of the *Kaiser-Wilhelm-Gesellschaft* were founded, among other things, on the model of the Pasteur Institute, with which Weizmann was closely acquainted. See Reinharz (1985, 379). Of relevance in this context is Weizmann's initiative, in 1914–1913, to establish an institute for medical research in Palestine, to be funded by Baron Rothschild; see: Reinharz (1985, final chapter).

[17] Such as the foundation for scientific research established by King Frederick August in Leipzig, under whose auspices a multi-disciplinary institute concerned with ancient, medieval, and modern history, geography, and art history operated. See Haas (1930). Another example pertains to the research departments of the *Naturhistorische Museum* (Natural History Museum) in Vienna, which were concerned with disciplines such as mineralogy-petrography, geology-paleontology, and botany; see Wettstein (1930).

probably would have preferred—the Pasteur Institute of Paris and the Institutes of the *Kaiser-Wilhelm-Gesellschaft* of Berlin.[18]

However, as a result of pressures from both within and without the university, its first academic instructional framework—the Faculty of Humanities—(see p. 108) began operating in 1928, bringing together the Institute of Jewish Studies, the Institute of Oriental Studies, and several courses and research activities in the humanities. The last of these—'courses' in philosophy, history, archaeology, and classics—were not initially linked to research institutes but were later combined and upgraded in the framework of the Institute of General Humanities. The courses given in the Einstein Institute of Mathematics were also joined, at least temporarily, with the new Faculty of Humanities.[19]

The Single-Discipline Institutes of the Hebrew University: The Einstein Institute of Mathematics

Edmund Landau (1877–1938), one of the greatest mathematicians of his generation, was among the leading figures of the renowned Institute of Mathematics at the University of Göttingen.[20] Upon the formal inauguration of the Hebrew University, Landau announced his willingness to come to Jerusalem and open what was to become the Einstein Institute of Mathematics (Katz 2004). This was an extraordinary step for a scholar of Landau's eminence in the period under discussion and

[18]The actual launching of regular instruction at the Hebrew University, i.e., within the framework of a 'faculty', began only in 1928–29. This was a compromise with the initial intention to embark on teaching only after each of the institutes and departments was headed by a world-class scholar, that is, one who had gained the rank of full professor at a prominent Western university. The demand for systematic instruction at Mount Scopus, the Hebrew University's first campus, came first and foremost from the local lay public whose spokespersons argued for the need to provide a framework of academic studies for the young people in the Jewish community in Palestine (the *yishuv*) at that time. Their demand was also backed by some of the university's researchers, eager for an audience from which future research students could be recruited. Further support came from some Board members who, from the outset, had been uneasy with the idea of forming 'elitist' research institutes (Heyd 1999).

[19]The inclusion of the Institute of Mathematics in the Faculty of Humanities was partly based on the contention that pure mathematics, as Magnes argued, is "preeminently one of the *Geisteswissenschaften*". Magnes to Landau, March 8, 1927. HUA, Einstein Institute of Mathematics (EIM) Files, File 16[3].

[20]A full—mathematically oriented—biography of Landau has yet to be written. Until then, the reader may consult the nine volumes of his Collected Works (Bateman et al. 1985–87). The planned tenth volume is to include biographical documents, photographs and facsimiles, etc. See http://blms.oxfordjournals.org/content/21/4/342.full.pdf. Landau participated in the university's opening ceremony in 1925 and was among those who lectured on that occasion. The title of Landau's paper, delivered in Hebrew, was "Solved and Unsolved Problems in the Elementary Theory of Numbers"—see Landau (1925). See also Corry and Schappacher (2010). Landau also contributed to the volume: *Scripta Universitatis Atque Bibliothecae Hierosolymitanarum, Mathematica et Physica*, Volumen I (Hierosolymis: MCMXXIII). See Landau (1923).

well beyond it. Around the same time, a donor was found who made his contribution contingent upon the establishment on Mount Scopus of an institute bearing Einstein's name, in a building named after the donor. No special institute of mathematics had been mentioned in the Preparations Report, but when it became clear that Landau really was willing to come to Jerusalem, *Kanzler* Magnes gave up the idea of using the donor's funds for other purposes, and preparations were begun for Landau's arrival. These included the acquisition of a suitable mathematics library, consisting largely of the famous private collection of Felix Klein of the University of Göttingen which the Hebrew University purchased at Landau's suggestion after Klein's death in 1925.

The organizational model that Landau proposed to establish in Jerusalem was that of a single-disciplinary institute. A memorandum submitted to Magnes specified:

"Professor Landau has suggested fixing the initial number of staff members in the following fashion:

- One professor for pure mathematics;
- one docent who will lecture according to the general plan;
- a librarian, to overlook the collection of models (the docent and the librarian are also to serve as scientific assistants to the professor)".

The explicit provision that the *Dozent* and the librarian were to be the professor's assistants makes it clear that all the institute's staff members were to be entirely subordinate to its director and devoid of any research autonomy.[21]

Landau apparently considered remaining in Jerusalem for several years, but ultimately he taught only for the winter semester of the 1927–28 academic year, after which he returned to Göttingen. However, he did assume the task of finding a suitable candidate to fill the position of director of the Einstein Institute of Mathematics. Because of some ambiguities that emerged from the negotiations with the candidates, not one but two distinguished mathematicians came to Jerusalem: Dr. (later Professor) Mihály Fekete of Budapest (1886–1957) and Professor Adolf Abraham Fraenkel of Kiel (1891–1965).

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[21] Memorandum on the Institute of Mathematics submitted on Landau's behalf to Magnes, November 25, 1925, Hebrew University archive, EIM file (Hebrew). A 1930 document on the organization of the university's academic institutions (institutes, faculties, and Council) stated: "The purposes of the institute fall into two parts: instruction and research. [...] With regard to research, staff members of an institute (or department) that has a director are not authorized to determine the institute's program of research. [...] Note: However, the director has a duty to develop the research potential of the staff members, and if there are staff members who are willing and able to undertake independent research according to their own proclivities, the director must facilitate such aspirations within the technical and financial limitations of the institute, on the condition that the comprehensive research program of the institute will not suffer as a result." See University Archive, Council File, Protocols of the Council 1929–1932 (Hebrew, translated by present author). The Hebrew University did not adopt the rank of the (untenured) *Dozent*, introducing instead the position of the (tenured) Lecturer. Similarly, it eschewed the German "Dr. Phil." degree, preferring the "Ph.D." for its future research students (the first of whom earned his doctoral degree in the second half of the 1930s).

With the arrival of Fekete and Fraenkel in Jerusalem, a change took place in the institute's organizational model. There were now two directors, and the type of control the professors exercised over the junior members' time and research topics was radically transformed. Everyone working in the institute, including students, was granted research autonomy. No explanation was given for the introduction of this change in the organizational style, which ran counter both to the model characteristic of research institutes in German universities and to Landau's conception of the role of the institute's director; and there is no evidence that it might have stemmed from responsiveness to the egalitarian ethos then current in the *yishuv* (the Hebrew term used for the Jewish community in Palestine at the time).

The change in the organizational model did not involve any adjustments in the institute's mathematical contents. The research agenda set out by Fraenkel and Fekete paralleled Landau's, focusing solely on 'science for its own sake', on pure mathematics (*reine Mathematik*) as it had been defined at the University of Berlin in the middle and the second half of the nineteenth century. Following a typical neo-humanist orientation, the mathematical leadership of the University of Berlin (particularly Edward Ernst Kummer, Karl Weierstraß, and Leopold Kronecker) created a mathematical tradition centered on two mainstream fields of research in pure mathematics: analysis and number theory. They reworked much of the foundations and advanced techniques of these two vast, fundamental fields, leaving aside any appeal to geometric intuition as a possible guiding principle. They showed no interest in exploring how mathematical tools could be applied to the sciences, or in solving problems suggested by physics or other fields.[22]

Landau had completed his dissertation in Berlin in 1899, had obtained the *venia legendi* two years later, and had begun teaching as *Privatdozent* with special status of professor before being appointed in 1909 as a (tenured, *ordentlicher*) professor in Göttingen, then the world's leading center for mathematics and the exact sciences. Steadfast in his devotion to the Berlin neo-humanist mathematical tradition, he rejected that part of the Göttingen tradition that encouraged the interaction and reciprocal influence of pure mathematics with applied mathematics and the natural sciences, and continued to develop the research agenda of his illustrious mentors.

Fraenkel had studied at Marburg with Kurt Hensel, a follower of both Karl Weierstraßand Leopold Kronecker, and himself a typical representative of the classic Berlin tradition of pure mathematics. Fekete obtained his doctoral degree at the University of Budapest under Leopold (Lipót) Fejér, who had studied with Herman Amadeus Schwartz, Weierstraß's successor at Berlin. Thus, both Fraenkel's and Fekete's mathematical roots converge with Landau's, leading to the emergence in Jerusalem of what was clearly a continuation of the scientific ideals and research agenda characteristic of the tradition of pure mathematics that reigned at the University of Berlin.[23]

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[22]The classic, most detailed study of mathematics in Berlin appears in Biermann (1988). For a more recent, concise account, see Begehr (1998), Pyenson (1979), and Pyenson (1983).

[23]The field of mathematics became increasingly differentiated and professionalized toward the end of the nineteenth century, and pure mathematics came to prominence as an independent area

Hence, first Landau and then Fraenkel and Fekete succeeded in establishing in Jerusalem a distinctive institution even from the 'German' point of view. At the same time, the Einstein Institute of Mathematics displayed not a whit of responsiveness to the Hebrew University's 'Jewish-ness'—as might have come to expression, for example, in the exploration of the 'Mathematics of the Jews' or the history of the Jewish calendar.[24] Nor were its activities influenced by the particular circumstances of Palestine, which might have indicated the desirability of introducing courses on applied mathematics and statistics, deemed necessary for students whose later professions would take them outside the university, or who would go on to become researchers within the university itself.

Fraenkel, whose field of mathematical research was relevant to philosophy, continued to publish in such journals as *Blätter für deutsche Philosophie, Erkenntnis*, and *Comptes rendus de l'Académie des Sciences*, while Fekete published in *Mathematische Zeitschrift, Mathematische Annalen*, and *Comptes rendus de l'Académie des Sciences*. After his arrival in Jerusalem, Fekete stopped publishing in Hungarian, and after 1933 both Fraenkel and Fekete—like many other Jerusalem academics—moved from German-language to English-language academic publication venues (Katz 1978). The choice of these and similar journals, and the subjects of the articles published in them, attest to the fact that the senior researchers in the Einstein Institute of Mathematics, and the junior researchers who joined them in the initial

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of research devoid of any connections with practical matters, founded upon staunchly elitist internal norms of research quality. At the beginning of the twentieth century, the 'seminary' of pure mathematics at the University of Berlin was recognized as a distinct academic unit, with three full professors—Schmidt, Schur, and Bieberbach—working solely in pure mathematics. During the same period, the Institute of Mathematics at Göttingen had three full professors—Hilbert, Courant, and Landau—while the Mathematical-Physical Seminary was directed by the professors of the Institute of Mathematics, together with eight other professors specializing in other areas of mathematics or in closely related fields. Göttingen also had an Institute of Applied Mathematics and Mechanics. Although two of the three institutes were thus devoted to (relatively) practical areas of mathematics, the Institute of Mathematics, too, was founded upon the concept of a relationship between pure mathematics and extra-mathematical concerns: It was established on the basis of Klein's plan for the advancement of mathematical sciences and their application to various realms of science and engineering, a plan supported by Courant, who acted as the institute's head during the period under discussion. In other words, the Institute of Mathematics at Göttingen was not devoted solely to 'pure' mathematics. Richard Courant's attitudes are exemplified by his role as editor, with Wilhelm Blaschke, Max Born, and Carl Runge, of the series *Die Grundlehren der mathematischen Wissenschaften in Einzeldarstellungen: Mit besonderer Berücksichtigung der Anwendungsgebiete* (Springer). For the contemporary mathematical composition of various universities' corresponding faculties see *Minerva—Jahrbuch der gelehrten Welt* (1923) and 1928; Neugebauer (1927); see also: Pyenson (1979).

[24]Fraenkel's very first mathematical study, undertaken in his youth, was on the Jewish and Muslim calendars; see his autobiography (Fraenkel 1967, 76–77). In the 1930 summer session, Profs. Fraenkel and [Shmuel] Klein taught a seminar on the Jewish calendar (see the Hebrew University Yearbook for 1929–30, p. 103), which was billed as being "For all Students [of the University]"—that is, it was not considered a mathematical subject *per se*. Fraenkel did not present this as a research topic for others, but he did continue to work and lecture on it from time to time for many years. See Fraenkel (1947).

years, continued to pursue a research agenda and programs which they had been engaged in before coming to Jerusalem, which had originated in the German universities.

Hence, the German-ness of this Jerusalem institute, insofar as its conception and research focus was concerned, continued unabated; but its organization differed.[25] It adhered fully to the mathematical research ideals developed at the University of Berlin in the second half of the nineteenth century, echoing the neo-humanistic views on which that university based itself in the first half of that century and which were disseminated from there to the German cultural milieu and beyond. The researchers at the Einstein Institute of Mathematics focused their studies solely on questions derived from problems internal to mathematics, that is, on 'universal' materials. This situation persisted for decades, even after Fraenkel and Fekete had retired from the institute and departed this world. The institute maintained its original German tradition, restricting itself to theoretical mathematics and systematically rejecting any forays into applied mathematics (Katz 2004).

The School of Oriental Studies

The School of Oriental Studies (the Oriental Institute, *Das orientalische Institut*, המכון למדעי המזרח) of the Hebrew University was established by Josef Horovitz (1874–1931), Professor of Semitic Philology at the University of Frankfurt, who inaugurated it with a series of lectures in March 1926. After returning to the University of Frankfurt, Horovitz acted as Visiting Director of the institute in Jerusalem until his death in 1931.

Horovitz's initiative in establishing a specialized institute for oriental (that is, Near Eastern) studies appears similar to Landau's in establishing one that specialized in pure mathematics. In 1925 and early 1926, the Hebrew University's Arabic Department operated within the framework of the Institute of Jewish Studies, as had been suggested in the 1920 Preparations Report; that is, it had the status of an ancillary field to Jewish studies. However, Horovitz, who was a member of the university's Board of Governors and also of its Academic Council, convinced his colleagues that it would be worthwhile to establish an independent organizational framework for oriental studies.[26] Together with the scholarly reasons given for his

[25] During the period discussed here and beyond, no publication by Fraenkel and only one publication by Fekete was written with students.

[26] "After the presentation of Prof. Horovitz's detailed proposal, a decision was made to open a School of Oriental Studies. Prof. Horovitz was asked to undertake the temporary directorship of this school; further questions regarding the budget and arrangements were referred to Prof. Horovitz and the administration in Palestine" (Hebrew University Archives, "Protocol of the Second Meeting", September 1926, *Decisions of the Board of Governors*, p. 19). At the same meeting, it was decided that Professor Benno Landsberger of Leipzig would be appointed Professor of the Hebrew University if he agreed to accept the position (which he did not). Horovitz, like Landau, participated in the university's opening ceremony in 1925 and was among those who lectured on

broad-ranging plan were some profound political ones, concerned with the need to convince the Muslims and the Arabs of the positive purposes of Zionism.[27]

It was certainly under Horovitz's influence that Magnes, in one of his public addresses, cited both reasons for establishing the institute. Regarding the universalist mission of the Hebrew University, he declared: "It is incumbent upon us as Jews to fulfill the role of mediators between East and West". He went on to explain: "This Institute of Islam is but an indication as to what we mean when we say that the problem of religion can be taken up in a comparative and fundamental way in Jerusalem perhaps better than in most places".[28]

The plan was to create a multi-disciplinary institute that would eventually include Assyrian-Babylonian and Egyptian-Coptic sections.[29] At the beginning, however, the Institute ('School') of Oriental Studies comprised only the Arabic section. The envisioned composition of the institute clearly attests to its intention to embrace the model of oriental studies current in the German universities; that is, its goal was to shed new light upon texts belonging to the 'high culture' of the ancient 'oriental' and 'Near Eastern' civilizations by means of historical-philological research, mirroring the concern of European classical studies with what were considered the finest texts produced by Western cultures. Most of the staff members of the institute were specialists in classical oriental studies, Arabic literature and history, and Islamic philosophy. They had a well-endowed library at their disposal—a university requirement for establishing a research institute—the 6,000-volume collection of the renowned Jewish Orientalist Ignaz Goldziher, which was purchased for the Hebrew University after his death (Kiryat Sefer 1924, 5).

Like Landau, Horovitz inaugurated the institute under his direction with a ceremonial lecture series. However, Horovitz either had no intention of settling in

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that occasion. He was also among those who contributed to the volume: *Scripta Universitatis Atque Bibliothecae Hierosolymitanarum, Orientalia et Judaica*, Volumen I (Hierosolymis: MCMXXIII) [Papers of the University and Library in Jerusalem, in the volume on "Orientalism and Judaism"]. The title of Horovitz's paper was *Das Koranische Paradies*. See above, footnote 20 on p. 113.

[27] Horovitz was a full professor at the University of Frankfurt, where he also directed the *Orientalische Seminar*. His appointment at that university was in the field of "Semitic philology with consideration of the literature of the Targum and the Talmud". Horovitz is mentioned in Fück (1955, 313–14). For more on Horovitz, his relations with Magnes, and his reasons for wanting a special institute of oriental studies, see Lazarus-Yafeh (1999), Milson (1996).

Among Horovitz's reasons for setting up a specialized institute of oriental studies, separate from the Institute of Jewish Studies, Milson notes that "the attitude of the Islamic-Arabic world toward Zionism" had not yet been determined, and an Institute of Oriental Studies at the Hebrew University might contribute to swaying that world towards Zionism. See Kedar (1967, 25, 30).

[28] "Addresses by the Chancellor", October 12, 1927, p. 40. Other heads of the university shared the view that one of the university's roles was "to promote an understanding between the Jewish community of Palestine and the Near Eastern countries" (Hebrew University, 1942, p. 31), Hebrew University Yearbook for 1925–26, p. 24. In several other publications by the Hebrew University these 'sections' are referred to as 'departments'.

[29] Hebrew University Yearbook for 1925–26, p. 24. In several other publications by the Hebrew University these 'sections' are referred to as 'departments'.

Jerusalem after the inauguration, or he postponed doing so until an unspecified future date. He returned to Frankfurt and directed the institute by long distance, through intensive correspondence.[30]

Like the Institute of Mathematics according to Landau's original plan, organizationally the members of the School of Oriental Studies, including lecturers (one or two rankings below professors), were subordinate to the director, who determined their research program from his abode in Frankfurt. These subordinates were required to take part in two major undertakings, designated as "collective research" projects[31]: The first, the preparation of a Concordance to Classical Arabic Poetry devoted to the works of the early Arab poets (up to the close of the Umayyad Period; the second, a critical edition of the most important work written by medieval Moslem historian al-Balādhurī, *Ansab al-Ashraf* (Genealogy of the Nobles).[32]

Magnes took pride in this research program and justified it in the following words:

> "The pieces of research just mentioned have nothing directly to do with Judaism, and they were chosen purposely in order to show that the Hebrew University was concerned with the study of Islam and of Moslem peoples and their literature on their own account. It is one of the fondest hopes of the Hebrew University that it may serve as a great center of Arabic learning".[33]

In Jerusalem as in Europe, these texts were considered 'universal' research materials; to study them was to engage in scholarship for its own sake.

Horovitz kept three or four junior researchers under his supervision busy with each of these projects. The publication lists of the junior staff members of the School of Oriental Studies in this period indicate that they had little time left for their own research.[34] However, in 1931, the year Horovitz died, several of them turned their attention to Jewish materials.[35] D.Z. Baneth continued the work on Judaeo-Arabic

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[30]Hebrew University Yearbook 1926–27, p. 23.

[31]*Ibid.*

[32]Hebrew University Yearbook 1942, pp. 31–32. Following Fück (1955), Hava Lazarus-Yafeh (1999) notes that the Baladhuri project was originally undertaken by the Orientalist Prof. C.H. Becker, but when he decided to enter political life, he suggested that it be carried on by Horovitz and his team of researchers in Jerusalem. The Concordance Project was intended to facilitate the research for Horovitz's projected book on early Arabic poetry. The Jerusalem team, adds Lazarus-Yafeh (again following Fück (1955)), was engaged in the most technical type of work on the Concordance, the collection of "scraps".

[33]"Addresses by the Chancellor", October 31, 1928, p. 71. Magnes went on to remark that the research projects Horovitz brought to the Hebrew University had "attracted the attention of the learned world, as is indicated by a commendatory resolution adopted at the recent International Congress of Orientalists at Oxford", *ibid.*

[34]The list of publications for the staff members of the School of Oriental Studies that appears in the 1926–27 yearbook mentions only two works by L.A. Mayer (p. 38). The list for 1928–29–1929–30 mentions two more publications by L.A. Mayer and one by Y.Y. Rivlin. Rivlin's book was a reworking of his doctoral dissertation, which was accepted by the University of Frankfurt in 1929.

[35]The work of D.Z. Baneth over the course of many years in the area of Judaeo-Arabic studies is a good example of an Orientalist's redirection towards Jewish materials. Baneth began working in

literature that he had begun before his arrival in Jerusalem, and S.D. Goitein's early studies on "The Spoken Language of the Yemenite Jews" began to be published that year (Goitein 1934). This scholarly direction, which was based partly on fieldwork among Yemenite Jews who had emigrated to Jerusalem at the time, serves as an example of a redirection towards research on local materials.

The only staff member of the School of Oriental Studies who responded consistently to the research opportunities presented by the local research resources was L.A. Mayer. He had been doing so even before joining the institute, in the context of his work in the Antiquities Department of the Mandate Government, thanks to which he was not dependent upon the institute for his livelihood. Mayer's concern with 'local materials' drew him to decipher Arabic inscriptions found in Palestine; and as time went on he became active as an archaeologist as well, in the framework of the 'third wall' excavations.[36]

Clearly, the School of Oriental Studies under Horovitz was German to the core, both organizationally and in terms of the content of its research programs. Its organizational structure replicated, in perhaps an even more extreme form, the authoritarian configuration of the German university research institutes.[37] From this point of view the German-ness of the School of Oriental Studies exceeded even that of the Institute of Mathematics, once the latter came under the direction of Fraenkel and Fekete. As demonstrated above, however, these research ideals, agenda, and programs reflected the academic outlook of the institute's first director. When several of its junior members began taking their own independent scholarly directions,

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this field while still in Berlin, in the framework of the Academy of Jewish Studies (*Akademie für die Wissenschaft des Judentums*, see below, p. 131) and in the context of his work on the text of the Kuzari by Judah Halevi, which is written in Judaeo-Arabic. From 1925 to 1931, he published nothing at all in Jerusalem, and from then on all his publications rested on Jewish materials, as can be seen from the stenciled list of "Scholarly Works Published by Academic Staff Members of the Hebrew University from Its Opening up to the End of the 1936–37 Academic Year" (May 1938; henceforth "Scholarly Works"), pp. 55–66. For Goitein's early studies, see *ibid.*, p. 65; and see the articles by Lazarus-Yafeh (1999) and Milson (1996).

[36]"Scholarly Works", pp. 95–98. Mayer specialized in archaeology and in Arabic and Islamic art. Before and after joining the staff of the university, he worked as a supervisor in the Antiquities Department of the Mandate Government (1921–29) and as a librarian and archivist in the Museum of Archaeology (see "The Hebrew University of Jerusalem: Its History and Development", Jerusalem 1948 (3rd edition), p. 178).

[37]Most of the German universities of the period had an institute or seminary of oriental studies (see *Minerva Jahrbuch 1923*). The extent to which they were directed according to the authoritarian model, with no research autonomy for the director's subordinates, is worthy of investigation in its own right. However this may be, Fück's depiction of Horovitz's subordinates in Jerusalem (see above, footnote 27 on p. 118) indicates that Horovitz's authoritarianism went even beyond what was accepted in Germany. Cf. the criticism of Kligler by the Survey Committee (see Hartog 1934). As Horovitz was surely aware, there was also another approach in the German academic world to cultivating the culture and languages of the peoples of the Near East. The *Seminar für orientalische Sprachen* at the University of Berlin, an academic institution with an avowedly utilitarian ideology, was directed by the renowned Orientalist Eduard Sachau, who had been Horovitz's dissertation advisor. See: Fischer (1888) and Sachau (1912).

they showed a strong inclination to apply the practices of oriental studies to Jewish materials, and to respond to the local research opportunities.

The Institute of Microbiology: The Department of Parasitology and the Department of Hygiene and Bacteriology

A Faculty of Medicine, to be associated with the Hadassah Medical Organization and its hospitals and laboratories in Palestine (Shvarts and Shehory-Rubin 2012), held a central place in the Preparations Report. The Jewish National and University Library (now the Israel National Library) already had a Medical Department, with special collections relevant for Palestine medicine and health as well as subscriptions to some 330 medical periodicals.

The Institute of Microbiology was expected to form the nucleus of the Institute for Medical Research and, eventually, the Faculty of Medicine. The Department of Parasitology, which was to be a division of the future institute, was founded already in 1924 when Saul Adler (1895–1966) expressed his willingness to join. With the establishment of other departments—bacteriology and immunology—Adler was temporarily placed in charge of the immediate program. Alexandre Besredka, a renowned immunologist and central figure at the Pasteur Institute in Paris, and who was a member both of the Hebrew University's Academic Council and of its Board of Governors, was expected to come to Jerusalem to direct the Institute of Microbiology on Mount Scopus. However, this expectation never materialized.[38]

Upon qualifying in medicine at the University of Leeds in 1917,[39] Adler was commissioned as an officer in the British Royal Army Medical Corps and served in Mesopotamia until 1920 (Gavron 1997, 34–35; Shortt and Adler 1967; Ashbel 1989). This was probably his first encounter with the medical issues of the Middle East and its typical diseases, including the 'Oriental Sore'. In 1920 Adler earned a diploma at the Liverpool School of Tropical Medicine (DTM) and subsequently joined its staff as a clinical assistant. In this capacity he was sent to Freetown, Sierra Leone, in 1921, to work at the Alfred Lewis Jones Laboratory,[40] he worked on a variety of subjects, such as helminthic infections of animals, malaria parasites in chimpanzees and lizards, and a coccidian parasite of civet cats. One of his investigations dealt with the possibility of inoculating humans with the malaria parasites

[38]Even earlier, Weizmann had anticipated that Professor Ascoli of Milan would be willing to assume direction of the Institute of Microbiology; see: Weizmann to Ascoli, October 29, 1923, and Weizmann to Ratnoff, October 30, 1923, in Wasserstein (1968) (henceforth the Weizmann Letters, Vol. 10).

[39]University of Leeds, Bachelor of Medicine (MB), Bachelor of Surgery (ChB), and Certificate of Vaccination.

[40]For a contemporary view on this medical area, see Scott (1942). For an historical perspective, particularly regarding the Liverpool School of Tropical Medicine, see Power (1998); a central chapter in Power's book is devoted to the Alfred Lewis Jones Laboratory (pp. 47–77), and Saul Adler is mentioned as well.

of chimpanzees. Indeed, it was Adler's first experience with self-inoculation (which in this case proved non-effective) but not the last.

In Palestine in the 1920s, malaria and trachoma were considered the country's most acute medical problems.[41] Malaria was already within the 'research territory' of Israel Kligler (see p. 123f below), and trachoma was being dealt with by local ophthalmologists; Adler therefore turned his attention to leishmaniasis, popularly known as 'Oriental Sore' or 'Rose of Jericho', still common today in Israel in the desert areas around the Dead Sea and the Negev. Although the leishmania parasite was well known throughout Africa and Asia, there was great uncertainty about its vector; even lizards were suspected of playing a role in its life cycle. Adler and his assistant, entomologist Oskar Theodor (1898–1987), tried to substantiate the connection between *phlebotomus*, the sandfly of Palestine, and local cutaneous leishmaniasis in humans. Having established the infection of *phlebotomus* by leishmania in its flagellate stage, they carried out successful transmission experiments of leishmania tropica to human beings by inoculation of naturally and artificially infected sandflies. Wary of the accepted entomological classification of sandflies, which relied on their external characteristics, they turned to an innovative systematic classification based on differences in their internal organs. This led to the identification of the precise strain of sandfly responsible for the transmission of cutaneous leishmaniasis.

This project exemplifies Adler's approach to medical research. In the British tradition of tropical medicine, it was characterized by the integration of epidemiological and biological investigations carried out in the parasite, the vector, and the human environments, and supported by extensive observations and laboratory experimentation. For his second project, Adler used a similar approach in experimenting with bovine piroplasmosis, transmitted by ticks, which was seriously harming local dairy farming.

In the two or three years of their research on leishmaniasis, Adler and Theodor published several articles in British journals devoted to tropical medicine, including *Annals of Tropical Medicine and Parasitology* and *Annals of the Royal Society of Tropical Medicine and Hygiene*. While these publications were read by a relatively restricted community of specialists in tropical medicine,[42] the publication of four "preliminary notes" in *Nature* in 1926 and 1927 may be taken as evidence that the discoveries made by Adler and Theodor were considered important to science in general. Their short communication "The Experimental Transmission of Cutaneous Leishmaniasis to Man from *Phlebotomus papatasii*" (Adler and Theodor 1925) was probably the first contribution to *Nature* from the Hebrew University of Jerusalem.

[41] At the end of the nineteenth and beginning of the twentieth centuries, more than half the population of Palestine was infected by malaria (Mühlens 1913; Kligler 1930) and even more were victims of trachoma, especially in urban neighborhoods (Anonymous 1915).

[42] Adler was no doubt aware of the international implications of his scientific work. In a January 1926 memorandum to Magnes he remarked, "if certain results will be achieved due to [our] experiments, then the problem [leishmania], which occupies scientists from India, North Africa, and China will find a kind of solution" (Hebrew University Administration files).

Adler became Associate Professor at the Hebrew University in 1928 and was promoted to the status of (full) Professor of Parasitology about a year later. He went on to become the most respected and influential biologist and scientist at the Hebrew University (Katzir 1968; Sheba 1968; Gavron 1997).[43] In the 1930s his studies of cattle diseases gained him the esteem of the Jewish agricultural community. In the same period he achieved international recognition and was awarded the Chalmers Gold Medal of the Royal Society of Tropical Medicine and Hygiene and a Laveran Silver Medal by the *Société de pathologie exotique* in Paris. Many years later, in 1957, he became the first Israeli scientist to be elected a Fellow of the Royal Society of London.

Neither Adler's intellectual origin nor his intellectual attitude was 'German'. His research agenda did not derive from any universalistic, non-local problematic; on the contrary, as a medical doctor and a fervent Zionist, he saw himself as committed, first of all, to overcoming local health problems, whether in humans or in livestock. The research program took its direction from local materials—that is, local diseases. Adler's preferred biological approach to etiological problems probably emerged both from his personal intellectual curiosity and from the British tradition and practice of tropical medicine (compare Balfour 1912). By giving his 'local' materials and problems priority while treating them as 'universal' materials as well, Adler developed a fruitful combination between what we would today call applied and basic research.[44]

By 1926, several medical research projects being carried out in clinics and hospitals affiliated with the Hadassah Medical Organization had been transferred to the new Hygiene (later Hygiene and Bacteriology) Department at the Hebrew University, under the direction of Israel J. Kligler (1889–1944). Kligler earned his B.S. from the College of the City of New York and, between 1911 and 1915, while working as a scientific assistant in the Department of Public Health at the American Museum of Natural History, he presented his M.A. thesis and doctoral dissertation in bacteriology and pathology at Columbia University, receiving his Ph.D. in 1915 for his work on dental caries.[45] During this period he published several scientific papers, especially in clinical bacteriology. For the next five years he was with the

[43] Because of his personal experience with self-inoculation, Adler is described as being one of those rare "true scholars [...] whose entire lives are devoted, truly devoted, to their work. If need be, they would no doubt take risks in order to achieve their end—their sole end being true scholarship" (Agnon 1989, 553–554).

[44] Like others of his generation in Jewish Palestine, Adler saw himself as an engaged scientist. As we shall see, the same may be said of his colleagues at the university's Institute of Palestine Natural History. However, the latter in a sense followed a reverse process, with many of its researchers progressing from general biological or geological questions to local ones; see publication lists in Hebrew University Yearbook 1925–26, pp. 29–31; and Hebrew University Yearbook, 1926–27, p. 33.

[45] Kligler's research biography is reconstructed here on the basis of consecutive Hebrew University Yearbooks (1925–1942); the obituary written by Kligler's colleague Leo Olitzki (1944); and the Survey Committee's Report Hartog (1934). Concerning this report, see Parzen (1974).

Rockefeller Institute for Medical Research (RIMR), where he continued his bacteriological and epidemiological research; an indication of the quality of his work there may be seen in the eponyms *Micrococcus kligleri* and “Kligler medium” or “Kligler Iron Agar”. During this period Kligler also became involved in research on problems of sanitation and hygiene, and in 1920–21 he was a member of the Yellow Fever Commission sent to Mexico and Peru. At the time it was still unclear whether yellow fever was caused by microbes or by viruses, but its vector, like that of malaria, was known to be the mosquito. Arriving in Palestine in 1921, Kligler’s first medical post was that of Chief Bacteriologist for the Hadassah Medical Organization. It was probably his prior experience in the large-scale endeavor to eradicate a disease carried by mosquitoes that made Kligler the right person, in 1923, to head the Malaria Research Unit in Palestine. Kligler maintained his ties with RIMR; in 1926 he took part in the RIMR Yellow Fever Commission in West Africa, and the autumn of 1928 saw him working in the RIMR laboratories on the mode of transmission of fowl pox by mosquitoes—an epidemiological project he was promoting at the Hebrew University. In 1926 Kligler was among the first six scholars nominated as full professors by the new university.

Kligler devoted his department to general and experimental epidemiology. Following the example of the RIMR Yellow Fever Commission, Kligler combined studies of the incidence of vectors of the disease under investigation in certain populations, like Tel Aviv and Tiberias, with laboratory studies. An example is his pioneering research confirming that the dysentery common to Palestine is caused by the dysentery bacterium and not, as had been thought, by an amœba. Further investigations were undertaken to develop a vaccine against bacterial dysentery.

Nutrition and climate studies were another of Kligler’s central research interests, evinced in a systematic study of the nutritional value of vegetables and cereals grown in the country. Along with growth studies on rats, complete chemical analyses of each food compound were made. The object of these studies was to obtain the basic data needed to propose a rational diet for the inhabitants of the country, suited to its peculiar climatic conditions.

Still, Kligler’s main interest for many years remained the eradication of malaria in Palestine. He established and directed his department’s Malaria Research Station at Rosh Pina and Lake Huleh, where, among other questions, the migration and behavior of infected mosquitoes were studied. He, and especially his assistant (later Professor) Gideon Mer (1894–1961), demonstrated the ability of the Zionist enterprise (which emphasized amelioration projects, such as drainage efforts and the provision of clean water supplies) to improve health for both Jews and Arabs by eradicating malaria from the most infected area of Palestine in the 1930s. When the League of Nations’ Malaria Commission selected malaria as one of three problems for international investigation in the mid-1920s, Kligler became one of its members.[46]

[46] Kligler’s description of his anti-malaria activities at the time (Kligler 1930) are placed in the wider retrospective context of the history of the Zionist settlement enterprise and of medical humanities in Sandra M. Sufian (2007).

During the period under discussion, Kligler and his assistants published in international journals devoted to parasitology and tropical medicine, hygiene, experimental biology, and medicine. An exception was "Observation on the Physical and Biological Characteristics of *Leptospira*" by Kligler and Aschner Kligler and Ashner (1928), published in the disciplinary *Journal of Bacteriology*. The 'pure research' nature of this contribution can certainly be attributed to its second author, who was overtly oriented towards biology 'for its own sake' rather than epidemiological research.

Kligler's organizational policy is evident from his place as senior author on most of the scientific articles published in his department. Even when the research was carried out by his assistants, he insisted that he bore responsibility, as director of the department, for the conception and quality of every research project carried out. Hence, though he was not a product of the German research system, his managerial style was typical of a German professor heading a single-disciplinary institute. Indeed, he was severely criticized for his authoritarian behavior, both within his department and by external observers, some of whom remarked that such behavior was intolerable by British and American standards.[47] If Kligler may be considered organizationally 'German', he was entirely non-German in terms of his epistemological ideals and scientific attitudes. In contrast, Adler gave his assistants and associates full research autonomy, made them equal participants in joint studies, and allowed them to publish their own research under their own names only.[48] And in accord with their respective research traditions, Adler and Kligler turned first to local materials, while treating them more like universal ones.

[47]Of the 18 publications of the Department of Hygiene, nine were published by the head of the department under his sole name, while seven more were published by him jointly with one of the department's four staff members, and only two were published by staff members under their own names only; see Hebrew University Yearbooks 1926–27 and 1928–29, pp. 74–76. In 1934, Kligler was heavily criticized for his managerial style, mainly because of what was seen as an exploitation of his subordinates; see Hartog (1934). A few months after the presentation of the Survey Committee's Report to the Board of Governors, Magnes presented his "Reply to the Report of the Survey Committee of the Hebrew University of Jerusalem" (July 1934), in which he transmitted Kligler's objections to the committee's evaluation and conclusions (*ibid.*, pp. 87–109). Professor Alex Keynan (1921–2012), who studied under both Kligler and Adler, believed that the difference between their respective organizational styles could be attributed, first and foremost, to their different personalities: Kligler was a "scientific organizer", while both Adler and Aschner were "gentleman scholars", interested mainly in the study and comprehension of nature (personal interview, March 13, 1997. The transcription of the recorded interview is held by the secretariat of the Israel Academy of Sciences and Humanities).

[48]Of the 14 publications of the Department of Parasitology mentioned in the Yearbook for 1927–28 and 1928–29, only two appear under Adler's sole name; six—that is, nearly half of them—were published jointly by him and Theodor; two list Theodor as the sole author; and four list Wittenberg as the sole author; see Hebrew University Yearbook 1926–27, pp. 72–74.

The Chemistry Institute

The plan outlined in the Preparations Report included establishment of an Institute of Chemistry alongside those of physics and microbiology. Beginning in 1921, Weizmann started to correspond with the biochemist Andor Fodor (1884–1968) of the University of Halle, who indicated his willingness to come to Jerusalem and help establish an Institute of Chemistry. This led to a formal invitation from the WZO to assume this task himself[49]; this fit well with Weizmann's personal plans to settle in Jerusalem and continue his scientific work there. "It will be nice", Weizmann wrote to Fodor, "to sit on Mount Scopus discussing ferments", at the same time informing Fodor that he had already sent on some important laboratory equipment and a collection of books, including organic and biological chemistry texts.[50] In the institute's embryonic stages, the possibility of a combined Institute of Chemistry and Physics had been discussed,[51] but Fodor seems to have objected to such institutional arrangements and that plan was abandoned.

Andor Fodor had studied inorganic chemistry at the *Eidgenössische Technische Hochschule* in Zürich (Diploma 1907) and pursued his doctorate at the University of Zürich (Dr. phil. 1910). In 1911 he became assistant at the Physiological Institute of the University of Halle, where he earned his *venia legendi* and became *Privatdozent* in physiological chemistry in 1922 and *Ausserordentlicher Professor* in 1923.[52] That year Fodor was among those honored with a prize from the

[49]Fodor was at the University of Halle (which in 1933 became the Martin Luther University Halle-Wittenberg) when he first wrote to Weizmann, in October 1921, mentioning a memorandum concerning the establishment of research institutes at the Hebrew University, then in formation. Weizmann, who was familiar with Fodor's scientific work, responded by inviting him to participate in the founding efforts of such institutes; see Weizmann to Fodor, October 29, 1921, in the Weizmann Letters, Vol. 10, letter 236. Fodor accepted the offer, and two years later he arrived in Jerusalem to supervise the conversion of the already existing Gray Hill mansion on Mount Scopus into a home for the research institutes. The Institute of Chemistry began operation "with great enthusiasm" in the summer of 1924; see Chaim Weizmann (from Jerusalem) to Vera Weizmann, September 27, 1924 (Freundlich (1977), henceforth the Weizmann Letters, Vol. 12), letter 184.

[50]The Weizmann Letters, Vol. 10, letters 273 (October 28, 1921) and 303 (December 7, 1921). From the outset of his correspondence with Fodor and others concerning the Chemistry (or Chemistry and Physics) Institute, it is clear that Weizmann intended to come to Jerusalem himself, to take part in the construction of the laboratories, to take an active scientific role in the Hebrew University, and to link the formation of the Chemistry Institute with that of the Institute of Microbiology; see *ibid.*, letter 303 (December 7, 1921). Neither plan was realized; see also Chaim Weizmann (1949) and Jehuda Reinharz (1985). The enthusiasm for Fodor held by Weizmann and others seems to have cooled over time, judging by the criticism voiced against him at the third meeting of the Board of Governors in 1926; see "Decisions of the Board of Governors", pp. 25–26. Fodor was among the first tenured professors appointed by the Hebrew University; see *ibid.*, p. 31. Weizmann's vision for combining an institute of chemistry with an institute of microbiology in Palestine/Israel was realized in 1933–34 with the formation of the Daniel Sieff Institute in the town of Rehovot (which he himself headed). Eventually the Daniel Sieff Institute became the Weizmann Institute of Science, opened officially in 1947–49.

[51]Weizmann Letters, Vol. 10, letters 281, 286 and 303.

[52]Andor Fodor File in the Hebrew University Archive.

Kolloid-Gesellschaft for "works in research on ferments".[53] Fodor was the first established European scientist to arrive on Mount Scopus in order to take an active part in setting up the Hebrew University, with the intention of settling permanently in Jerusalem. The university's Institute of Chemistry began to function in the summer of 1924.[54] As previously noted, it had been decided that each of the university's institutes would begin operating once it had a single professor, and that the staff would eventually include two professors. Since Fodor, the first chemist to arrive in Jerusalem, specialized in biochemistry, it was decided that the institute should have two departments at the outset, a "Department of General Chemistry, Synthetic and Analytic", for which Fodor would provisionally take responsibility, and a "Bio-Chemical Department, including Bio-Colloid Chemistry and Bio-Physical Chemistry", which would be headed by Fodor. Thus, at least temporarily, it may be said that the Chemistry Institute was a single-disciplinary research institute; but the university declared that the research in both departments, but particularly in the first one, would encompass:

> "Research work in the whole of chemistry, as far as possible, with due regard to the problems of economic and scientific interest in Palestine. The selection of the research work in general will be left to the Director of the Department".[55]

Furthermore,

> "Apart from purely scientific work the Chemical Institute of the Hebrew University has for one of its activities the object of taking its part of knowledge in developing our knowledge in the country of Palestine" (Fodor 1926).

Fodor saw himself working in the tradition of distinguished German chemist and Nobel laureate Emil Fischer (1852–1919), as interpreted by Emil Abderhalden's biochemical institute at the University of Halle, where Fodor taught and did research before coming to Jerusalem.[56] The Hebrew University awarded him the rank

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[53] *Ibid.* and Beneke (1995, 92).

[54] Chaim Weizmann (from Jerusalem) to Vera Weizmann, September 27, 1924, Weizmann Letters, Vol. 12, letter 184.

[55] The Hebrew University of Jerusalem, *The Inauguration, April 1, 1925*, small format. The brochure's front page is in Hebrew, and it also includes text in Arabic. Cf. the copy of this statement printed in *The Jewish Guardian*, March 27, 1925, p. 18, in which "chemistry" is replaced with "organic chemistry". In the *Hebrew University Yearbook* for 1926–27 (pp. 30–32), the two departments are referred to as the departments of "Analytical and Synthetic Chemistry and Bio-Chemistry (including Colloidal Chemistry)". In time, with experienced chemists whose specialty was inorganic chemistry and physical chemistry joining as faculty members, the organizational entity of the Institute of Chemistry ceased its formal function.

[56] "In the first years of his work there [Jerusalem], the Director set himself the purpose of solving the same problems on which he had been working in Germany [. . .] some of which [. . .] were the heritage of the tradition of Emil Fischer, in which the Director had specialized during his dozen years of working in the Abderhalden Institute in Halle" (from a memorandum written by Fodor in 1935, a photocopy of which is in the present author's possession; see the description of the institute's work in the Yearbook for 1925–26, pp. 24–26). The publications of the institute mentioned were in the realm of 'pure' research, as evidenced by their appearance in journals specializing in what we would now call 'basic research'.

of (full) professor; his book, *Die Grundlagen der Dispersoidchemie*, published in Germany in 1925 (Fodor 1925), was the first scholarly-scientific work in which the author's home institution was referred to as the Hebrew University. From the point of view of the criteria outlined in that publication, his point of departure was the European, or German, scientific agenda to whose progress it was meant to contribute, and it was directed solely at members of the discipline; it had nothing to do with any specifically local concerns.

As evidenced by the investigations cited in the Hebrew University Yearbook for 1927 and 1928 as part of the report of the "Department of Biological and Colloidal Chemistry", Fodor continued in Jerusalem the work he had begun in Halle, focusing on the structure of proteins and the theoretical framework and methodology posed by colloidal chemistry. One of the institute's first reports stated:

> "Research was continued on important problems of theoretical Colloidal Chemistry: the absorption of inorganic (kaolin, talcum) and organic colloids (proteins, and especially gelatin) and the viscosity of colloidal systems and coagulation of sol systems [...] the great importance of these problems in the life of cells and tissues. [...] The investigations begun in previous years on the nature of enzyme-action were continued, and led to experimental corroboration of certain theoretical conclusions concerning the mechanism of enzyme-systems".[57]

Fodor viewed his work as relevant to the understanding of the most basic phenomena of life—that is, as having profoundly theoretical qualities. Evidence of this may be inferred, for example, from the subject of his address during the university's inaugural Academic Lectures: "Correlation of Problems in the Physical Sciences and Biology". In the terms proposed in the present chapter, Fodor concerned himself with 'universal' materials and, certainly from his own point of view, with universal questions. His publications in the period under discussion included the second edition of his book, *Das Fermentproblem* (1929), and many articles, credited to him alone or in collaboration with his assistants, in journals such as *Kolloid Zeitschrift, Biochemie Zeitschrift, Zeitschrift für physiologische Chemie*, and *Biochemical Journal*. All these were disciplinary journals in physiological chemistry, like those in which he had been publishing since 1909.

However, a few articles published in collaboration with his assistant, the agronomist Adolf Reifenberg (1899–1953), and with some of his other assistants, indicate an addition to the institute's original research agenda: Fodor did turn to studying local materials as well.

The tradition of chemical research in Germany, and the examples provided by its great practitioners, offered chemists engaged in what appeared to be 'science for its own sake' a legitimate option to engage in investigations of practical significance, such as industrial research. Examples of this may be drawn from the scientific and industrial biographies of two German Nobel laureates in chemistry, Adolf von Baeyer (1835–1917) and Fritz Haber (1865–1934), and also from those whom Fodor saw as his immediate scientific forebears, like Emil Fischer (who had been Baeyer's student) and Abderhalden, both of whom had industrial patents registered

[57] Hebrew University Yearbook for 1927–28 and 1928–29, pp. 25–27.

in their names.[58] By simultaneously continuing his pursuit of 'pure' biochemical research and seeking to contribute to the Zionist enterprise by "assist[ing] agriculture and industry in Palestine with expert advice",[59] Fodor took an accepted path in the research tradition from whence he had come.

Thus alongside the universal research agenda of the biochemistry department, another—local—research agenda was pursued in the General, Synthetic and Analytical Chemistry Department of Fodor's institute. For the sake of applied research in the area of Palestine's natural resources, or of industrial development, Fodor called upon his training in general chemistry, his particular areas of expertise, and his laboratories which were surely among the best—if not the best-equipped—in Palestine at the time. He applied the tools of chemistry to solving various questions arising from life and work in Palestine, such as those connected with the fermentation of tobacco in the context of the efforts to integrate tobacco cultivation into Jewish agriculture at the time, or the possibilities for exploiting the country's mineral resources.[60]

Fodor went about his applied research in two ways. The first, based on separation between his basic and his applied research, did not yield any publications in international or even local periodicals.[61] The second was characterized by the formulation of research questions on local materials in such a way as to give them the status of universal materials, even as they retained their relevance to local agriculture. These studies included the work on the fermentation of dry tobacco, a study on sprouting peas and, above all, a study on the formation of the Mediterranean red soil (*terra rossa*) which was analyzed using the methods of colloidal chemistry. The results appeared in the international disciplinary journals in which Fodor was accustomed to publish, such as *Kolloid Zeitschrift*. Fodor characterized them as research on the practical problems of they identified as Applied Biochemistry and Colloidal Chemistry. Thus the response of the Hebrew University's Institute of Chemistry to local problems was made possible both by its practice of maintaining the separation between basic or pure research on universal materials and applied research on local materials—and by the integration of the two.

To what extent was the nascent Hebrew University's Institute of Chemistry German in nature? As exemplified above, notwithstanding its esteem and proclivity for pure research, the German university tradition had a sub-tradition of chemical research that looked favorably upon applied studies, particularly if the insights,

[58] For patents registered by Emil Fischer and Emil Abderhalden, see http://depatisnet.dpma.de:80/DepatisNet/depatisnet?action=einsteiger.

[59] Hebrew University Yearbook for 1926–27, p. 30.

[60] "The experiments on the fermentation of tobacco [. . .] on Dead Sea salts and Palestinian phosphates and minerals, with the view to working out scientific and practical methods for their exploitation" (Hebrew University Yearbook for 1927–28 and 1928–29, p. 26).

[61] In at least one case, in the context of the third meeting of the Board of Governors in 1926, this applied research was criticized by one of the distinguished scholars in the university's Academic Council, the physicist L.S. Ornstein, who chastised the poor quality of "Expedition to Southern Palestine" by A. Reifenberg and L. Picard, issued as a Hebrew-English brochure bearing the imprint of the Institute of Chemistry directed by Andor Fodor.

discoveries and processes revealed by 'pure' research could be applied in extra-academic contexts. As director of the Institute of Chemistry, Fodor followed this two-tiered German academic tradition. However, he also engaged in applied research unrelated to his academic pursuits. Even as Fodor emphasized his commitment to universal research, the rhetoric of the institute's reports published in the university's yearbooks in the second half of the 1920s demonstrates that the director was quite proud of the applied research being carried out in his institute. It would appear, then, that this integration was his intentional policy.

Managerially, Fodor put many of his junior assistants on his own research agenda (as his venerated professor, Abderhalden, had done when Fodor served in Alberhalden's laboratory in Halle, as attested to by the latter's list of publications.[62]) However, at the same time he enabled some of his subordinates to develop their own independent research agenda.[63] Fodor allowed his subordinates to share in the publications of the institute's research programs, sometimes as collaborators in the studies carried out by the director and, rarely, as independent researchers, as can be seen from the list of publications by staff members of the institute.[64]

From the above description, one may conclude that the directions and content of the investigations carried out at the Hebrew University's Institute of Chemistry, as well as its structure of authority, reveal a strong German-ness; however, as far as the research materials were concerned, this German-ness also integrated an *ad hoc* parallel responsiveness to local research problems stemming from its agriculture, natural resources, and industry.[65]

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[62]Between 1911 and 1920, Fodor and Abderhalden published 17 "original works" together; in all of them Abderhalden was the senior and Fodor the second author (see Fodor's list of publications, Fodor's personal file, Hebrew University Archives).

[63]The best example is Reifenberg's pioneering *terra rossa* study, for which Reifenberg eventually was awarded his doctorate by the University of Giessen. Subsequently, he established himself as an expert on Mediterranean soils; see Adolf Reifenberg (1938).

[64]Hebrew University Yearbook for 1926–27. That year, 17 articles from the Institute of Chemistry were published in scientific journals in Germany and England. Thirteen of them listed Fodor either as sole author or chief author with one of his staff members as the second author. The remaining four listed three other staff members as sole authors (pp. 49–51).

[65]Two historically oriented articles relating to the legacy of Fodor and the Institute of Chemistry have been written; see Nathan Sharon (2000); and Deichmann and Travis (2004). Deichmann and Travis question the introduction of chemistry at the Hebrew University in that it was not based on the three main branches—inorganic, organic, and physical chemistry—usually characteristic of contemporary universities. They are also critical of Fodor on account of his research program, which they describe as outdated, and because of his quarrels with his subordinates and other colleagues. Thus they assess Fodor's contribution to Israeli biochemistry as a case of "negative founder effect". The present author disagrees completely with Deichmann and Travis, not least because Fodor was the first scientist to put biochemistry on the biochemical protein-enzymological trajectory, which proved extremely fruitful, as Nathan Sharon has shown. Sharon points out that many of Fodor's students developed into distinguished biochemists of top-ranking international status. One of them, Ephraim Katchalski-Katzir, the first laureate of the Japan Prize (and Israel's fourth President), wrote: "Here [in biology at the Hebrew University] my interest was attracted by large molecules, the macromolecules of a cell, which play a critical role in determining life processes. I was fascinated by the lectures of our biochemistry professor, Andor Fodor, who intro-

The Multi-disciplinary Institutes of the Hebrew University: The Institute of Jewish Studies

The founders of the Hebrew University's Institute of Jewish Studies had before them the *Wissenschaft des Judentums* enterprise, established by Jews in the German cultural sphere. The 'Science of Judaism' emerged within that sphere's 'metropolitan' center, in Berlin and other cities, but operated outside it as an independent system. It was based on the resources of the local Jewish minority, in particular affiliated to Jewish institutions such as the *Jüdisch-Theologische Seminar Breslau* (founded in 1854) or the *Rabbiner-Seminar für das orthodoxe Judentum* (founded in Berlin in 1873). The new Hebrew University's Institute of Jewish Studies[66] thus seemingly represented a major innovation, at least organizationally. As a multi-disciplinary institute that brought together many fields within Jewish studies, established alongside the other institutes within the framework of a secular university, it was the polar antithesis to the common German-Jewish organizational model with its religious affiliations; in fact, it was similar to the later non-religious *Akademie für die Wissenschaft des Judentums* (established in Berlin in 1919). The use of Hebrew as the language in which academic studies were conducted was also an innovation, responding to the Zionist-Hebrew national expectation for a departure from the 'diasporic' condition in which Jewish studies were conducted in the language of the relevant national majority.[67]

Even so, a further examination of the organizational context within which the new institute operated reveals an additional aspect. The Hebrew University's Institute of Jewish Studies may have held university status, even that of 'first among equals'; however, it preserved the element of separation between the Jewish and the general fields that characterized the German-Jewish system, where the former was

duced me to the world of biopolymers"; see Katchalski-Katzir (2005). In a recent article Deichmann (2007) analyzes what can be seen as a 'paradigm shift' by the relevant scientific community, from "colloid chemistry" to macromolecular structures, to explain the specific biological activity of proteins like enzymes and antibodies. Thus we should understand Fodor within the perspective of this process of scientific change. Fodor was a respected and well-known proponent of colloidal biochemistry. Concurrently, at the height of his scientific career, although he may not have been an 'innovator' or 'early adopter' but rather a 'late adopter', he did espouse a relatively new macromolecular scientific outlook; see Fodor (1949). However, if this is the case, perhaps Fodor's persistent negative' adherence to colloidal chemistry may turn our attention to an interesting possibility in circumstances similar to those which prevailed in Jerusalem during the second quarter of the twentieth century, a successful process of transference and implantation of scientific tradition: Theories held by the relevant scientific *Träger* are less important than the accessibility of a good library, which existed on Mount Scopus in Fodor's day. Equally important are traditions of scientific investigation, such as laboratory techniques, 'tacit knowledge', exposure to the contemporary scientific forefront, as well as standards of classical scientific inquiry and the passion for excelling in scientific research, all of which are conveyed in laboratory settings or in student training seminars.

[66] In Hebrew it was called המכון למדעי היהדות, that is, Institute of the Sciences of Judaism.

[67] The claim that *Wissenschaft des Judentums* was the first modern intellectual and organizational framework for ethnic studies, engendered during the first half of the nineteenth century, was made in the earlier—Hebrew—version of this chapter (Katz and Heyd 1997). See also Adelman (1989).

encountered within the Jewish *Hochschule* framework, and the latter in the public universities. Thus, at the Hebrew University, both organizationally and from the point of view of content, Jewish history was kept separate from 'general' history, Jewish philosophy from 'general' philosophy, and so on (Rein 1997). In this sense, although the Jerusalem institute was brought into the center of the Zionist-Jewish scholarly system, it retained attributes typical of the peripheral status of Jewish studies within the German milieu.

The Hebrew University's founders had anticipated that the Jerusalem institute would be unique in adding to the accepted fields of Jewish studies that of Palestinology, or *Eretz Israel* Studies (Magnes 1936a). However, the Institute of Jewish Studies found it hard to address Palestine's local materials, such as the country's landscape, natural elements and denizens, or its archaeological findings, whose study might have facilitated the desired realistic interpretation of historical Jewish texts, the study of the Hebrew language, or the historiography of the Jews and of *Eretz Israel*. The conspicuous exclusion of these materials from the institute's program can be traced to its attachment to the scholarly heritage of Germany's *Wissenschaft des Judentums*, which dictated restriction of the field to Jewish materials pertaining to the Jewish texts designated as 'high culture', such as could be studied using historical-philological methods. And since the intellectual ideals of the *Wissenschaft-des-Judentums* enterprise were derived from those of German classical and historical scholarship, it may be concluded that Jewish studies at the Hebrew University were characterized by the strong presence not only of the German-Jewish scholarly heritage but also by that of the German humanistic tradition. The new Institute of Jewish Studies thus resembled the Hebrew University's Institute of Oriental Studies—during its first years—in its lack of response to the newly emergent Israeli aspects of Jewish-ness; it may be characterized as an institute that was both wholly Jewish and wholly German. However, in contrast to the Institute of Oriental Studies, the Institute of Jewish Studies operated as a framework for several autonomous scholars, six (full) professors and four to five lecturers, each of them developing their own individual research programs (Katz 2005). Most of their publications were in Hebrew and in scientific journals newly founded in Jerusalem, like *Kiryat Sefer, Tarbiz*, or *Zion*. In these respects as well, the Institute of Jewish Studies fulfilled its founders' expectations. And as in the Einstein Institute of Mathematics, credit for the publications was reserved for the teacher-researcher alone, promoting neither scientific cooperation with colleagues nor encouragement of future research students.

The Institute of Palestine Natural History

The Institute of Palestine Natural History proposed to study all the fields connected with the natural history of the territory of Palestine–*Eretz Israel* and the adjacent provinces of neighboring countries. During the first half of the 1920s, it functioned within the framework of the non-university Zionist agricultural research organization (then located in Tel Aviv). This organizational arrangement emulated an example created outside the German metropolitan center, designed to serve groups of recent German settlers in extra-European territories, the natural history of which was almost unknown to European science. Although this model was originally meant for non-academic yet scientific purposes, its research agenda reserved a central place for disciplinary research programs that had the character of basic research, with a focus on the relevant local materials. The objective, first and foremost, was to prepare a knowledge base drawn from observations within various local environments, and for this purpose to foster experimental applied research in fields such as agronomy.

The transfer of this German colonial model to the territory of Palestine and the *yishuv* community and then to the organizational context of the Hebrew University (its direct affiliation to the agricultural research station was temporary) meant creating an institute within which several disciplinary research programs could operate independently alongside one another. It was the German-Jewish botanist Otto Warburg (1859–1938)[68] who transplanted this model from the German colonial periphery to the emerging (Israeli) social and academic center. Following several years of botanical exploration and research within remote territories of Southeast Asia, Warburg became a prominent botanist, a valued specialist in the German colonial enterprise of the late nineteenth century, and a professor of tropical agriculture at the *Orientalisches Seminar* in Berlin. In the late 1890s Warburg joined the WZO and became a central figure within its leadership (including a term as third president of this organization), active particularly in directing his academic and organizational expertise to the various Jewish settlement projects in Palestine.[69] However, all the initial disciplinary programs of the Institute of Palestine Natural History had a shared agenda: charting the local inventory of the various elements of nature within the territory of *Eretz Israel*. This was to be done in accordance with the European tradition of scientific exploration, mapping and classification. Under Warburg's academic leadership, the junior members of the institute were charged with applying the various disciplinary traditions of academic natural history (geology, botany, entomology, vertebrate zoology) to the different components of local, multifaceted Palestine environments in their particular—developing—field of expertise. The European legacy emphasizes systematic natural history and the establishment of comprehensive national collections which represent the inventory of the relevant territory; this culminated, in the case of botany (i.e., the section of the institute led by Warburg himself), in the edition of a handbook of the flora of Palestine (Eig et al.

[68]For a recent scholarly biography of Otto Warburg, see Leimkugel (2005).

[69]The claim that the model for the Hebrew University Institute for Palestine Natural History was taken from the German colonial model is based on studying similar research institutes, for example the one at Amani (today in Tanzania, formerly Tanganyika). The latter is an example of those German multi-disciplinary colonial research institutes which Warburg was familiar with, thanks to his role in the German colonial enterprise; see Bald and Bald (1972). For Warburg's ideas concerning the relationship between basic research and the colonial economy, see Reimer (1903, pp. 193–207); regarding the wider context of contemporary German research interests in extra-European territories, see Pyenson (1985).

1931). This handbook, *The Analytical Key*, based on the newly founded herbarium at the Hebrew University, was intended to serve both scientific and educational purposes and was a forerunner of a future academic edition of *Flora Palestina*.[70] However, the research autonomy granted to the junior staff members allowed a change in the institute's original research agendas. For example, its systematic botany projects were now supplemented by a regional phytogeographical and ecological focus on the natural elements of the Mediterranean and Western Asia desert regions. This opened the way to scientific discoveries of general interest and to innovations in the relevant disciplinary conceptual systems (Reichert 1939; Bodenheimer 1935).

Thus, although the Institute of Palestine Natural History was initially modeled on a German organizational scheme and on typical European research programs aimed at creating a repository of various natural elements, its German-ness was limited, or perhaps entirely lacking. Its organization was tailored to a research framework typical of German extra-European, colonial contexts, and the institute was definitely directed in a non-centralistic style. However, creating a repository of the various natural elements was not its exclusive research agenda. The local materials—serving as universal ones within the Linnaean tradition and ambition of collecting all the plants in the world, classifying them and giving them scientific names—functioned as a substrate for a new outlook within the emerging research traditions of the geography of plants and animals and its associated ecology. At the same time, the institute's basic commitment to studying local materials led to a significant increase in the knowledge base of the various elements of Palestine's natural history. This enabled the institute's researchers to facilitate the advancement of two national goals. The first was that of expanding Zionist settlement and developing agriculture; the disciplinary knowledge accumulated by the institute provided a basis for the relatively scientific-based planning of settlements and for applied agricultural research (Katz and Ben-David 1975). The second goal had to do with the emergent local culture; in this context, the local materials functioned as Jewish—indeed, Israeli-Jewish—materials.

Nineteenth- and early twentieth-century Palestinology, even when focused on the natural history of Palestine, was concerned mainly with the biblical dimensions of the historical Holy Land. Regarding the natural elements of the country, such as plants, most of the Palestinological reconstructions were oriented towards the scriptural past and were not unequivocal.[71] By contrast, the Hebrew educational system

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[70] During the 1930s, the Hebrew University Press sold more than one thousand copies of this publication, probably making it the most popular book within the Jewish community in Palestine after the Hebrew Bible. Evidence for the broad diffusion of binary names for local plants in Israeli culture may be found in the novel *Days of Ziklag* by S. Yizhar (1958) and in the popular song "A Waltz in Defense of all that Grows" by songwriter Naomi Shemer.

[71] One of the early works of the type of flora, produced by Linnaeus and his disciples in 1756, was *Flora Palaestina*. It listed about 600 distinct plants collected in the territory of *Palaestina* (which extended from Lebanon to Egypt), ordered by their Linnaean binomial names. In some cases it used two other naming systems: vernacular plant names in Arabic; and (alleged) biblical names in Hebrew (both transcribed in Latin characters). Each of the three plant-naming options in the *Flora Palaestina* was eventually associated with a defined research program during the nineteenth cen-

that was developing in the period under discussion created a demand for unequivocal terms and names of native environmental objects that were not yet available in modern Hebrew. The institute researchers met this national, cultural demand for knowledge of the country' (*yedi'at ha'aretz* in Hebrew, a calque from the German *Heimatkunde*) and for a nomenclature of its natural objects by means of scientific Hebrew terminology provided to the country's teachers and the general public (Katz 1985). In so doing, the Institute of Palestine Natural History went further than any of the university's other institutes in providing cultural *desiderata* expected by the contemporary Zionist Jewish Hebrew-speaking community in Palestine/*Eretz Israel*.

Summary and Concluding Remarks

This chapter sought to examine the Hebrew University of Jerusalem as an offshoot of a European academic 'stem'—more particularly, a German and a German-Jewish one—and how it flourished, in national, spatial and institutional circumstances different from those of its origin, in response to its new surroundings. In other words, to what extent can we discern in the fledgling Hebrew University the presence of pre-existing models—in the shape of scholarly ideals, scientific agendas and research programs, as well as organizational structures? And to what extent did it create new models, whether out of a desire to amend or reform the existing models, or in response to the new circumstances within which this national university began its operation in Jerusalem?

Indeed, it is tempting to view the opening of the Hebrew University in 1925 as part of the broader phenomenon of the voluntary emigration and enforced expulsion of scientists and scholars from central Europe in the 1920s and 1930s, leading to the creation of new centers of scientific and scholarly research. The great nineteenth-century German scientific heritage which hitherto had slowly pervaded Europe and beyond, was now carried by its former disciples to new intellectual havens, from Istanbul to New York and Buenos Aires. At least in its beginnings, however, the Hebrew University was less the outcome of the aggressive push of antisemitism and Nazism and more the result of the pull exerted by one of the Jewish national movements of the time—Zionism. The central place held by the establishment of a Jewish university within the multifaceted Zionist visions is the only overwhelming explanation for the preference of a few dozen scholars and scientists, most of them

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tury and later. The first, following Linnaeus' floristic-taxonomic program, is exemplified by Post (1896). The second one aimed at the reconstruction of the biblical scenery, manners and habits; ethnographic research of Palestinians' Arabic dialects, folklore and everyday life was considered an appropriate way of comprehending the literal, i.e., textual meaning of both the Old and New Testaments, and Tristram (1868) is the best example of this scholarly tradition. The third one, the lexicographic program, based on the philological research tradition of Semitic languages, aimed at the reconstruction of ancient Oriental languages; it is exemplified by the classical work of Löw (1924–1934).

of German-Jewish origin, or trainees of the German academic system, for the new university, remote from European cultural centers.

The Hebrew University project was guided by a vision nurtured in the bosom of a national liberation movement, which envisaged the Jewish university as one of the central organs for the expression of its essential currents. Among those currents was the aspiration to excel in scientific research—an ambition from which an operative plan for this institution-in-the-making was derived, incorporating a set of academic priorities. On the other hand, the project's practical realization involved no small degree of contingency, resting as it did on the personal transfer of academic expertise. In most cases it was the particular scholar or scientist who was willing to come to Jerusalem who determined the specific nature of the academic tradition that arrived with him: the initial scholarly focus of the institute, the composition of its disciplinary and research fields, and the style of its management, as well as the mode in which the institute responded (if at all) to the new cultural and physical environment. Moreover, as evidenced by what transpired in subsequent periods, these beginnings would set the trajectory for the institutes' development in years to come, from the point of view of their research foci and agendas.

The confluence of these factors led to a framework in which some of the models and traditions brought to Jerusalem were indeed those of a German metropolitan academic center; others, however, were not. The university's national-Zionist character, manifested primarily in its being symbolically 'Hebrew' and located in a non-European territory—*Eretz Israel* and more specifically, Jerusalem—spurred an interest in scholarly models drawn in part from the periphery of the German metropolitan domain. Once they arrived in Jerusalem, all these models were drawn into the center of a new system.

The foregoing discussion has shown that the various institutes of the new university responded in different ways to the notions expressed by the university's founders, resulting in a continuum with strict 'German-ness' (including Jewish-German-ness) at one end of the continuum and different measures of 'local-ness' at the other. Thus the principal contribution of the Institute of Mathematics and of the School of Oriental Studies to the Jewish national enterprise was designated as their commitment to excellence in the pursuit of pure science in the strict German academic sense. Similarly, the exclusive concern of the Institute of Jewish Studies with materials drawn from Jewish 'high' culture—as opposed, for example, to local Jewish ethnographic materials—ensured its wholly German character. In the Institute of Chemistry, by contrast, the limited allocation of any part of the research program to the study of local materials, combined with the organizational style of the institute's head, which allowed the junior researchers some autonomy, gave it a degree of non-'German-ness'. The Institute for Palestine Natural History went even farther in this direction, while at the utmost extreme of 'local-ness' lay the two units for medical research, whose traditions explicitly placed indigenous issues at the center of their concern. Moreover, the practical results of the research conducted in these two units put their scientists in the position of fulfilling national expectations of the highest importance: improving the health of the country's population and livestock.

It may be said that during the early years of the Hebrew University of Jerusalem, the nineteenth-century German scholarly heritage was well represented in several forms. Does this justify labeling the Hebrew University a 'German' university? This chapter has suggested several criteria for identifying German-ness, in particular a commitment to the neo-humanistic ideal of science for its own sake'. This kind of commitment did characterize some of the Hebrew University's research institutes, but not all of them. However, if this criterion is replaced by a less constricting commitment to disciplinary research, a different picture emerges. While disciplinary research does imply an adherence to programs that respond to an inherent disciplinary agenda, it allows for some relevance of—or even involvement in—study programs whose questions derive from problems extrinsic to the autonomous disciplinary agenda, and which eventually may be classified as applied research. Given the German roots of most of the research programs carried out within the emerging university, its commitment to this type of disciplinary study does not conflict with the enthusiasm for new research programs and disciplinary investigations of local materials and is indicative of the autonomy of both the institutes and their scholars. The Hebrew University may have had deep roots in the German research core, as shown by its adoption of some German scholarly ideals, scientific traditions, and research agendas; however, it developed not as a dependent, quasi-colonial extension of that core but as an independent, autonomous offshoot, sovereign in its determination of the scholarly and scientific agenda and research programs to be nurtured within its institutional frameworks.[72] In all this, the Hebrew University resembled a range of institutions typical of contemporary Europe and the United States.

Moreover, the ramifications of the set of epistemological and organizational principles formulated for the Hebrew University in the 1920s, and the framework of institutes that emerged on this basis, went far beyond the determination of the university's own development in the 1920s and 1930s. Given the Hebrew University's involvement in the establishment of Israel's other universities—or at least, its status as their prime example—it may be said that these elements also had a formative influence upon the whole of Israel's scientific research frameworks and system of higher education. This claim is buttressed by the extension of two of the characteristics discussed herein to Israel's other universities: All of them maintain the centrality of basic disciplinary research, and several of them have kept the separation—both conceptual and organizational—between 'Jewish Studies' and 'General Hu-

[72]The claim that the Jerusalem case does not belong to the family of research systems developed under colonialism or other forms of dependency is based on the pertinent research literature. With the publication of the seminal works of Donald Fleming and of George Basalla, a new research area—the comparative history of science outside Europe—was launched. It became clear that even long after political sovereignty had been attained, the scientific activities conducted in 'provinces' retained a strong dependence on their respective European scientific metropolitans. The pervasiveness of this phenomenon also characterizes the early (and even not so early) stages of the introduction of Western (mainly British) science to British ex-colonial territories such as the United States and Australia. See Home (1990); see also Reingold and Rothenberg (1986), Petitjean et al. (1991), MacLeod (2000).

manities'. If this claim is sustained by future research, it will attest to the extent to which particular structures for the pursuit of scholarship that were conceptualized in response to the condition of the Jews as a non-sovereign ethnic minority, such as the German-Jewish idea of the *Wissenschaft des Judentums*, continued their vitality in sovereign frameworks as well. This suggests the desirability of studying the history of the Hebrew University's early years comparatively, in juxtaposition, for example, to other research and higher education settings that developed in response to ethnic minority status or in the context of a struggle for national sovereignty.

The early history of the institutes of the Hebrew University of Jerusalem might also fruitfully be compared to that of other German high culture traditions that reached the *yishuv* in the 1920s and 1930s. Those that found organizational expression included the concert-hall music cultivated by the Palestine Symphony Orchestra—which was to become the Israel Philharmonic Orchestra—and, to a certain extent, the country's psychoanalytical movement. Other traditions, carried by individuals, included Bauhaus architecture and artistic dance and ballet. It would surely not be too speculative to suggest that the longstanding adherence of the Israeli Philharmonic to a classical German repertoire recalled the German character of the Einstein Institute of Mathematics or the Institute of Jewish Studies. On the other hand, in their creative response to local materials, the transplanted Bauhaus architecture and artistic dance and ballet traditions bore more resemblance to the study of the natural history of *Eretz Israel* at the Hebrew University.[73]

As has been claimed above, the academic policy adopted by the founders of the Hebrew University was based on research institutes rather than teaching faculties as the central organizational units of the new institution. This structure was intended to guarantee against a "low scientific standard and a correspondingly low reputation" (see above, "Preparations Report" p. 106). After the foregoing voyage through these institutes during their initial years of operation, the question nonetheless arises, to what extent this initial insistence on research institutes indeed achieved what was expected of it.

The foregoing examination of the academic performance of these research institutes indicates clearly that, even from a contemporary perspective, the new university indeed was successful in at least two aspects. First, it merged well into the research agenda of the forefront of science at the time. Secondly, and concomitantly, it led to a considerable degree of intellectual independence, as evidenced by the original research programs developed because of the university's responsiveness to local

[73]The most fruitful comparison, however, may well be found by looking at the German-Jewish tradition of Jewish Studies, which reached the United States in the nineteenth century, several decades before it arrived in Jerusalem. It developed at first in the context of the various Jewish movements—that is, outside the university system—but in the twentieth century, and particularly since World War II, it has become integrated into the universities, at first in the United States and more lately in Europe as well (Cohen and Greenstein 1990). To undertake such a study would be to compare several offshoots of the same core—that of the German-Jewish scholarly heritage, which, for its part, is an offshoot of the German scholarly heritage (Wiese 2004).

research opportunities and desiderata. Thus, it can be affirmed that the university's "scientific standards" were at least reasonable, even during its first years.

However, the heads of the new university aspired to more than simply ensuring that the new institution did not exhibit low scientific standards and reputation; they sought first-rate scientific excellence, and they saw the research institutes as the means to achieve this goal:

> "[...] Research Institutes by attaining a high scientific standard will confer luster upon the University [...]" And: "[...] A very high level of Scholarship must be placed in the forefront of the University. The quality of work turned out from two or three Institutes in the first two years of the University life will set the standard. This standard will be the lodestar in the future for the students for whom provision will later be made".[74]

Some 85 years later, the university can proudly cite "[eight] Nobel prizes and a Fields Medal in Mathematics won by graduates and staff members of the university".[75] Even if this list suggests that the Hebrew University is a bit too magnanimous in claiming some of those laureates to its own credit, there are other quantitative indicators that attest to its institutional academic quality. For instance, membership in the National Academy of Sciences (USA): In 2006, nearly half the 15 Israeli members of the National Academy of Sciences at least began their research careers at the Hebrew University.[76] And the academic genealogy of several of them brings us back to the Institute of Chemistry and the Einstein Institute of Mathematics as their *alma mater*.

To what extent can we actually attribute the eminence achieved by these leading scientific figures whose roots were in the Hebrew University actually to the ongoing influence of these early research institutes? And what precisely was nature of such influence?[77] These are questions that call for future research.

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[74] The Hebrew University, *The Inauguration*, large format; see also footnote 6 (on p. 105) above.

[75] https://www.huji.ac.il/huji/nobel/indexE.htm.

[76] According to the National Academy of Sciences of the United States of America, Membership Directory, July 2006, of the foreign members that year, 5 were Italian (p. 340), 11 were from Sweden, and 24 were from Switzerland (p. 342).

[77] See footnote 65 (on p. 130) above.

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Part IV
Organization and Growth

Chapter 8
The Excellence of IT: Conceptions of Quality in Academic Disciplines

Andrew Abbott

Abstract The chapter approaches the subject matter, the Research Assessment Exercise (RAE), from the perspective of the quasi-enthnographer.

In the UK, as funding councils have started to finance institutions on the basis of a RAE designed to measure 'quality', questions arose as to how such concepts can be measured, whether or how research groups ought to be funded, and to what extent such measurements are subject to manipulation. The chapter addresses the first question on the basis of responses to the RAE by academics. Five dimensions are explored: cognizability (or measurability), units of measurement (or what should be measured), space and time (or their spatiotemporal character), modality (positive and negative aspects affecting perceptions) and content ("what is 'excellence' in research?"). No shared foundations on which to base a RAE could be deduced: "nowhere in these comments is there anything about the substantive content of excellent work nor, indeed, of work at all". This statement is true for both the sciences on the one hand and the humanities and social sciences on the other.

There is a curious anomaly in the study of knowledge and its institutions. While the history and sociology of science have flourished, the history and sociology of the social sciences and humanities have not. There has been a steady but small flow of empirical work. But there is little or nothing corresponding to the great theoretical debates between the Mertonians and their opponents over scientific knowledge and its purported norms.

This difference may reflect the different projects of the two great continents of knowledge. The practice of science assumes that there are answers "out there" in the natural world and that we use theory and empirical investigation to find them. But the humanities and humanistic social sciences (HHSS) are more interested in creating new interpretations of well-known things, or putting together things previously kept apart, or maximally filling a space of possible things to say. These practices do not have the same directional quality as do the sciences, in which old work is necessarily subsumed and rejected by new work. There is no best interpretation of

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Moby Dick, nor must we possess such an interpretation before we write a book about Melville. There are to be sure what I (Abbott 2008) called Rankean facts, which are subject to purely empirical investigation: the date of Melville's death, the status of his finances in 1854, and so on. But there is no necessary order or direction to interpretations of his great novel, just as there is no necessary order or direction to HHSS knowledge more broadly.

But despite the surface plausibility of this identification of differing goals, it is an empirical question whether humanists and humanistic social scientists do in fact think they accomplish something different than do scientists. To investigate this question I here use data on disciplinary reactions to an outside threat that challenges disciplines to clarify not only their conceptions of research excellence, but also of their underlying knowledge projects. My chief finding is that the knowledge projects of the sciences and humanities seem not different but similar, and in particular similar in their contentless character.

Method

Our data come from the British Research Assessment Exercise (RAE). Dating from the late 1980s, the RAE uses expert panels to rate every department and research unit in the United Kingdom. Units to be assessed submit a fixed number of papers per person, as well as a unit narrative and various statistics. A concurrent consultation process identifies and appoints expert panels in each field, which read the submitted materials, rate both individuals and units, and publicly report the rating of units and the distribution of the ratings of individuals. These ratings then determine a substantial portion of unit funding.

Disciplinary responses to the 2001 RAE can all be found on the relevant website.[1] They respond to a widely-distributed "invitation to comment" from a committee charged with the post hoc evaluation of RAE 2001. The invitation resulted in verbatim responses to a varying number of fixed questions by a considerable number of voluntary (and hence self-nominated) respondents. There are 87 total responses whose disciplinary origins can be clearly identified: 30 from the humanities, 31 from the social sciences, and 26 from the natural sciences. These 87 responses cover most of the academic disciplines in those areas. Since I aim to contrast patterns of discourse and assumptions across the broad continents of academic knowledge, such breadth is sufficient. As for quantity judgments, the data will support "most, many, some, few", but nothing more detailed.

The data's self-nominated character might seem worrisome; perhaps these are mainly complaints from departments that did badly, getting ratings of 3a or less. This is however not the case. There are 24 responses (of the 87) from departments. Of these only 4 are from departments rated 3a and another 5 from departments rated

[1]The responses can be found at www.ra-review.ac.uk/invite/responses/subject.asp. Since the RAE system is changing fundamentally, there will probably be no such response to the 2008 exercise.

4. The rest are rated 5 or in a few cases 5*, the highest rating in RAE 2001. Nor is concentration a problem: the departmental responses are scattered by geographic location, by discipline, and by status of university. The remaining responses are from national bodies (disciplinary or interdisciplinary societies, and councils of chairs or of program directors) or from senior administrators within the three areas here discussed.[2]

Strategic intent is clear in some responses; for example, interdisciplinary groups often push for their own separate panels in the next RAE. But the majority of these responses are neither rants or plaints. If there is an overriding emotion, it is an exasperated perplexity. Moreover, any biases implicit in self-nomination, emotional reaction, and strategic response do not affect the investigation. My aim is to recover from the language and assumptions of these responses their underlying conception of excellence in research. There is little reason to think that scholars would systematically deform their conceptions of excellence in this setting. They are more likely to have used their customary conceptions, indeed all the more so if they were angry. General differences between the knowledge projects of the humanists and social scientists should still be obvious.[3]

The more important bias is in ourselves. As scholars we think we already know perfectly well what is 'quality' research or 'excellent' research. Yet it is a discipline's substantive sense of these words that we seek. To disarm such prior beliefs, I global-replaced the word 'excellence' throughout the data with the word IT. I replaced the word 'quality' with the word ITQ. I replaced the word 'excellent' with the word ITLIKE. I shall follow that convention in quotes below, hoping to prevent readers, too, from importing their own knowledge into their understanding of IT.

Having made this replacement I seek to infer the meaning of IT, ITQ, and ITLIKE from statements made about them. For example, the phrase "a work of lower IT" implies that IT is a quality that inheres in works and that has a rank order, just as the sentence "only the ITQ of recent publications should be measured" implies that ITQ can be lost by an individual over the life course. Neither of these statements, it should be noted, gives any information about the substantive content or purpose of IT.

The Excellence of IT

I consider the meanings of IT and ITQ in these documents under five headings. The first of these is cognizability: how can IT be known? The second is units: of what

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[2]Of responses from administrators, I used only those from deans or heads of schools, since these can be attributed to the three subject continents. Of those from societies, some are based on surveys, some on consultation with membership or with committees, and some on the opinion of whatever society officer chose to respond.

[3]I should underscore that I am not interested in explaining why respondents thought this or that about the RAE, in finding 'causes' of their attitudes like RAE rating received, type of institution, funding, etc. Nor would knowing such 'causes' change my analysis, which is addressed to implicit conceptions of knowledge.

kinds of things is IT a property? The third is spatiotemporal character: where is IT? where might IT appear? When? Does IT endure? The fourth concerns the issue of modality: is IT necessary or merely possible? absolute or relative? always positive or sometimes negative? Finally what is IT substantively? Do the sciences say that IT work "extends our understanding of the natural world", while humanists think IT work "deepens our interpretations of human action and values?"

Cognizability—Metrics and Experts

Two central facts emerge about how IT and ITQ are known. First, IT and ITQ cannot be measured. Second, IT and ITQ can be recognized only by experts: "The expert and the peer are [...] one and the same" <h12>.[4]

Humanists are blunt about this. "We reject", says one group, "any approach that relies solely on the dubious notion of objective data" <h10>. Such measures, says another response, have "no obvious pertinence to arts and humanities research" <h11>. These denials are however limited to the arts and humanities; an algorithm based on metrics "may be quite properly useful in science and engineering" <h12>, remarks one group.

By contrast, the statisticians and some of the social scientists feel that measuring IT by metrics is feasible and preferable. The Royal Statistical Society says that "Good practice, which provides academic objectivity and possesses [virtues like rigor, fairness, clarity, and so on] is well understood, and indeed commonly taught by statisticians [...]"<ns19>. Or again, "where hard metrics can be used to inform expert judgment, panels should be formally obliged to use them" <ss4>.

But there is disagreement. "The notion that such metrics are 'totally objective' is clearly a fallacy as virtually all metrics are subject to different interpretations" <ss9>, says one group. "The work of the sociologists of science [...] has repeatedly demonstrated the underlying subjectivity behind such measures" <ss2>, says another. Indeed, there is some overt hostility. The heads of anthropology departments say "monographs of fundamental importance to the continuation of the discipline do not necessarily rate high in bibliometric measures, while meretricious works may do so" <ss22>. Another group is blunter: "Garbage in, garbage out" <ss26>. As in the humanities, suspicion of colleagues surfaces: "Citation enhancement is a known sport", remarks the LSE Policy group <ss15>. Others argue that citation analysis "will largely reflect the existing disciplinary nature of knowledge while there is no a priori reason why such organization will correspond to future needs" <ss17>.

The mixed verdict of the social scientists disappears in the sciences, which are—like the humanities—overwhelmingly negative. There are to be sure some radical objectivists. "The system should be measured using metrics, the majority of which

[4] < h12 > means the twelfth humanities response in our list of respondents. < ss∗ > and < ns∗ > are the equivalents for social and natural sciences. Since the quotes are retrievable via search algorithm from the on-line data, we save space by omitting a list of respondents.

are comparable and objective fact" <ns7>. Or "The assessment must be automatic, leaving no room for subjective assessment" <ns13>. But these are rare drops in a sea of rejection: "[S]ome objective data must be fed into the consideration of research strength, but it is inappropriate to judge research purely on the basis of metrics" <ns3>; "No single metric can measure the ITQ of a piece of work" <ns11>. As elsewhere, there is suspicion of colleagues: "care must be taken to avoid the negative consequences of 'metric-driven' behavior" <ns9>.

In summary, the vast majority from all fields believe that IT and ITQ cannot be measured by objective indicators. Such indicators are irrelevant. If relevant then they are unfeasible. If feasible, then they are wrong. Disagreement comes only from those who invented those indicators or whose daily practice involves them. To some extent this immeasurability is attributed to games-playing and manipulation; to some extent metrics are thought useful in other fields or as minor adjuncts to other forms of judgment. But the main and decisive fact is rejection.

While IT and ITQ cannot be measured, they can nonetheless be recognized by members of the group itself. This fact is accepted from the humanities ("If you want a sound assessment, it makes sense to ask the experts" <h4>), to the social sciences ("expert peers should carry the main burden of judgments of ITQ" <ss19>), to the natural sciences ("lay people cannot judge research IT because by its very nature it exists at the upper limits of even expert knowledge" <ns17>.

But only true experts can recognize experts. Even closely allied fields are considered inexpert. Architects complain that the built environment panel "had little appreciation of architecture as a discipline [...] nor of creative design output or of book output that crossed the technology/history divide" <h7>. Economic history, women's studies, and criminology make similar complaints. Even within mathematics this "outsiders" issue is raised: "grades awarded to some departments reflect the uneven distribution of disciplines [i.e., subfields of mathematics] across the panel and a formulaic approach to grading rather than the intrinsic ITQ of the work being assessed" <ns22>.

All kinds of outsiders are rejected: practitioners ("users and practitioners [...] are important constituencies, but final decision must rest with panels composed predominantly of peer academics" <ss18>), financial experts ("The inclusion of financial experts appears only to be justified if there is an explicit need to judge value for money rather than absolute research IT" <ns17>), non-UK disciplinary experts ("Many of them, despite their eminence, are not familiar with the mindset of the UK civil service [...] It is questionable if they contributed anything to the process" <ss29>), even critics or audiences (" 'practitioner researchers' should identify the peers of the work, the sort of practitioners who will most immediately understand and appreciate what this piece is doing, explain what one's peers will find original and striking about the piece [...] <h18>).

In short, IT cannot be measured, but IT can be recognized by experts, who are defined as the members of the group whose ITNESS is at issue. This is a totemic religious system in which only clan members can recognize the totem.

Units

ITness is implicitly predicated of several things: works, research in general, individuals, groups (research groups, departments, centers), and even whole disciplines. There is also explicit discussion of the proper unit in which to conceive of ITness and of who among scholars is capable of ITness.

IT is sometimes explicitly predicated of works and research. The latter is more common: “research ITQ in the humanities” <h1>. Sometimes it is not research that has ITQ, but “output”, as in “Research income measures input and cannot verify the ITQ of output” <h3>. Only once in a while do we hear of highly specific IT as in the phrase “accurate measurement of research ITQ at the level of specific contributions” <ss11>. The idea that primarily works have ITness pervades the discussion of the ITness of individuals. Thus, the common belief that highly-rated departments or individuals may rest on their laurels logically contains the assumption that a given individual can produce works of varying IT throughout the life course, which in turn implies that the true locus of IT is the work, not the individual.

That point about implicit units made, however, the majority of responses explicitly locate IT either in individuals or in groups of one sort or another, rather than in works. “Assessment could be at Unit of Assessment [UoA] level, but measure of IT should be at individual or research group level” <ns8>. A group of humanists speaks of “the ITQ or range of an individual’s or a department’s research” <h14>. Most commonly, the explicit unit of IT level is the individual. For the economists, for example: “The ITQ of a researcher is a latent signal to be extracted from observable outcomes in the noisy outcome that constitutes creative research” <ss11>. Or from the humanities—“The lone researcher who may [...] produce internationally recognized publication [...]” <h30>. And from the historians “The ITQ of research in any institution of higher education is ultimately dependent on the ITQ of its individual researchers” <h26>. Even the physicists, with their gargantuan team experiments, recognize an individual level: “where possible, [assessments] should be made at the individual level. In subjects where papers have many authors, the individual should be encouraged to state their [sic] role in the research” <ns10>. All of these phrasings assume that IT is something that finds its place in people, and most often that it is an enduring quality of people.

By contrast, there are respondents—chiefly but not exclusively from the natural sciences—whose language locates IT in groups. For example, “[there are] nodes of IT within the units” <ns15>. Some respondents are adamant that the group level is the real or proper one. “If individuals are assessed we could lose synergy created by larger groups and units of assessment” <ns17>. Or “Assessment of individuals would be unwieldy and invidious and assessment at the level of institutions would conceal centres of IT [...]” <ss19>.

By contrast, others are quite hostile to group-level concepts of IT. “While collaborative work is practiced by many Arts and Humanities Scholars, the bulk of their work is and will be for the foreseeable future done in a single scholar mode which experience has shown yields large amounts of ITlike output [...]” <h27>. Still others favor a bilevel concept: “selected publications should remain a key diagnostic of individual calibre, but [. . .] the final departmental scores should also reflect to some degree the overall research culture of research groups and the department" <ss25>.

These various loci of IT are clearly not exclusive. But overall it seems that IT exists in certain people as a potential, perhaps even as a realized potential. It may in some fields be located in groups or "research cultures". But in the last analysis IT is directly manifest only in work itself.

Space and Time

As we have seen earlier, units of IT like individuals and departments are embedded in a larger community of experts who are capable of judging a particular type of IT. This larger community—implicit throughout—is a 'discipline'. The detailed properties of disciplines are never given; what a discipline shares is simply its ability to judge the IT of its own work. A discipline, that is, is a type of IT.

This exclusivism is evident in the complaints mentioned earlier about the inability of outsiders to judge the IT of research or researchers, which arise most often in discussions of 'interdisciplinarity'. "There must be a more clearly worked out system of inter-panel assessments for genuinely interdisciplinary groups" <h20>, says one design department. Faculty in women's studies resent being rated in disciplines "where the specificities of their research cannot gain full recognition" <ss19>. The History at Universities Defence Group was blunt: "Interdisciplinary research and multidisciplinary research should be encouraged, but the tail must not wag the dog" <h26>.[5]

Exclusivism is also underscored by complaints that panel experts were generalists unable to comprehend the details of specialized work. We hear this from scholars in French language, from geographers, from psychologists, from mathematicians. Certainly the majority agree with the Leicester archeologists who remark "creating broader subject panels would weaken the reliability and credibility of peer review" <ss25>.

Inside disciplines, the spatiality of IT varies somewhat between the humanities and the natural sciences. Division of labor is unmentioned (probably because taken for granted) in the natural science responses. But it figures occasionally—and negatively—in the humanities, where it is perceived as alien to the humanistic style of research: "Many fields, especially in the humanities, do not require concentration of people to enable high ITQ research. Lone scholars, or small groups of researchers frequently make significant contributions" <h20>.

Explicit discussion of IT differences between units within a discipline is largely limited to the sciences, where it is taken for granted. There is much explicit elitism:

[5]At the same time, many respondents saw themselves as potentially subject to more than one 'discipline'. The disciplines, that is, may make up not a set of exclusive categories, but rather a system of tolerances, in which many scholars fit under several panels and each panel could potentially assess several such different subgroups of scholars.

"The absolute number of 'high ITQ researchers', rather than the mean, matters in research, since 'extreme values' are what really makes the difference" <ss12>, and "weaker or developing groups should be encouraged not to apply for expert review, and to bid for funding for a small pot of money ring-fenced for developing research capacity" <ns16>.

Conceptions of the distribution of IT change when money is involved. Should IT be conceived as absolute or as relative to available resources? This is not an issue for humanists, who get little money in any case. Among social scientists, it engages only the economists, who predictably argued that "marginal research productivity should be equated across all units so that the impact of the next pound of funding is the same everywhere" <ss11>. But the issue divides the natural sciences. Where one group holds "In essence, exceptional 'value for money' has been achieved in the short-term by jeopardizing the longer-term health of the university research infrastructure" <ns1>, another says "the criteria for judging performance should, in addition, include value for money" <ns5>.

A particular distributional puzzle is the department that loses major funds because of minor ITQ differences. One such victim complains at length, but a very highly rated department voices the same issue, speaking of the "large funding impact a relatively small change in submission can make" <ns8>. At the same time there are complaints throughout the humanities and sporadically elsewhere that large differences in rating had had *no* consequences (except perhaps smaller declines than worse-rated departments had). Thus the relation of funding to the distribution of IT was felt to be insufficiently elastic by one group and excessively elastic by the other.

Across the disciplines, one might have expected that money comparisons would produce explicit statements about the differing knowledge projects of the humanities versus the sciences. But the comparisons are contentless. The natural scientists—being much richer—are of course silent on this matter. Many humanists and social scientists, however, argue that the humanities are excellent value for money, being nearly costless ("Many of the very best research projects are run on limited funds" remarked one group <ss5>). And social scientists criticize cross-field comparison on methodological grounds. There is, says one group, "no empirical evidence [...] that research IT can be measured on a scale that allows meaningful between-discipline comparisons to be made" <ss4>. Even the economists say present scores "are more or less worthless for cross-subject comparisons" <ss11>. But—most important—there is nothing at all about the differing substance of the values being pursued in the differing disciplines.

Like its distribution in disciplinary space, IT has also a distribution in time, both in the life cycles of individuals and in those of disciplines. Moreover, this distribution has a dynamic aspect. All of these themes are central among the responses, but, like the spatial responses, produced no substantive discussion of knowledge projects.

Nearly all responses agree that the IT of departments changes significantly over time. This is implied by the uniform opposition to the proposal of ratings based on general "track record" and infrastructural strength rather than on specific ratings of actual work. One judicious respondent puts it simply: "historic performance in previous assessment cycles [...] is not a guarantee of future achievement" <h3>. This theme repeats endlessly. Humanists think historical ratings have "obvious in-built tendencies to intellectual conservatism, the privileging of established fields at the cost of developing ones, and the entrenching of institutional elitism" <h7>. Social scientists think they would lead to "elitism and disciplinary sclerosis at one end, stagnation and decline at the other" <ss28>. Natural scientists think historical ratings would produce "stagnation with no motivation for improvement" <ns12> or again that they would lead to "complacency on the part of higher-rated departments and a lack of support for developing fields [...]" <ns17>.

Only a few see problems with this insistent focus on recent ITQ. One group wrote: "It is unfair for the distribution of research funding to be overly influenced by past performance; on the other hand, top-rated departments must secure appropriate reward for IT" <h26>. On the other hand, another said ratings "need to reflect the current situation, or at least the recent past: [...] Otherwise there is no incentive to improve" <ns8>. This widespread argument that "otherwise there is no incentive to improve" implies that IT is driven partly or even largely by external incentives. Indeed, nowhere in this data is a statement of the form "We aim at great work whether you fund it or not, but it will be easier if you fund it". To be sure, the whole rhetoric of the RAE militated against such a statement, but its complete absence is surprising nonetheless.

About the speed of these various changes, however, there is disagreement. A few responses argue that IT changes slowly. "Changes in research strength are, on the whole, slow" <h4>; "Institutions go down as well as up, albeit slowly" <ss29>. Usually, however, slowness is attributed to the monograph-writing process, rather than being derived from an actual theory of slow change in IT. Many social science and humanities responses want longer review periods to adjust to this longer rhythm of production. The historians remark that "some of the greatest and most influential works of historical scholarship have taken years to conceive and research" <h26>. Many responses suggest, explicitly or implicitly, that faster production means lower IT, a danger for younger scholars in particular. One humanist mentions young colleagues who "rush out a series of articles rather than deepen or broaden their work into a first-rate book-length publication" <h14>. A natural science group note that the "pressure to produce results quickly [is] especially felt by younger staff" <ns11>.

In the natural sciences a different concern for the long-run emerged in responses that made an implicit distinction between what we might call merely IT research—safe, solid, and predictable—and truly IT research.

> "Much of [speculative research] will fail. It can only be assessed on a very long timescale and it cannot be assessed on the simple basis of the number or short-term impact of individual papers [...] Truly outstanding science, [...] is so unpredictable and can take so long to gestate that it is almost impossible to envisage an assessment process that would work" <ns1>.

This distinction between different temporal types of IT perhaps corresponds to those humanistic comments that saw rapid production as an invitation to lesser IT. Yet neither set of comments links this difference in rates to any differing substantial content of IT or to differences in knowledge projects.

But while some responses emphasized the long-term nature of true IT, the more common response insisted on sharp and sudden changes. From the humanities we hear of “the leaps that many UoAs achieved from 3 to 5 as a result of institutional investment” <h20>. From the social sciences, “Longer [intervals] than about 6 years might fail to pick up substantial changes in relative position” <ss13>. From the natural sciences: “individuals, groups and departments can improve or indeed deteriorate markedly over a period of perhaps 5 years” <ns2>.

Discussion of change in ratings revealed further temporal themes. That no one wants purely prospective ratings clearly indicates that no one believes (or wants to believe) that IT can be predicted accurately. All the same, the majority of those who discuss the issue want some prospective component and many of those want that component to dominate. This strong orientation to the future ties to the theme of sudden, sharp changes in IT and to the near uniform rejection of historical ratings. Many believe that support for past performance is inherently problematic. On the one hand is self-fulfilling prophecy. “If a panel awards a 5* [. . .], then the Department will get resources to allow them to perform well in the future [. . .]” <ss4>. On the other hand is the expectation of complacency and consequent decline. These contradictory arguments perhaps arose in a general hostility to disciplinary elites, since either way their IT was suspect (either because the money helped or because it made them complacent and lazy; oddly, members of those elites are among the respondents arguing these very points).

So we see that IT changes, that those changes are sharp and continual, that IT responds strongly to monetary support. Indeed, constancy of IT is to some extent seen as impossible or even dangerous. A few disagree, discerning a “true IT” whose underlying rhythm is slower and independent.

Modality

We have seen so far how IT can be known, what are IT’s units, and how IT is distributed in space and time. We turn now to the modality of IT, IT’s positive and negative possibilities. On the positive side are things like unsuspected loci for IT, developing and nurturing IT in persons, the potentiality for IT in all scholars, and the expression of untrammeled IT. On the negative side are the corruption of IT and the manifold means of counterfeiting IT.

The notion that IT can be found in unsuspected places appears in some but not many responses, scattered across the three areas. A humanities association speaks of “institutions [. . .] that have demonstrated research IT and distinctiveness, often with limited investment” <h3>. A social science group tells us “in HSS, where research is cheap, it is very important to recognize what [institutions] are doing to develop research ‘against the odds’ ” <ss2>. Even among natural scientists we hear this belief occasionally. In a biochemical society survey only 34 % of survey respondents wanted the field to take a core and periphery shape, suggesting a strong belief in the periphery.

A more common theme is the potentiality for IT in individuals and the need to recognize and develop it early. Even in the humanities, we hear, assessors must have “familiarity with the work of the departments they are assessing, including the work of younger scholars” <h4>. Among the social sciences, anthropologists fear that “the importance of young researchers is likely to be overlooked” <ss30>. In the natural sciences this issue is a central concern: “The wider research environment [must be] funded on a scale sufficient to ensure that strong people can flourish early in their career, wherever they find themselves” <ns24>. This echoes the earlier complaint that the RAE pressures the young towards non-ITLIKE work, suggesting a surprising vulnerability of those with potential IT.

An occasional undertone in the HSS documents is a belief that in some sense everyone in a discipline can have IT, evident in widespread concerns for fairness. Each institution should be assessed in the same way, says one superb humanities department; “This is the only equitable way of proceeding, but should be sensitive to institutional differences which affect the kind and range of activity” <h8>. Some argue that IT should be measured not in the abstract or absolute, but against the actual aims of a department or group: “Units should be judged on the basis of their own statements of priority research themes” <ss13>. All of these comments breathe a sentiment of IT as a potential in everyone. IT is a matter of achieving what you set out to do. IT therefore lies within the grasp of every group in the system.

A final positive possibility is research unlimited by any constraints—external or self-imposed, financial or intellectual. Throughout, this is called “blue skies research”, a term that did not appear in the invitation document, but that was nonetheless widely used. Both the term itself and its usage identified blue skies research as the ideal of IT. In the humanities, the term “blue-skies” was often made equivalent to “long-term research”. “We have had to become responsive and reactive to invitations from editors and publishers rather than ‘indulge’ in ‘blue skies’ projects [...] the outcome of which will not be assessable for some time” <h20>. Indeed, in a way blue skies stand for disciplinary dominance, as in “The capacity for blue skies research and respect for the primacy of academic values must be maintained” <ss2>. Blue-sky could also mean novel, as in “breadth of representation is important to ensure that blue-sky research or newly developing areas can also be assessed on equal par with more established topic areas within the discipline” <ss9>. One group simply listed the ideal qualities of research: “Novel, entirely blue-skies, flexible, long-term, freely-defined, curiosity-driven research [...] does not fit neatly into this sort of process” <ns1>.

These then are the positive possibilities of IT. But there is equally a negative side. IT can be corrupted. IT can be counterfeited. Moreover, the agents of these things are the colleagues hitherto so positively presented as the only legitimate assessors of IT, the researchers who are all capable of IT, the scholars who will take the long view in order to be IT, and so on.

To begin with corruption. To the ideal of blue skies is opposed the equally widespread idea that IT is driven by incentives and indeed by money. To the ideal of a discipline that controls and dominates the direction of knowledge is opposed the discipline that runs after funding. “Special interest groups [could] get together and lobby effectively to steer money in the direction of their own pet projects thus starving other potentially ITLIKE researchers who have been less quick to see how to play the game” <h9>. The system “will lead to researchers taking ‘safe’ rather than innovative paths in research” <ns3>. “[It] would encourage number-chasing at the expense of genuine value, and originality would be the casualty” <ns4>. Another response spoke of avoiding metrics which “could be seen to follow what was in vogue and for which it may be easier to command grant income” <ns18>.

These statements are not a vote of confidence in colleagues. Quite the contrary, they portray an IT subject to the whims of funders and fashions, driven by modes of assessment that coerce colleagues too weak to resist them. This despair is only worse when we look at the topic of gamesmanship itself. There are so many comments about gamesmanship—an issue which was explicitly raised in the letter of invitation—that one cannot begin to quote them, only to sketch the main themes. The underlying conclusion is that nearly everyone believes that IT can be faked and fairly easily faked. One can pretend that second-rate research-active staff are not really research-active and leave them out of the exercise altogether. One can lump weak staff into a hodge-podge in one unit of assessment in order to look better in the others. One can hire excellent faculty in anticipation of the RAE. One can create what one response called “spurious research clusters” <ss12>. Some responses feared “citation cartels” of various sorts.

Gamesplaying was discussed in nearly all responses. Generally it was seen to be the strategy of chairs, deans, and even vice-chancellors, rather than of individual professionals. It was also seen to be largely optative: If we use metrics, then there will be gamesplaying, and so on. But it was a pervasive fear. Although one’s colleagues might not actually play the game themselves, they would allow it on their behalf. Although the IT of individuals might be relatively pure, that of groups was easily counterfeited. Given the pervasive idealism about other aspects of knowledge, this cynicism about colleagues is doubly surprising.

Content

Our final and central issue is the content of IT. The question “what is IT in research” was posed directly in the letter of invitation, and most responses addressed it.

Most humanities responses define IT as whatever the current experts or panels say it is, “the best work of the current generation”, in the words of one response. Many responses explicitly underscore the diversity of definitions across disciplines, insist on locally-defined criteria, and refer questions of impact largely to impact on disciplinary knowledge or practice. Several also spoke of the evolution and dynamism of definitions, one noting that “traditional publication is out of date by the time it is printed”. This points to the second major humanistic criterion for IT, voiced variously as creativity, originality, or innovation, but often as simple novelty. A few responses mention a third, more general criterion: “advances our knowledge and understanding” or “makes a substantial contribution”.

In summary, IT work in humanities is work that the discipline thinks is new and that perhaps advances some knowledge project, presumably disciplinary. The definition is thus structural and progressive. It is purely formal, having no substantive content whatsoever: nothing about beauty, justice, or other values, nothing about complexity or subtlety of interpretation, nothing about understandings of human existence, the human condition, the human project. IT work is simply whatever the discipline thinks is new and advances the discipline's (unstated) knowledge project.

The social sciences evince much the same pattern. Again disciplinary definition and newness are central attributes of IT. To these is added one new theme, the theme of application in the real world, which among social scientists is mentioned as often as disciplinarity and novelty. And advancement of knowledge is more often mentioned.

Among the social scientists, one response does set a substantive criterion for IT, although rejecting it at once:

> "One useful distinction is between formal and substantive criteria for research IT. Formal criteria (that good research should present an accurate picture of reality and help to develop theory in order to understand that reality) are unlikely to generate controversy" <*ss*17>.

Here at last is a substantive statement; social research aims to understand social reality (note, however, that the comment has the labels of form and substance backwards). But the response then argues that the only audience for social research that is itself stable enough to sustain consistent IT criteria over time is the discipline. Thus at any given time, the disciplines must set the actual criteria for IT: "IT in contribution to future research is thus suggested as a primary criterion of research IT that is most likely to be 'future-proof' ". The respondent thus circles back to IT being simply whatever the discipline says it is.

In the natural sciences, this picture repeats with slightly different emphases. Novelty continues to be central, but applicability in practice now equals it as does advancement of knowledge. Discipline-definition recedes somewhat, but the advancement achieved is clearly understood as disciplinary advancement.

There is, however, another new aspect to the natural science responses. Two entries specifically invoke the criterion of fertility in the production of future disciplinary knowledge. One entry remarks:

> "IT is probably only something which is recognized long after a work is published; and almost certainly not something done especially with a 'research assessment exercise' in mind, since in many areas this simply produces a spate of overinflated and under-prepared publications" <*ns*4>.

Several responses echo this distinction between hugely influential, paradigm-shifting work (and the high risk of attempting it) and routine, everyday science. The responses clearly believe that the former is extremely rare.

Thus across the three areas, the constant theme is that excellence—let us use the word itself at last—is defined by a given discipline at any moment, and that, in particular, it is whatever that discipline will at that time find new and innovative. In some sense, innovation is thought to advance the larger project of the discipline, but there is no mention, with the one exception given, of what this project actually is. In the social and natural sciences, a further criterion of excellence seems to be applicability in the real world. If we reread the humanists' responses with that theme in mind, we will find a few similar claims; for example, "Less quantifiable but no less significant is history's contribution to the quality of the nation's life and the culture of the nation's citizens in general" <h27>.

Theorizing Knowledge Projects

In short, these data taken together tell us absolutely nothing about our original problem. We had hoped to uncover the difference between ideal conceptions of knowledge in the two great families of knowledge—the humanities and their social scientific allies on the one side, and the natural sciences and their social scientific allies on the other. But there are no clear differences. There are, to be sure, some indications of difference. The humanities are more likely to think that excellence is decentralized than are the natural sciences. The humanities also customarily think change in excellence is slower than do the natural sciences. But all the same, the humanities recognize fast change and the sciences retain their faith in the slow pace of major discovery. The natural sciences, finally, are more oriented to advancement of a corpus and to applicability than are the humanities.

But while these differences are interesting, they are both few and unsurprising. What is more surprising is the overwhelming similarity. Most qualities of excellence are shared across the two great continents of academia: from how it is known, to who has it, to where it is, to its continual change, to its transience and potential, to its tendency to corruption, and finally to its identity with the new and the creative.

We underscore two particularly important themes within this broad similarity. First, as we just saw, nowhere in these comments is there anything about the substantive content of excellent work nor, indeed, of work at all. Aside from the brief remark of one social scientist, the projects of these disciplines are all unspecified, even unnamed. Although a broad difference in the knowledge projects of the humanities and the natural sciences remains an intriguing possibility, there is no evidence about those projects in this data, and a distinct suggestion that there is no difference, since the conception of excellences is purely formal at this point in history.

The second particularly noteworthy similarity is the pervasive normative dualism. On the one hand things are good. One's colleagues are those who know excellence. Excellence is protean and tolerance-governed. The potential for excellence pervades the future, the young people, and the research periphery. This is a very hopeful picture. But on the other hand, excellence can be sharply discontinuous and falsely concentrated. One's most noted colleagues are probably resting on their laurels, their current achievements the result of self-fulfilling prophecies. Excellence can be corrupted in a dozen ways, and indeed one's colleagues are at best willing beneficiaries of their administrators' manipulations and at worst game-players themselves.

The broad similarity of responses across the disciplines has three possible sources. While this data cannot adjudicate between them, they do however suggest that one of those theories is more plausible than the two others. These theories involve first the RAE context, second the larger social structure of academic knowledge, and third the cultural structure of communities of scholars.

A first reason for this similarity would be the RAE framework itself. Perhaps that framework forces all respondents into the same stances, even in a data-elicitation setting where we might expect disciplinary peculiarities to figure prominently. The strong and consistent negative themes in this data—above all, the suspicion of peers—probably have roots in the RAE itself, which was a divide-and-rule project aiming to induce academia's cooperation in cutting its own throat. This is exactly the mechanism Michael Burawoy (1979) analyzed in *Manufacturing Consent*, in which higher management secures surplus profit via rules which harness inter-worker rivalries to management's advantage. It is therefore hardly surprising that there should be a kind of tacit collusion against the government 'boss'.

A second possible explanation traces the similarity of responses to disciplinary social structure. In a demographically mature academic employment system, advancing careers require interinstitution mobility, which in turn requires currency convertible across institutions. Only research (rather than teaching or local service) can provide such convertibility, and the desire to manufacture research currency produces a research inflation analogous to any other form of credential inflation. Moreover, the rapid exhaustion of many over-researched fields promotes relabeling, theory-churning, and rediscovery of the wheel in an attempt to produce artificial novelty.[6] On this argument, the similarity of employment and career structure across the two major areas of academia drives a similarity in incentives and behavior patterns that in turn creates the broadly similar research habitus uncovered by the responses here analyzed.

These two arguments can account for much of the broad similarity of the responses across the disciplines. In particular, these arguments seem to explain the negative side of the normative dualism noted earlier. What they don't explain is the positive side of the dualism and the prevalence of unnamed knowledge projects. A third theory would address those findings more directly. It looks not to the social structure of academic communities, but their cultural structure. Indeed, it focuses on the normative commitments of scholars, on patterns of values rather than of social structure.

There are many hints suggesting such a theory. There was for example the considerable idealism uncovered by the analysis of modalities of excellence. Disciplines orient to the future. They worry about transiency and the perpetual need for renovation. They believe in the possibility of excellence among those of low status—the peripheral, the young, the unexpected. And they all use the romantic metaphor of 'blue skies'.

Following these hints, a cultural theory of disciplinary similarity begins with the recognition that the disciplines evolve in a world that is simultaneously one of

[6] For an extended analysis, see: Abbott (2001).

scarcity and of abundance. Because our social theories have roots in political economy, the former of these is much more familiar to us. We are used to thinking about scarcity, both in social structural terms (PhDs competing for scarce jobs, interdisciplinary competition for funds and talent, division of labor) and in cultural terms (exhaustion of research areas, declining productivity of paradigms, etc.). All of these things we understand under the sign of scarcity.

Yet abundance may be more important to the life of disciplines than is scarcity. Specialization arises more from the overwhelming complexity and extent of knowledge than from open competition over scarce topics. The glut of knowledge on the internet echoes an earlier glut that has existed since the 1960s if not the 1920s (Abbott 2011). If we consider the various oppositions that pervade the social sciences—positivism versus interpretation, narrative versus analysis, and so on—it is clear that these seven or eight distinctions can be crossed to generate dozens if not hundreds of possible "disciplinary positions", only a handful of which have actually been tried as disciplines (Abbott 2004). Not only is our knowledge of the world immensely diverse, we have also only begun to explore the different ways of producing such knowledge.

When we social actors are confronted with abundance, we employ a number of strategies. The simplest is to abandon ourselves to randomness—the strategy of many surfers of cable TV and the internet, as it is of that school of Bible readers who "let God choose" what they are going to read. Another strategy is to subdue abundance through organization, subdivision, indexing, and the other strategies of mapping and abstraction. These enable us to ignore certain areas altogether: "I'm not good at mathematics", "I don't like to travel in countries where I don't speak the language", etc. Along with this selective ignorance goes specialized knowledge. Organization permits us to know much about some areas even as we know little about others, except as they are fractally related to areas we know well (Abbott 2001).[7]

In between these strategies of selective ignorance and specialized knowledge is that of canon—the somewhat arbitrary selection of certain particular things and people as emblems, representatives, or paradigms. Hence Max Weber dominates our social theory courses even though many of the ideas now attributed to him were borrowed from others. Weber-as-canon cancels the obligation to read each of the dozens of slightly different but equally distinguished theories of society. Yet another control strategy is the screening of abundance for quality or authenticity or righteousness or some other characteristic that justifies selective ignorance within a particular organized field. So we decide that only books published by this or that press are worth reading. Or only women can write gender theory. Or only quantitative analysis can properly identify the impact of social class on education. Although these statements all have their theoretical justifications, it may well be that their main function is actually to reduce the amount of work to be read and comprehended.

[7] Note that this mechanism could explain division of labor as well as does the scarcity/competition argument favored by Durkheim and many others.

All these various strategies turn abundance into scarcity, which we understand much better. For if only certain presses are worth reading, we can have a competition for who gets to publish with them. If only quantitative analysis is worth doing, we can have a competition to find the best form of quantitative analysis. If each discipline knows only certain things, then we can have a competition between them for resources, students, and so on. We find all of this not only more comprehensible, but also right and proper—things as they should be.

In summary, the disciplines may well originate in an attempt to constitute coherent knowledge communities in the enormous chaos of things to be known. Far from being a division of labor forced by intensive competition in a crowded turf, they are in fact like small communities of whales in the immense ocean, banding together for companionship. To that end, they select canons of common work. By agreeing to restrict "classical sociological theory" to Marx, Durkheim, and Weber, sociologists create a common set of language, allusions, problems, and methods. Similarly they organize themselves into subdisciplines, ranked departments, elite publishers and journals, and so on, all in order to recreate a more familiar world of scarcity amid the frightening abysses of abundance.

Relations between the disciplines so constituted are maintained by "interdisciplinarity". Those who have practiced interdisciplinarity know that disciplines comprehend each other only with great difficulty, for the process of canon-formation and structuration through which disciplines create intellectual coherence leads to quite idiosyncratic habits. Like all clan systems, therefore, the academic system has kinship rules. These are the inverse of normal kinship rules in modern societies. The full children of interdisciplinary collaboration are outcasts from the core system and usually become parts of the hybrid, problem-driven world of application. (Occasionally, they are part of new discipline formation.) By contrast, the products of what we might call the morganatic form of academic marriage—the extensive borrowing of a set of ideas, canonical works, methods, etc., from a discipline by a member of another discipline—become legitimate descendants of the borrower's discipline, which thus does not have to breach disciplinary boundaries.[8] These rules have kept the disciplines continuously in existence over a long period as relatively independent lineages but at the same time prevent their ever completely losing contact with one another.

The creation of a reassuring scarcity via disciplinization (a cultural structure in the ecology of possible knowledge) in turn produces the social structures of internal status hierarchies that drive the careerism noted throughout this data. These hierarchies are dominated by "nearness to the center" of the discipline. This center is indicated by the (arbitrary) canon of works, methods, theories, and so on. In any given short run, this canon is reified into a fixed hierarchy that enacts the scarcity system that provides the incentives and directions that push careerism.

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[8] On the argument made here, boundaries are not so much about conflict with competitors as they are protections against the leakage of scholarship into the endless chaos of "all possible knowledge". This is precisely the reverse mechanism of that posited in Gieryn's celebrated article (Gieryn 1983).

But participants know this fixed intellectual structure to be a reification and that the inevitable biographical succession of leaders will with equal inevitability change it. Indeed, the actual 'center' of the discipline is the set of rules deployed in making these changes. These process rules define what makes work 'interesting', 'excellent', or whatever we wish to call it. They may include universal rules. In the sciences, for example, excellent work must subsume most earlier knowledge, as Whewell argued long ago. The process rules may also include "reflecting barriers". In literary studies, the cry against "excessive scientism" has emerged many times in the last century and a half, always pushing the discipline back towards literary appreciation (Graff 1987). But they also include quite local, particular desiderata. That is, there is not any one excellence for all times and all places, even in a single discipline. Excellence must always be judged dynamically and locally, in relation to the particular moment and particular intellectual setting from which it aims to move.

But this means that excellence has no permanent qualities beyond novelty. It cannot be given a particular name, any more than in universal religions God can be given a particular name without losing the limitless, infinite, transcendent quality that is God-ness. So it is little surprising that scholars commenting on the RAE would not mention, even in the most cursory way, the underlying projects of their disciplines, even though the comments were a place for bold claims and grand gestures. To give a name to a discipline's project is to set limits on it, to reduce its infinite possibility. It is also to concede to others the right to define the discipline's future direction, when in fact that direction is made in the performance of research.

Put another way, "discipline as a cultural system" is what we get when a group of scholars loosely agree on a general way to move through the universe of things to know. They teach their recruits how to decide, from any given particular position in that universe, what are excellent next directions in which to proceed. With such rules, the recruits will have guidance even when the original leaders are gone.

In this argument, 'unnaming' arises because at the core of each discipline is a process rule, not a picture of some particular ideal. Such a picture would lead to dangerous reification. It would substitute for the boundless future of the discipline a fixed core of worship. As countless religious and revival movements show, this would be fatal to survival. At the same time, we know from the theory of Gibbs sampling that it is quite possible to construct algorithms that are perpetually iterative, but that tend to circle around a given area. That is, the fact that the disciplines invent their futures in part by locally-derived process rules does not in any way condemn those disciplines to purely random wandering. But it does mean that the rules for novelty and excellence at one point in disciplinary time and space are not likely to be the same (except in the most purely formal sense) as those of another. Hence there is no way to specify them once for all, either for the RAE or for anyone else.

This then is the heart of tacit knowledge (Polanyi 1958). It is the knowledge of how to move forward from where we are right now, of how to combine long-run aims and short-run paradigms to drive a judgment of local novelty.

My paper therefore must end on the brink of a new theory of knowledge institutions. We sought the different projects of the humanities and the sciences. Yet we found they thought about their projects quite similarly. On theoretical reflection, this seems to be caused not by an actual similarity, but by the fact that the cultural unity of the lineages that are disciplines arises in a set of continuous recipes for generating novelty. These bring together long-run phenomena like canons and methods and short-run phenomena like research paradigms and trendy topics. If these recipes have some enduring qualities, they do not have any particular identity with one particular end point. It is no surprise then that the RAE did not find IT.

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Chapter 9
Lessons Learned from the Study of Multi-organizational Collaborations in Science and Implications for the Role of the University in the 21st Century

Ivan Chompalov

Abstract The chapter addresses forms of research which engage teams spread over one or more institutions. The practice to pool 'brains' has become evident since WWII; the subsequent fashion to build 'science parks' followed this tradition; and other forms of pooling became necessary when research depended on costly infrastructure.

The chapter reports on four types of research cooperation: bureaucratic, leaderless, non-specialized, and participatory. While the 'participatory' category appears to dominate in the field of particle physics, the remaining three categories cover cross-disciplinary endeavors. The more formally organized and tightly managed projects were found in the field sciences (e.g. space science, geophysics), while small, more informally organized and more loosely managed projects are more common in the laboratory sciences (e.g. materials science). No association could be established between size and perceived success of the collaboration.

Modern universities are generators of new knowledge and human capital. In the context of Big Science it is becoming increasingly difficult to produce cutting-edge research and new technology without collaboration that often involves an institutional network of a variety of organizational actors: funding agencies, academic departments, national laboratories, corporate laboratories, non-government entities, and private donors. In short, scientific and technological collaborations, especially those involving multiple institutions as well as substantial resources, have turned into powerful engines that drive the expansion of research universities and impact on the society at large.[1]

[1] A great deal of attention has been devoted recently to the changing modes and organization of R&D, including the mode of interaction between academia, industry, and government, which has gained currency as the "Triple Helix" model (Leydesdorff and Etzkowitz 1996, 1998; Etzkowitz and Leydesdorff 2000; Leydesdorff 2005). The model explicitly tries to explain knowledge-based

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Research on Multi-organizational Scientific Collaborations

Such collaborations became more common in the 1970s and their size started growing as well. The history of particle physics after the 1950s, for instance, illustrates that expansion in the number of researchers on a project also expands the number of involved organizations. Thus, a typical bubble chamber collaboration at CERN in the mid-1960s consisted of about fifteen physicists. One decade later the number of researchers working cooperatively on CERN's largest bubble chamber, Gargamelle, was about 50 people from seven organizations. In 1985 the Delphi collaboration, working with the Large Electron-Positron Collider at CERN involved over 350 high energy physicists from 37 organizations in 17 countries (American Institute of Physics 1992).

The field of particle physics was not alone in this postwar development. Ionospheric physics developed as a subfield of geophysics involving large-scale research of a collaborative nature that received particular impetus during the International Geophysical Year (1957–1958). Astronomy, as part of space science, had been a largely co-operative effort involving significant technological facilities for decades before the age of government largess (Tatarewicz 1990). Oceanographic researchers began to utilize large collaborations of multiple organizations in the 1960s and 1970s, including five of the best-known multi-organizational projects—CLIMAP, MODE-1, GEOSECS, CEPEX and MANGANESE MODULES (Mazur and Boyko 1981). Currently, the home page of the US National Science Foundation includes in its list of disciplinary programs a link to "Crosscutting and Interdisciplinary Programs" that encourage scientists to draft multi-organizational proposals.[2]

There is an extensive literature on scientific collaboration both within and across sectors and a variety of often disparate approaches. One popular and established approach is entirely quantitative. Bibliometric studies use the public evidence of research activity—most often papers and reports, patents and agreements—as indicators of trends and processes. Co-authorship is typically employed as an indicator of collaboration. Because scientific journals are specialized by field and because journal articles include the organizational affiliations of the authors, bibliometric studies can determine trends in collaboration across nations and across areas of science. Studies have generally shown that multi-institutional as well as international co-authorship has increased over time. From the early 1970s until the early 1980s the

innovation systems by examining network overlays of communication among the three sectors. These frameworks operate on a macro-level of analysis with an emphasis on technology transfer, innovation, and enhancing the economic applicability of scientific research. Just like Ben-David's focus was narrower, primarily on academic science and centers of learning that could preserve the relative autonomy of science and mitigate the hazardous tendencies of politicization and commercialization of science, most of our projects represented—and focused on—academic experimental and basic research. One of the few exceptions is the first case study in this chapter (the Center for Nondestructive Evaluation—a NSF-funded Industry/University Cooperative Research Center), which was conducted somewhat later and fits well in the Triple Helix model as an example of a particular innovation based on the interplay among academia, industry, and government.

[2] See: www.nsf.gov/home/crssprgm/start.htm.

proportion of internationally co-authored papers doubled (Luukkonen et al. 1993). Measured as a percentage of all co-authored articles, international co-authorship increased globally from 17 % in 1981 to 29 % in 1995 across all countries and fields. Not only is international collaboration increasing, but inter-sectoral collaboration has grown as well. About one quarter of all papers published by academic authors involved co-authors in another sector, compared with 20 % in 1981.

While bibliometric studies have their limitations, inferences from organizational affiliation can be relatively sophisticated. Kundra's investigation of Indian medical sciences in the first half of this century (Kundra 1996) distinguishes between intra-departmental collaboration (within the same department), inter-departmental (between departments within the same institution), inter-institutional (organizations within the same country), and inter-national collaboration (organizations in different countries). The main deficiency of bibliometric research on scientific collaborations is that it just measures outputs, but not the important underlying processes of formation, organization, evolution, and consequences of these collaborations.

Qualitative research on scientific and technological collaborations occupies the opposite end of the spectrum. Carried out primarily by historians, ethnographers, anthropologists, and qualitatively-oriented sociologists, case studies of single or a small group of collaborations provide richness of detail and track the internal dynamics of a collaborative project, but lack reliability and generalizability. Historians of science have provided extensively documented narratives of the development of accelerator laboratories that host particle physics collaborations and recounted the stories of individual collaborative experiments (Krige 1993; Galison 1997). Anthropologists and sociologists have observed collaborative research projects and provided important interpretive tools (Traweek 1988; Zabusky 1995; Collins 1998; Knorr Cetina 1999).

Case studies share a micro-sociological focus, a qualitative methodology, a cultural-anthropological or narrative orientation, and owing to the research intensity required by the approach, an emphasis on single organizations, centers, or projects. Their strengths are in providing theoretical guidance, identifying social processes, and raising questions about important organizational and cultural dimensions. But when the findings of case studies are contrasted, they display such diversity as to defy generalization. They have several weaknesses and shortcomings, for example an inability to provide a systematic assessment of the relative importance of one process over another. Factors such as communication, the division of labor, work as a process, technology, negotiation, and size are all 'crucial' to the scholar who discovers their importance, but little attempt is made to show why some factors may be more important than others. Furthermore, it is not clear whether the collaborations that have been studied are representative. To what extent are the findings of case studies generalizable? Is an observed relationship unique to a particular collaboration or is it a pattern characteristic of most?

We argue that, while valuable, both bibliometric and case studies are limited. To compare collaborations across specialties and to reach empirically based conclusions about their characteristics, evolutions and outcomes requires the analysis of collaborations from several areas of the sciences in a systematic fashion. The colleagues with whom I worked[3] and a number of other sociologists, historians and archivists of science conducted such a study; it was sponsored by the American Institute of Physics and funded by a plethora of funding agencies, and adopted a 'meso-level' perspective (Shrum et al. 2007). This ambitious undertaking was carried out in three phases that took approximately ten years to complete (from 1989 to 1999). The first phase, through the early 1990s, was devoted to the study of particle physics. Phase two examined space science, geophysics, and oceanography—all field sciences with long traditions of teamwork (American Institute of Physics 1995).

In both phases, we interviewed between five and fifteen members of particular collaborations, selected to cover a range of scientific styles and conditions. Through 1995 approximately 500 interviews had been conducted, focusing on the history and organization of these collaborations, their technologies, management, and outcomes. In the third phase, through the late 1990s, twenty three additional collaborations from five new specialties were included: ground-based astronomy, materials science, heavy ion and nuclear physics, medical physics, and computer-mediated collaborations (American Institute of Physics 1999).

The comparison of the final sample of 53 collaborations in the physical sciences enabled us to characterize types of collaborations and to assess the importance of structures based on their connections with processes or outcomes of interest. Far from 'everything' being related to 'everything else', there are relatively few patterns that emerged. Often the interactions among several organizations or several sectors produce multi-organizational collaborations (those that involve more than two separate organizations). We conducted an empirical analysis of approximately 600 semi-structured interviews, conducted over a ten-year period with scientists, engineers, project managers and graduate students from these 53 collaborative projects in a variety of areas of physics and allied sciences. We thoroughly examined the structural dimensions of these collaborations (formation, magnitude, organization, technological practices) and their outcomes (success, trust, and conflict)—as well as the relationship between the two—were thoroughly examined. We employed cluster analysis to derive the typology of collaborative projects and cross-tabulation and one-factor ANOVA—to test for significant relationships. Among the most intriguing findings are the following (Shrum et al. 2007):

- The twin imperatives of bureaucracy and technology and achieving a healthy balance between the two are essential for collaborations to endure and succeed. Mediating, harmonizing or, in the best case scenario, creating a positive synergy between the two is the essential drama in the evolution of most collaborations.
- The expense and scarcity of certain instruments are the most prominent features that renders inter-organizational collaboration necessary. For certain fields, such as contemporary experimental particle physics, there is no practical alternative to collaboration and there are few models of how to do science except in a collaborative context.

[3]Wesley Shrum and Joel Genuth.

- Trust, though viewed as a desirable form of relationship among collaborators, does not turn out to be associated with self-reported success. It is, however, inversely related to conflict. Conflict, in turn, has no relationship with success. Some of the most successful collaborations in our sample were conflict-habituated during crucial periods of project evolution. Thus, under certain circumstances, disagreements and even conflicts could be stressful yet intellectually stimulating.
- Contrary to common sense, the beginning of a project may have more to do with its success than its end. Collaborations that struggle to acquire resources, with higher levels of uncertainty in their formative stages, are viewed as more successful. So are international collaborative projects that involve "jumping through a lot of hoops" before they take off.
- Much history and sociology of science and technology has called attention to the importance of disciplinary traditions and communities in the practice of research. On the basis of the information gathered and analyzed in the study of inter-organizational scientific collaborations, we were most impressed with the irrelevance of discipline or field of research to multi-organizational collaborations. The lone exception in the two typologies (one in terms of project formation and the second in terms of organizational style) of multi-organizational collaborations resulting from cluster analysis was particle physics, which dominated the participatory type of collaboration.
- When examining how multi-organizational scientific collaborations get started, we discerned several distinct patterns. From this diversity of agents, organizations and motivations, we identified five main processes of formation. The first was characterized as 'conventional', since it involves academics who previously worked together and who undertake a project that is too large and complex for a single team of scientists. An initiator may communicate the need to "wake up and smell the coffee". A second process is similar in its relative homogeneity and conventional acquisition of funds, but these projects coalesce around participants with little prior history of working together. Often the context is one of significant struggle ("Damn the torpedoes!") and these projects are less likely to have a single initiator. A third variety emerges from a group of scientists with pre-existing relationships but no obvious source of support. They face pressures from their employers, sometimes opposition from fellow employees, but manage to "get out of the rut" with a project that often creates a new facility or a novel alignment of scientific interests. The fourth process, like the second, involves participants without pre-existing relations, but these collaborations are assembled through impersonal selection processes and external brokerage. Finally, in the "collaborators wanted" model, scientists with a shared vision bring together others with valued competencies in a conventional resource framework. Participants need not have worked together before and they may come from different sectors. These are typically the collaborations of particle physics. Usually, their participants take collaboration for granted.
- Projects that were initiated by universities, as opposed to government laboratories or corporate laboratories, or those that had a more substantial university component, tended to be more bureaucratically organized.

To study the organization and management of scientific collaborations involving multiple institutions, we focused on the concepts of bureaucracy and interdependence. 'Bureaucracy' itself is often used as an undifferentiated concept that combines a multitude of organizational aspects. For scientific collaborations, we operationalized bureaucracy in terms of formalization, hierarchy, leadership, and division of labor. The analysis showed that claims for scientific collaborations as informal, free-wheeling formations, without hierarchical structures or clear leadership, that utilize strong communitarian organization are only partially true (Krige 1993; Zabusky 1995; Knorr Cetina 1999). Generalizations about the essentially informal and collective social organization of collaborative projects in science are often based on a narrow analysis of high-energy physics. Our thesis of particle physics 'exceptionalism' rejects the extrapolation to scientific collaborations in general.

Cluster analysis revealed four types of scientific collaborations: bureaucratic, leaderless, non-specialized, and participatory. The last category is dominated by particle physics and is the only field-specific type. If anything, particle physics collaborations are atypical. Since few projects from other areas of physics and allied sciences share common features with particle physics, their marked 'egalitarianism' must be considered exceptional. They are more likely than other fields to endorse strong collectivism and consensus in decision-making. At the same time, they tend to be run less bureaucratically, with fluid organizational structure, fewer levels of authority, and infrequent formal contracts. Both qualitative and quantitative analysis showed the utility of distinguishing between two kinds of formality: administrative and scientific. These two types are unrelated to each other. For example, the pattern that emerges in particle physics experiments is that they tend to exhibit informal administrative/managerial structures, but retain tight control over research, data acquisition, and external communication of results.

Another feature of "particle physics exceptionalism" is that these experiments typically have no lead center, but always have a host organization—the accelerator site. The latter is specific for high-energy and heavy-ion physics collaborations due to the limited number of facilities and the enormous costs of building and operating particle accelerators and detectors. Thus, particle physicists are forced to collaborate. No single institution can afford to build, maintain, and operate such expensive facilities. The more informal organization of multi-institutional particle physics experiments can at least partially be attributed to their long tradition of cooperative research (Knorr Cetina 1999; Krige 1993; Galison and Hevly 1992), the well-established funding pattern, and the greater monodisciplinarity of the field as compared with materials science, medical physics, or geophysics.

Most inter-organizational projects do not mirror the structure of those in particle physics, but vary substantially across fields in terms of organizational and managerial arrangements and styles. Except for particle physics, which is overwhelmingly participatory and non-bureaucratic, the membership of the other three types proves to be cross-disciplinary. The juxtaposition of the four types of collaboration indicates the importance of organization for the acquisition of instrumentation, the analysis of data, and the communication of results. The most salient connection here is between the character or structure of the collaboration's leadership and the character or degree of its interdependence. The more integrated a collaboration's data acquisition, the less meaningful are the independent interests of the member organizations and the less likely the collaboration is to be highly formalized. Particle physics experiments routinely coordinate the parameters of the instrumentation they employ to acquire data and then integrate the data streams from experimental components. And particle physicists have committed themselves and their organization to experiments with no more formalities than signing proposals and then, when an accelerator laboratory so requested, signing memoranda of understanding that specify the division of labor the collaborators had already determined. Rarely are their participants concerned with defining and protecting the interests of their employing organizations. If we leave aside variations in detail, the overall pattern that emerges for most collaborations is "hierarchy within consensus" rather than "consensus within hierarchy".

The four patterns of organization, as well as the linkage between the management and technological interdependence of the constituent research teams, can benefit science policy makers, program managers and scientific leaders, given the crucial role that public science plays in technological innovation, transfer, and industrial development; the fact that a growing portion of publicly funded research is carried out in collaborations; and the growing number of legislative acts that stimulate R&D collaborative endeavors, particularly in the US (Chompalov et al. 2002).

These and other interesting findings have important implications for the research university of the 21st century, such as the need to reorganize the current rigid disciplinary structure and make it more attuned to trends of the future like interdisciplinarity and collaboration; the necessity for a more flexible and efficient allocation and reallocation of resources; the need to encourage cyberspace creative networks; the imperative to build inter-sectoral bridges; and the need to strike a better balance between research, teaching, professional development, and outreach or service to the community, among others.

Research intensive universities are the heart and soul of innovation, discovery, and entrepreneurship. Typically, they are a small fraction of the organizations involved in these processes but play a crucial role. For example, in the United States there are roughly 4,400 institutions of higher education, but just 150—or a bit more than 3 percent—are classified by the Carnegie Foundation as "research intensive". However, they increasingly fuel R&D and the economy by accounting for the bulk of discoveries and innovations, as well as preparing a disproportionately high number of leaders in all sectors of academia, business, industry, and government. To a large extent this happens through intensive intra- and inter-organizational collaborations, be that solely scientific collaborations or innovations, partnerships, alliances and collaborative networks that span science, technology, and industry.

Two Case Studies

To illustrate the importance of multi-organizational scientific collaborations for the growth and impact of the modern research university—and in light of the major findings from the systematic study of these collaborations in the 1990s—I shall now take a closer look at two case studies. In 2000–2001, I was actively involved as a postdoctoral research fellow in the Research Value Mapping Program at Georgia Institute of Technology in studying a variety of high-end collaborative arrangements in research-intensive universities across the United States, and thus became familiar first-hand with the wide-ranging beneficial impacts of these powerhouses of future scientific and economic growth. The aim of the program was to increase our understanding of the success and productivity of collaborative S&T research by focusing on research value mapping and research value alliances (Rogers and Bozeman 2001). Valuable outcomes from such collaborative ventures can be linked to participation customs, networks, and innovative social structures. Such outcomes can include project outputs (articles, patents, awards, citations, etc.), project graduates, the building of scientific and human capital capacity, network ties with industry, government, and universities, and similar outcomes.

The majority of these projects, some of which of a fairly long-term nature, were NSF-sponsored Science and Technology Centers, but there were some new formations actively promoted by the major national funding agencies, such as the case study below.

Case Study 1: Center for Nondestructive Evaluation[4]

Technical Focus and Center History The Center for Nondestructive Evaluation (CNDE) at Iowa State University was the only instance of an NSF Industry/University Cooperative Research Center (I/U CRC) in our sample. From an organizational and public policy viewpoint, it is a single-institution, multi-disciplinary, sponsor driven, and pre-competitive arrangement. Substantively, the center is engaged in research leading to quantitative nondestructive measurement techniques.

From the outset CNDE was envisioned as an establishment with a more applied focus. The idea for a center originated at Ames Laboratory[5] where a core of researchers whose primary expertise was nondestructive evaluation and nondestructive testing had assembled. Two external developments sped up the formation of the Center. The first was that in the early 1980s the Air Force and DARPA[6] funding for nondestructive research and applications at Ames began to dwindle. The second was that when NSF started the I/U CRC program, it became increasingly obvious to the core group of researchers at Ames that the I/U CRC program was a good

[4]We conducted a site visit of the Center on August 25, 2000. During the visit a total of 15 interviews with principal investigators, postdocs, graduate and undergraduate students were completed, including an extensive oral history interview with the director of the center, R. Bruce Thompson.

[5]Ames Laboratory is a government-owned, contractor-operated research facility of the US Department of Energy that is run by Iowa State University (ISU).

[6]Defense Advanced Research Project Agency; DARPA is the research and development office for the US Department of Defense.

mechanism to further develop nondestructive evaluation technology, transfer it to industry, and develop the necessary human capital as a mechanism of transfer. So, a proposal was written and the center was officially opened in 1985. It was created with funding predominantly from 14 industrial sponsors and to a lesser extent from NSF.

The center is heavily involved in applied research. At the time of the study, it collaborated with about 24 companies including Boeing, GE, Siemens, Westinghouse, Pratt & Whitney, Sperry, Honeywell, Caterpillar. The major applications of CNDE research are in the aerospace industry, but also in car manufacturing, agriculture, animal science, and the biomedical sector. For the past several years, the center R&D activity has focused on seven broad projects or problem areas: engineering applications of ultrasonic measurement models; eddy current, thermal, and optical techniques; radiographic techniques; magnetic techniques; signal processing, pattern recognition, and AI; nondestructive evaluation (NDE) for material properties; and NDE for composites.

Two very successful companies have been spun-off from the center and CNDE has had a substantial impact on the development of local, mainly small businesses. Thus, in some ways it is similar to the University of Michigan Center for Ultrafast Optics. There is a visible international dimension, more specifically focused on the creation of the World Federation of NDE Centers which was initiated by CNDE at ISU. This is also reflected in the staffing mix with both faculty and graduate students coming from a variety of countries (England, China, Japan). There is a strong educational component which spans all levels (K-12, community college, university graduate and undergraduate). This is also one of the channels for recruiting students. The effect on careers has been noticeable. In recent years several strong faculty recruitments have positioned CNDE as probably the strongest and largest such center in the world. The impact on student career paths has been mainly in preparing students for good jobs in industry, since the focus of the center is on hands-on experience in nondestructive evaluation and testing. The only drawback, and it is a slight one, is that most students are too narrowly trained and, thus, are being prepared for somewhat specialized careers.

Scientific Impacts As a world leader in the development of new NDE measurement techniques and interpretive models, CNDE[7] has had a substantial overall impact on the development of the fairly narrowly specialized and small research area of nondestructive evaluation and testing. The scientific impact has been an accumulation of advances in the knowledge pertaining to NDE and NDT rather than a single, breakthrough discovery. Important strides have been made in all seven problem areas. Among the most outstanding achievements of center researchers are

[7] We have focused on the quality of accomplishments and impact of CNDE rather than on the quantity of the Center's output, which was in itself impressive, given the small size of the collaboration (just 25 collaborating professional researchers and one institution). Over a fifteen-year period, the collaboration published over 600 articles, registered 34 patents, and led to the establishment of several spin-off companies.

the development of models and simulations of ultrasonic and X-ray inspection and measurement; research on pulsed eddy current inspection; novel ways of image processing; techniques for detection of corrosion and buried layers; the elaboration of magnetic techniques for addressing inspection problems.

Technical Impacts CNDE has been inherently involved in technology development and application of various techniques for NDE and NDT. The center has created a number of software tools in order to predict the results of an inspection. Several of these have been commercialized (the X-ray simulation software XRSIM; the ultrasonic inspection technique). CNDE has patented over 30 inventions and has had a number of licenses sold to various industrial companies. In addition, the center has been heavily involved in technology transfer both of software and hardware. Among some of the highlights in technology development have been the creation of enabling technology to establish the Federal Aviation Administration (FAA) Center for Aviation Systems Reliability in 1990 and the facilitation of the founding of the Engine Titanium Consortium in 1993 through the use of enabling technologies from CNDE.

Relations with Industry The relations of CNDE with industrial partners have been both active and pervasive, as might be expected given the center's mission. Linkages have been set up at all levels of interaction (client-sponsored research, technology transfer, technical assistance, licensing, outreach). Center research has had wide applications in the fields of aviation, transportation, energy, manufacturing, agriculture, and recently in the biomedical sector.

Education and Outreach Outreach and education have been outstanding dimensions of CNDE activity. A variety of programs have either been initiated or administered by the center. One major outreach effort at the state level was the establishment by CNDE of the Iowa Demonstration Laboratory for NDE Applications, which serves as a resource to small and medium-size manufacturers through demonstration of NDE technologies, educational seminars, and application guidance. Other outreach milestones are the facilitation of the formation of the World Federation of NDE Centers and the biannually published newsletter "CNDE News", which reaches an estimated audience of 3,000 readers. CNDE has been actively engaged in different forms of education at all levels. Among its notable achievements have been the establishment of an NDE engineering minor at ISU; the administration of the "NSF—North Central Collaboration for Education in NDE/NDT" program; and web-based educational programs in NDE for community colleges and for K-12.

Student Careers The major impact on student careers has been the training they received in both theoretical and especially hands-on research in NDE and NDT, which has positioned them well for jobs in the area. The bulk of graduate students have gotten positions in industry and government laboratories. CNDE graduates can boast placements in some of the biggest and most prestigious companies (e.g. Boeing, Microsoft, Texas Instruments, McDonnell Douglas, General Electric). A specific feature of the way the center prepares students for the labor market is that most students are somewhat narrowly trained and thus better equipped for specialized careers.

Collaboration Research collaborations have chiefly involved interdisciplinary joint projects among CNDE participants from different Iowa State University departments. However, the most prominent aspect of collaboration has been the cooperation between the center and industrial companies or other universities and colleges on various ambitious programs. Thus, CNDE has collaborated with Ohio State University in setting up and running the FAA Airworthiness Assurance Center for Excellence. CNDE also participates in the Engine Titanium Consortium, which brings together researchers from ISU and engineers from GE, Pratt & Whitney, and Allied Signal to develop and transfer inspection technology. Finally, CNDE collaborates with a number of community colleges by providing them with training tools and by facilitating student transfer.

Case Study 2: Center for Biological Timing[8]

An example of a typical and popular form of doing collaborative research that has been funded by NSF for quite some time is the Science and Technology Center (STC). The majority of these formations have been extremely influential in pushing forward the boundaries of scientific and technological knowledge and will undoubtedly continue to do so in the 21st century. Below is a description of such a center that was studied in 2001.

Technical Focus and Center History The Center for Biological Timing (CBT), which formally became an NSF Science and Technology Center in 1993, is an instance of a multi-organizational, multi-disciplinary collaboration to study and solve major problems in biological timing. Its precursor was the so-called Biodynamics Institute, which was recognized as a University of Virginia (UVa) academic enhancement program Center in 1989. Its intellectual focus was initially just on biological clocks, but by the time the first proposal to NSF for funding as a STC was submitted (1991), the Center had extended to deal with all forms of oscillations. The initial proposal, submitted only from UVa, received positive reviews but was not approved. The core group of researchers came back in 1993 and applied again. This time the proposal included Northwestern University and Rockefeller University to cover the molecular aspects of biological timing and the Center was funded. Over the years the Center changed, as it added formally Brandeis University and then informally Scripps Research Institute, which at the time of the study collaborated with CBT but was not a formal member.

[8]We carried out a site visit of the Center for Biological Timing on April 21, 2001 and interviewed 3 principal investigators, one postdoc and one PhD-student, including a thorough interview with CBT's director Gene Block. We also relied on the analysis of documents and articles that the collaboration had generated.

Initially, CBT had a somewhat loose idea of what it would do, but was forced during the first year review by the Advisory Board to become more focused. Consequently, the center decided to develop an overarching vision and cluster research activity around a core project, which was termed the Clock Genome Project and sought to isolate novel clock mutants in *Arabidopsis, Drosophila*, and mouse. In a nutshell, the concept for the core project was to have biologists, generating mutants, then a molecular group, characterizing the mutants, then physiologists, looking at how the brain changes, then endocrinologists, examining how the biological clock affects the circadian clock, i.e. a 24-hour clock that affects the frequency of the oscillation. This sequential multi-disciplinary effort was described by the director of CBT in the following way: "So, the whole Center was like a factory. You are building these mutants and then trying to characterize them. Apart from that, you have other projects, but that was the main focus."

The Center for Biological Timing rode the recent wave of biological timing establishing itself as one of the hottest scientific fields in the past decade. CBT has been extremely productive—members of the Center have published over 40 articles in Science during the lifetime of the Center until 2000. Its remarkable success was probably a combination of singleness of focus almost from the outset (The Clock Genome Project), enough resources, and scale. Constant long-term funding and giving CBT largely a free hand to choose the direction of their research are additional contributing factors. CBT has made a tremendous contribution to the field of biological timing not only scientifically but also by providing a valuable service to the expert community by organizing conferences and meetings. The immediate future of CBT, despite its smashing success (as proved, for instance, by articles in *Science* and *The Chronicle of Higher Education* in 2000 reporting groundbreaking CBT research) is unclear. At the time of our site trip, UVa was applying to NIMH for a new center (in essence an extension of CBT), but the prospects for funding seemed uncertain.

Scientific Impacts The scientific impact of CBT[9] has been profound and far-reaching. The most significant scientific discovery was the identification of the biological clock genes. Specifically, CBT researchers discovered 5 additional genes, among them the first mammalian clock gene. The second big discovery, although of a lesser magnitude, was the link of the physiology to the molecular genetics of biological clocks in mammals. CBT developed a technology that was able to report routinely the duration of the mammalian neurons and found that when you do genetic mutations on the cells they behave differently. A third breakthrough was the understanding of the frequency modulation of the gene expression.

[9] Just like the previous case study, I emphasize here the caliber of the accomplishments of CBT researchers, rather than the raw number of various outputs. Again, for a small collaboration with only 29 participating researchers from four different institutions, the sheer volume of publications was impressive—165 peer-reviewed articles over a two-year period.

Technical Impacts Since the focus of center activities has been almost entirely on fundamental research, there has not been a significant impact in terms of new technology. The lone exception here is the work on the Technology Development Project and the application of two effective techniques: (i) using luciferase as a reporter gene expression in the cells of transgenic mice; (ii) taking advantage of the jellyfish color protein to look at protein-protein reactions.

Relations with Industry Because of its primary goal of developing basic scientific knowledge, CBT has not been engaged visibly in commercialization. Nevertheless, some encouraging relationships with industry have been initiated. One of them involved a big molecular biological firm, Promega, which was interested in using luciferase as a reporter gene. Due to some legal complications, the company eventually distanced itself from CBT. The other was a more successful relationship with a steel company, in which CBT researchers were trying to find out how the biological clock relates to accidents during shift work. At the time of our site trip, CBT was in the process of talking to some companies about doing a study of oscillations in driving.

Education and Outreach CBT has tried to integrate outreach into their mission at the suggestion of NSF. It has been largely successful in developing an active outreach program, which encompasses The Remote Access Online Real-Time Science Experiment, the dissemination of a Biological Timing Tutorial CD ROM, and the Undergraduate Summer Research Experience program, among others. The CBT has also been providing a valuable service to the expert community by organizing conferences and meetings, including various international workshops.

Student Careers The impacts on graduate students have been mainly to prepare them for the hot job market in the promising field of biological timing. They are well positioned to establish a successful career. However, getting postdoctoral fellows has been a problem, chiefly because of reluctance on the part of NSF to fund them with Center grant money. The Center, however, has managed to find subtle ways to keep graduate students and postdoctoral research fellows involved. One channel of recruitment of graduate students and postdocs is through CBT's strong involvement in international activities and exchange (mainly with Latin America and Japan).

Collaboration Several collaborations on particular research projects were formed within CBT. Those have been both inter-university (e.g. Northwestern, Rockefeller, and Scripps on the Clock Genome Project; UVa, Brandeis, and Scripps on the Technology Development Project) and single-university inter-departmental (e.g. the Department of Biology and the Department of Medicine at UVa on the Pacemaker Mechanisms Project).

Discussion and Conclusion

The two case studies presented in the preceding section illustrate some of the principles and more salient patterns discerned in scientific collaborations earlier in the chapter. Examining scientific collaborations outside the narrow confines of physics creates an opportunity to test the robustness of the analytical framework and major findings of the AIP study of multi-institutional collaborations. The Center for Nondestructive Evaluation (CNDE) represents an instance of collaboration in applied research that is, in a lot of ways, atypical of the 53 projects in the AIP study sample. For starters, CNDE is not a multi-organizational collaboration, but a single-organization, multidisciplinary, sponsor-stimulated, and pre-competitive academic/industrial cooperative arrangement which does not fit neatly into established classifications. The Center for Biological Timing (CBT), by contrast, does constitute a multi-organizational scientific collaboration that resembles very closely the AIP study collaborations, except that it represents the biological sciences and not the discipline of physics.

According to our empirically derived benchmarks for size, both collaborations were fairly small. CNDE was comprised of only 25 full-time professional researchers and, although it had a number of graduate students working for the center, these were only affiliated with it temporarily. There was only one participating organization (Iowa State University) but a much larger number of research teams, organized around seven problem areas. Similarly, CBT consisted of only 29 scientists who were classified as "full-time research staff". It, too, did have a larger number of graduate students and more postdocs than CNDE. For this center, the number of organizations was quite small—just four collaborating universities (University of Virginia, Northwestern University, Brandeis University, and Rockefeller University), with Scripps Research Institute collaborating but not being a formal member. Although the number of teams fluctuated, overall there were three long-lasting research groups.

Two findings from the multi-year AIP study of multi-organizational collaborations in physics and allied sciences were confirmed. First, as our larger sample demonstrated, no association could be established between size and perceived success of the collaboration. Both CNDE and CBT were considered relatively successful by either their participants or outside members of the respective professional communities, but so were other collaborative projects whose magnitude dwarfed the two cases of interest. Second, both of these NSF centers exhibited another pattern that revealed itself upon deeper analysis of the AIP study sample—that large, more formally organized and more tightly managed collaborations are especially prevalent in the field sciences (e.g., space science, geophysics), while small, more informally organized and more loosely managed collaborative projects are more common in the so-called laboratory sciences (e.g., materials science). Both CNDE and CBT clearly belong to the latter category and thus manifest the typical scenario.

Several of the major findings from the larger AIP study were essentially supported by the in-depth look at the two case studies, for example the need to strike a working balance between the demands for bureaucracy and technology in order to accomplish the collaboration's chief goals, or the fairly expensive demands for instrumentation and other resources that necessitated collaborating instead of trying to go it alone, although not to the extent most visible in modern particle physics. In the same vein, despite the differences in their origins, the occasional disagreements and temporary tensions in CNDE and CBT did not evolve into full-blown conflicts and did not erode trust to a tipping point, so that ultimately both centers delivered on most of their promises.

The birth drama of the two NSF-sponsored centers confirms the AIP study conclusion that the beginning of a collaboration is instrumental in evaluating its success—both cases experienced resource uncertainty and struggled to obtain much needed funding, and, by being able to overcome this hurdle in their formative period, set the stage for their later success. The empirically derived classification of scientific collaborations in terms of how they were formed and the discovery that there are five main patterns of this origin can be fruitfully applied to the two case studies.

Bearing in mind that CNDE was not an inter-institutional but inter-departmental and interdisciplinary collaboration, we can still understand its inception as following the pattern characteristic of the first type of collaboration formation (the "dominant sector/conventional collaboration", or, more informally, the "Wake Up and Smell the Coffee" type). Since the collaboration involved just a single institution, there is no point in focusing on a dominant sector, unless we consider the university to be a dominant sector. However, the collaborators who came from different departments within Iowa State University did have a prior history of working together, knew each other, and had already had several joint publications. In that sense, CNDE was built on pre-exiting relationships and can fittingly be labeled 'conventional'. And just like most AIP collaborative projects of this type, the participants realized that a new and excellent opportunity to launch a research project was available if they proposed a project that required that they work together, i.e. a project that would not stretch the norms of their home organization but, at the same time, would give them an excellent shot at performing world-class research. The impetus for starting CNDE was mostly the fact that, as the existing funding stream from DARPA and the US Air Force began to wane in the early 1980s, NSF created a new funding program—the Industry-University Cooperative Research Center.

CBT is a good example of a multi-organizational collaboration in biology that followed the model of the second type of project formation—what we call the "Dominant Sector/Unconventional Collaboration", or, more colloquially, "Damn the Torpedoes—Full Speed Ahead". As was the case with those in the preceding category, these collaborations had a dominant instigating sector (all formally participating institutions in CBT were research universities) and an obvious source of funds. Unlike that category, their members had generally not worked together previously, although they knew each other by reputation, and the members' parent organizations influenced the collaboration because the collaboration promised to improve the relationships among the parent organizations. The obstacles, or 'torpedoes', that CBT strived to overcome, were primarily the unsuccessful initial grant proposal, which necessitated an expansion of the scope of research and the inclusion of Northwestern University and Rockefeller University two years later, and the uncertainty and risk of collaborating with researchers from other parent organizations whom you had no previous history of working jointly with.

Turning to the organization and management of the two case studies, several observations merit attention. Both projects were fairly small and, although external funding agencies expected some minimal bureaucracy and structure, overall the formal structures and organization of work never amounted to a regulated, repetitive routine that we associate with bureaucratically run collaborations. Since the data acquisition was not integrated to the degree that particle physics collaborative projects exemplify, CNDE and CBT were not as informal and the independent interests of the member organizations were more meaningful. Therefore, the two case studies very closely approximated a particular type, which we identified in the AIP study as the semi-bureaucratic leaderless collaboration. Although this type did not exhibit any discipline-specific prevalence, it had the largest number of projects in materials science (similar to CNDE) and a good number of medical physics collaborations (similar to CBT).

Like the bureaucratic collaboration, the leaderless collaboration has fairly formally organized, highly differentiated structures that serve much the same purpose: to insure that private interests are not stamped on the collaboration, especially when significant levels of resources are at stake, and to insure that appropriate people stay focused on specialized tasks. But unlike bureaucratic collaborations, leaderless collaborations do not designate a single scientist to decide scientific issues or even to represent scientific interests. The strong sense of hierarchy present in a bureaucratically organized and managed project—in which some scientists were more important than others, the important scientists felt they were outranked by project management, and the Board of Directors actively monitored developments and adjudicated disputes—was not present in leaderless collaborations. In leaderless forms, administrators sought the input of research scientists on key affairs, appointed scientists to take charge of developing instrumentation, and attended to the collaboration's external relations while benignly neglecting internal politics. Both CNDE and CBT had an Advisory Board and an administrative leader (the director of CNDE Bruce Thompson and the director of CBT Gene Block, respectively), both lacked a designated scientific leader or a formal Science Steering Committee—but, instead, had several science working groups, headed by a principal investigator that interacted occasionally, in the spirit of collegiality and respect for each other's autonomy, with the center staff and with each other.

In the past, scientific research and development (R&D) could be done relatively inexpensively. That is no longer the case. Mass producing cars, sending a man to the moon, sequencing the human genome, and other feats of the 20th and 21st century science and technology require enormous capital investment, detailed attention to the way work is organized, and legions of scientists and technical experts. Add to this the intensely competitive business and geopolitical environment and one can readily understand why ever larger sums have been invested in research and development and ever more organizations have had to pool resources over the past hundred years.

By the beginning of the 20th century, the scientific and engineering genius operating in isolation was only rarely able to contribute much to scientific discovery and technological innovation. By mid-century, most research and technological innovation was organized along industrial lines, involving collaborations among various organizations often from different sectors. Entire armies of experts and vast sums of money were required to run the new invention complexes. The prototype of today's invention factory was the Manhattan Project—a quintessential multi-organizational collaboration, which built the atomic bomb in the last years of WWII. By the time of Hiroshima, the manufacturing complex of the United States nuclear industry was about the same size as that of the United States automobile industry. The era of big business and big technology had arrived. Only governments and, increasingly, giant multinational corporations could afford to sustain the research effort of the second half of the 20th century.

As the 20th century ended, there seemed to be no upper limit to the amount that could be spent on R&D. In 1997, i.e. at the time when the AIP study was being completed, American research and development spending reached $205.7 billion, up from $74.3 billion in 1960 (calculated in 1997 dollars to take account of inflation). During the same period, industry's share of spending rose from 33 % to 65 % of the total expenditures for R&D, while the government's share fell from 66 % to 31 %.

Despite this change in the funding mix for research and development, the United States currently still leads the world in R&D spending and performance with 36 percent ($344 billion as of 2007) of the world spending in this strategic area, which totals close to a trillion US dollars in 2007, but the gap between the US and other countries has been narrowing in recent years.

The large investments necessary to do cutting-edge research today also imply that it becomes harder and harder for a single organization to engage in such research and more and more R&D will be carried out in collaborations among several organizations that often involve hundreds of scientists, engineers, support staff, postdoctoral fellows, graduate and undergraduate students. These fairly recent massive research undertakings reflect the necessity to take advantage of opportunities opened up by globalization and new communications technologies to pool resources and work together from different locations. Such multi-organizational—and often interdisciplinary—collaborative mega-projects are also reflective of a trend toward more fluid, flexible, and temporary organizational arrangements that are rapidly becoming the rule rather than the exception in the modern social institutions of science and technology, as well as vital components in the future growth and influence of research-intensive universities in the 21st century.

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Part V
Epilogue

Chapter 10
The Legacy of Joseph Ben-David

Marcel Herbst

Abstract The chapter is an attempt to pay tribute to an exemplary scholar and to review Ben-David's formation and intellectual positions, in the context of his own environment and on the basis of the spectrum of contributions collated in this anthology.

In this Epilogue Ben-David's professional development, from that of a young Hungarian refugee and immigrant to Palestine to that of a preeminent scholar of science and higher education, is sketched, and aspects within Ben-David's system of thinking are emphasized which might form a base on which to build.

Science, its institutional home, the research university, and higher education in general have become central pillars in support of today's societies and world economy. This constellation is relatively new, not older than a few decades. Science as we understand it today originated in 17th century Europe, research universities entered the scene during the 19th century, and higher education—in the form of mass higher education—took on a new role in the decades following World War II. This development, extending over more than three centuries, was accompanied by a change of support structures, from the comprehensive church and private laboratories or intellectual circles to principalities, parochial congregations, entrepreneurial groups, and local authorities. The institution of the university, which we generally link to the founding of the *Università di Bologna* in the year 1088, is perhaps one of the most resilient institutions of today's world. But over the course of this past millennium, particularly during the last 200 years, it has changed a great deal. The 'sciences' were integrated and research universities were established in the 19th century, first in Germany and subsequently in the US and in other—European and non-European—countries; the universities were consolidated and formed intellectual and technological backbones of their respective societies during the turbulence associated with industrialization, World Wars, and the Cold War; and mass higher education, in concert with the transformation of the respective societies into service-oriented economies, has lead to retrenchment, new models of leadership and gover-

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nance, new modes of budgeting and funding (Herbst 2007), and to a diversification of higher education institutions (Clark 1997; Trow 1997).

When Joseph Ben-David started his professional life during the 1950s, the world looked back on an agonizing World War and ethical aberrations of monstrous dimensions[1]; and it looked forward to a Cold War, manifest soon after World War II and, in particular, after the Sputnik Shock (1957). Europe was divided, large regions within Europe had been destroyed to various degrees, and the European universities, particularly the German institutions, were drained (Krohn 1987/1993). But the 1950s and the 60s were also a period of hope, a period of *Wiederaufbau*, of normalization, of state building, of economic prosperity and *Wirtschaftswunder*, of intellectual and cultural challenges. This pertained, specifically, to Britain and the US, to France, Italy and Japan, to Israel, and to Germany. The universities in the UK (Medawar and Pike 2000) and the US gained in this aftermath, not to speak of Israel. Immediately after World War II, the US embarked on an exerted program to foster domestic science and higher education (Bush 1945; Zachary 1997), building on the momentum of the war machinery and the exigencies of the post-war demands, and it actively pursued the "postwar reconstruction of science in Europe" (Krige 2006). In the US, the post-war decades became "academia's golden age" (Freeland 1992; Graham and Diamond 1997).

The Unfolding of Ben-David's Themes

Ben-David's adulthood and professional formation falls into that period of reconstruction and hope. He enrolled at a young institution, the Hebrew University of Jerusalem (see Chap. 7), where he studied history and sociology (Collins 1986; Westrum 1986). While studying, Ben-David was employed in the field of social work, and upon graduation he enrolled at the London School of Economics (LSE) on a scholarship from the British Mandate. His program at LSE was a special one directed at social workers in the British colonies (Shils 1987a).

Like other students of his war generation, Ben-David entered a world with more questions than answers, and the challenges to find answers proved, at least for some of them, a *Leitmotiv*. As a student he worked with delinquent youth, and this exposure may have paved the road to fields which subsequently interested him. Martin Buber, the existentialist theologian, was exposing Ben-David to introductory sociology and sociology of culture, and it is likely that a range of other 'transgressions' had their impact on the student. Certainly the balancing act between Martin Buber and Edward Shils, who was teaching as a guest lecturer at LSE and whose seminar Ben-David attended in the late 1940s (Shils 1987b), and his discussions with Shmuel Eisenstadt, who subsequently would supervise his PhD-thesis (Ben-David 1958) and was doing his post-doctoral studies at LSE, must have fostered

[1] In his introduction to Karl Mannheim's "Ideology & Utopia", Louis Wirth referred in 1936 to such phenomena as "perversities of culture" (Mannheim 1985/1936, xii).

and later released some creative energies. Upon returning to the Hebrew University of Jerusalem, Ben-David completed a thesis in history that was published posthumously (Ben-David 1997).[2]

Over his lifetime as a scholar, Joseph Ben-David worked in a range of related fields and addressed a broad range of issues, eschewing narrow specialization and transgressing various disciplines, in particular history, sociology, political science, philosophy, and education. He spent the academic year of 1957–58 at the Center for Advanced Studies in the Behavioral Sciences at Stanford, and that year affected and focused his professional development. He formed long-lasting relationships with American colleagues, and he had the opportunity to interact with leading social scientists who happened to be there, such as Talcott Parsons or Milton Friedman.[3] He spent the academic year 1964–65 at the University of California at Berkeley where Randall Collins was his research assistant. Eventually, Ben-David became George S. Wise Professor of Sociology at the Hebrew University while also being appointed as Stella M. Rowley Professor of Education and Sociology at the University of Chicago.

The earliest publications of Ben-David (in English) dealt with classical sociological issues which emerged as part of his initial focus on social work (Ben-David 1953; Frankenstein 1953; Eisenstadt and Ben-David 1956).[4] His dissertation, completed under Shmuel Eisenstadt in 1955, and associated work already had a focus that was to occupy him for the rest of his life: the social structure of professions (Ben-David 1955, 1956, 1957, 1964b), and the study of professions focused on specific disciplines such as medicine (Ben-David 1958). With this, Ben-David had built a platform to address issues that would become central to his analyses as a mature scholar: issues of professional or scientific roles (Ben-David 1958, 1960a), issues of scientific productivity or academic organization (Ben-David 1960b), or issues of markets and competition (Ben-David and Zloczower 1961): his vision was, as Randell Collins (1986) has pointed out, “that institutional roles in organizations are the fundamental underpinning of scientific activity, and that whatever changed these roles, qualitatively or quantitatively, would affect the kinds and amounts of science that was done”.

Subsequently, Ben-David issued, again with Awraham Zloczower, a second article in the *European Journal of Sociology* which must be regarded as seminal from a

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[2]In this passage, I rely here in particular on Miriam Ben-David’s recollections, “On Joseph Ben-David” (Ben-David 2009); see also Ben-David (2012).

[3]The list of social scientists who had spent the academic year of 1957–58 at the Center for Advanced Studies in the Behavioral Sciences and who formed a close relationship with Ben-David also included Ralf Dahrendorf, David Landes, David Mandelbaum, Robert Solow, Fritz Stern, George Stigler, Milton Singer, Ithiel de Sola Pool, Charles Glock, Benson Ginsburg, and Louis Gottschalk (personal communication by Miriam Ben-David). See also Miriam Ben-David (2009, 2012).

[4]For a bibliography of Ben-David’s writings see: Ben-David (1991a, 561–568). In the following I shall refer only to a subset of Ben-David’s writings, namely those that appear central to the discussions of this volume.

developmental, or a science propagating, point of view; the two authors tried to explain "how and why universities became what they are" (Ben-David and Zloczower 1962). In a following article on "Scientific Growth", Ben-David (1964a) amended this view, and he identified "[t]he crucial problem for those concerned with scientific policy", then as now, namely "the relationship between particular scientific institutional systems or components thereof and particular kinds of scientific production such as basically new ideas, discoveries based on existing ideas, contributions to industry, medicine, and other practical fields" [p. 470f]. He remarked that "[n]o one has yet attempted to study this question systematically", and this assessment, in spite of a range of studies with such a focus, remains valid to this day.

In the above mentioned article of 1964 Ben-David applies a perspective that is characteristic of his cross-disciplinary approach to science and higher education studies, and he bridges approaches of history, sociology, and philosophy of science. He takes issue with a major notion within philosophy of science, Thomas Kuhn's notion of a "paradigm" (and associated "paradigm shifts") and its role in the evolution of science and disciplines (Kuhn 1970/1962) and suggests other explanations for a development, or an evolvement, of science. He publishes "The Scientific Role" (Ben-David 1965), and later on the "The Scientist's Role" (Ben-David 1984/1971), and explains the emergence of 'science' (in the 17th century), notwithstanding other explanatory factors such as puritanism (Merton 1938), with the emergence of a new role, that of a scientist.

Ben-David's notion of the scientist's role was not strictly descriptive but also normative, as Yaron Ezrahi has indicated (see Chap. 3). This perspective is evident in Ben-David's view on science and its relation to economic growth (Ben-David 1968) or, subsequently, in his view on science planning (Ben-David 1977c). He saw science as a value in itself, as an attribute of a free society, and he didn't see, correctly I presume, a close link between science at the cutting edge and economic development. His views on the relation between science and economic growth were formulated just before mass higher education was perceived as a phenomenon (Trow 1970), before science education and higher education diverted that markedly, and long before higher education was commonly hailed as an engine of economic development and prosperity.

In his perspective on the sociology of science, Ben-David (1970) begins by pointing out that "[t]he sociology of science studies the ways in which scientific research and the diffusion of scientific knowledge are influenced by social conditions and, in their turn, influence social behavior". This statement, as such, allows for at least two foci: the broader, macro-sociological focus on the interplay between science and society; and the narrower micro-sociological focus on the connection between science and its immediate framework within which it takes place. Ben-David worked in both fields, ingeniously interweaving the two approaches, and he found it natural, perhaps even in a way necessary, to view science and science settings comparatively.

In reviewing the perspectives of a modern sociology of science (1920–70), Ben-David identifies two key periods: the period prior to World War II, and the post-war period. During the first period, the discussion was driven, according to Ben-David, by natural scientists turned 'amateur' sociologist. These introduced a quantitatively based reasoning which started to blossom with the pioneering work of Eugene Garfield in the early 1950s. Moreover, there was an early attempt to 'use' science, "to apply science to the advancement of social welfare" in the respective societies. Because science was viewed as a truly international activity, and because of the paucity of studies in the field of comparative sociology, there was also a tendency to "disregard the differences between social settings of scientific and technological conditions of discovery". But not all studies lacked this comparative approach, seen by Parsons as the "logical equivalent to experimentation" (Camic 1991, 126), as Abraham Flexner's study demonstrates (Kerr 1994a).

The post-war period, as I pointed out initially in this Chapter, had a different drive, a different orientation. Science, and its relation to society, was seen from a different perspective and, for a period at least, micro-sociological aspects and productivity issues were being investigated. With the advent of bibliometric supported science studies in the 1950s the opportunity of an "interactional study of the scientific community" or the "study of scientific productivity of research groups" presented itself. Ben-David saw Donald Pelz as the main representative of this research orientation (Pelz and Andrews 1976/1966). Furthermore, he also saw the focus on research groups, or networks of such groups, on different science cultures within a discipline, as antithetical to Thomas Kuhn's implicit notion of the scientific community (Kuhn 1970/1962).

By the advent of the 1970s, Ben-David's main theses on science and higher education were developed. He used that decade to expound his theses in a range of monographs[5]: "The Scientist's Role in Society" (Ben-David 1984/1971), "American Higher Education: Directions Old and New" (Ben-David 1972), "Centers of Learning: Britain, France, Germany, United States" (Ben-David 1977a); and he coedited a collection of essays honoring his former teacher, Edward Shils, "Culture and Its Creators" (Ben-David and Clark 1977). Ben-David also felt more inclined to depart from a strictly positive or interpretative stance, common also in the social sciences, to assume normative, prescriptive positions.

While Ben-David, as stated, focused on macro-sociological as well as micro-sociological aspects of science and higher education, he fused the two approaches to blend a sociology of knowledge with a sociology of professions or organizations (Ben-David 1977b). During the last years of his life, he continued to focus on a broad spectrum of issues. He published a range of 'theory papers', such as a contribution on the "Ethos of Science" (Ben-David 1980b), a paper on the merit of sociological inquiries, "Sociology and Its Uses" (Ben-David 1980a),[6] a chapter on "Sociology of Scientific Knowledge" (Ben-David 1981), a chapter on "Puritanism and Modern Science" in an anthology dedicated to S.N. Eisenstadt, his PhD thesis supervisor (Ben-David 1985), and another contribution on "Science, Scientism and Anti-Scientism" (Ben-David 1986).[7]

[5] He also participated in an evaluation by the OECD of the Japanese higher education system (OECD 1971); see Takeishi (2012).

[6] With Liah Greenfeld as assistant.

[7] Published posthumously.

Much of what is contained in these 'theory papers' appears from today's perspective as a tightrope act, even for many who share his rejection of 'anti-scientism'. Ben-David tries to retain a strict notion of science and scientists in their search for 'truth', following the tradition of Max Weber and others; and he distinguishes, somewhat sharply, between basic research, applied research and development, seeing basic research, if I interpret him correctly, as the activity compatible with the scientific ethos. He strongly believes in the ethos of science, in the self-cleansing capacity of an objective, bias-reducing, truth-seeking science, and he rejects relativistic positions of a sociology of knowledge. He also appears to distance himself from the pragmatic "science is what scientists do" (Churchman 1961, 1971). Ben-David's notion of the ethos of science, his focus on basic science and the natural sciences as an 'ideal-type' of science (in Max Weber's sense), greatly reduces the spectrum of activities which might be subsumed under the term of 'science'; and this has implications for his picture of institutions which harbor science—i.e. the prime foci of Ben-David's research.

The major theses of Ben-David and his relevance for today's world of higher education, however, do not hinge on his purported views of science. Whether or not Ben-David adhered to the notion of "science is what scientists do", whether or not we distinguish between basic and applied research, or whether or not one accepts descriptive as well as normative activities under the umbrella of science, his major theses remain largely unaffected. Ben-David was in no way naïve or doctrinaire, he was conscious of fraudulent behavior in the field of science, of extra-scientific motives, of political pressures and biases, or of the simple limitation of resources, and he clearly distinguished between the individual scientist and the institution of science (Ben-David and Sullivan 1975, 207). While the former was allowed to be biased or narrow-minded, driven by hunches or motives, the latter was what mattered, and in particular "the separation of the social-control mechanisms for recognizing and evaluating research from the organization for research and teaching in science" (Ben-David 1977b, 263).

The Major Themes

This anthology on Ben-David and the essays contained herein attempt to focus on the relation between science and its supporting institutions, in particular the university, and on Ben-David's perception of such a relation. Ben-David was clearly not the only scholar who addressed issues of a sociology of science, or issues pertaining to higher education and universities, but he was one of the few scholars who bridged these two broad areas of investigation. Furthermore, he was one of the few scholars who worked comparatively, linking higher education cultures of various nations, in particular those of Germany, France, England, and the US.

Just before Ben-David entered the field, Merton (1973/1952, 211f) bemoaned the sorry state of sociology of science[8]: "not many persons cultivate the field altogether

[8]Robert Merton's note was originally published as the Introduction to Barber (1952).

and those who do are for the most part physical and biological, rather than social, scientists". While sociology as such was well established, at least for the time being, with "scores of studies deal[ing] with the sociology of marriage and the family, with population and crime, and an appreciable number deal[ing] with the sociology of religion, [...] the sociology of science has not yet enlisted sufficient interest to merit separate notice in the annual catalogues of sociological research". The same assessment of the situation was shared by Edward Shils, whose lectures Ben-David had attended at the London School of Economics and who, according to Merton, is said to have remarked that the study of science and scientific institutions is "among the major undeveloped areas of sociological inquiry". Whether it was Shils' influence that brought Ben-David to the sociology of science, or whether it was the practicing of his own embryonic theory of "role-hybridization" which led him to enter an underdeveloped field, we do not know[9]: it is likely that it was a combination of both.

The broad, inter-disciplinary approach of Ben-David, spanning history, sociology, political science and higher education studies, coupled with his comparative analyses, eventually shaped and characterized the particular views of Ben-David. Comparative analyses of higher education systems were, and are, not that common. Higher education studies are normally written by people interested in education, and those people may only feel free to report on the system they are 'naturally' familiar with (Paulsen 1920/1906). An exception are studies such as those by Abraham Flexner prior to World War II (Kerr 1994a) or, afterwards, by Martin Trow (Halsey and Trow 1971; Burrage 2010), George Weisz (1983), Bob Clark (1983, 1995, 2008) or Claudius Gellert (1988). Collection of essays with an international focus, written by local experts, are a more recent phenomenon (Clark 1993). Philosophy of science studies, as meta-studies of science, normally do not touch the interplay between science and social structure, and may be neglected in our context.[10]

Ben-David's researches in science and higher education covered three broad and interrelated thematic areas into which this anthology, apart from Prologue and Epilogue, is subdivided: Role and Ethos (Part II), Center and Periphery (Part III), and Organization and Growth (Part IV). The concept of 'role' already formed a central part of Ben-David's dissertation and provided an opportunity to reflect on professions and their implicit 'ethos'. It also provided an immediate opportunity to think about 'productivity' and 'growth', other early concepts of Ben-David. Growth, of course, is tied to an increase in the number of scholars and students in given fields, to the diversification of scientific fields and to the implicit framework of ideas, and such development is embedded in societal conditions. All these factors together will affect notions of 'center' and 'periphery'.

[9]Or, conversely, his thesis of "role-hybridization" emerged also on the basis of personal experiences.

[10]In practice, however, the distinction between philosophy of science, on the one hand, and sociology of science (concerned mainly with micro-sociological aspects) or sociology of knowledge (concerned mainly with macro-sociological aspects), on the other, is frequently blurred. Philosophy of science, on the whole, deals with norms of scientific activities, mostly distinct from the social structures within which science is embedded; sociology of science or sociology of knowledge, on the other hand, specifically focus on the interplay between science and social structure.

Shift of the Academic Center

In Ben-David's notion of center versus periphery, the center of the scientific world moved from Italy (in the 16th century) to England (in the 17th century) to France (in the 18th century) to Germany (in the 19th century) and to the US (in the 20th century) (Ben-David 1984/1971, 15). Modern science had its first blossoming in the scientistic inclined societies of England and France, but it bloomed, ironically enough, in the 'ascientific', and at times even 'antiscientific' environment of the German university. The reasons for such an unexpected—and unplanned—phenomenon, in Ben-David's view, are many, and I shall try to sketch some in the following.

German science did not come on stage with the founding of the University of Berlin (1809) and the so-called Humboldtian system[11]; around that time, French science still reigned. While the reform movement in France at the end of the *ancien régime*—and the abolishment of universities and the associated formation of *grandes écoles* and polytechnic schools—was "spearheaded by scientists and scientistic philosophers", the reform in Germany was advocated by philologists and "ascientific philosophers". The new German university was not meant to foster science in the French or British 'empirical' sense; "[. . .] originally the spirit prevailing in the new universities was more a revival of the spirit that had prevailed in the Greek schools of philosophy than an attempt to base education on modern science". The German university was meant as a place for a growing stratum of intellectuals in search of recognition and status (Schwinges 2007) and, in the course of a 'national' reorientation, as a home for "a new speculative philosophy that extolled an ascientific idea of a nationalistic philosophical, literary, and historical culture [. . .]" (Ben-David 1984/1971, 113–117).

The German university in its Humboldtian form was neither a specialized institution nor a professional school like the new *grandes écoles* and polytechnics in France. It pursued the aim to study the full spectrum of humanities and nature, including medicine; it was to replace the research function of academies, pursued the unity of teaching and research, and the close cooperation of the mature scholar with the student; it demanded adequate financial support from the local authorities; and it persisted on the notion of various 'freedoms' for its members that were not that common at the time, affecting censorship and the 'freedom of inquiry' (regarding teaching, research activities, and learning) (von Humboldt 1964b): "Science had to be accommodated in an inimical environment, and special safeguards had to be devised for securing this freedom" (Ben-David 1984/1971, 119). The subsequent export of this idea beyond Prussia to all German speaking regions of Central Europe and to neighboring non-German speaking regions as well (Schwinges 2001; Schalenberg 2002), and the concomitant competition which ensued among the various universities and their sponsoring principalities, was a first factor in promoting

[11]The concept of the 'Humboldtian system', or the 'Humboldtian university', appear to have emerged in the 20th century, more than 100 years after Humboldt's contribution in 1809 (von Humboldt 1964a); see Paletschek (2001, 77). Furthermore, earlier reforms in Göttingen (1737) and Halle (1787) appeared to have provided the foundation for the new university culture (vom Brocke 2001, 367).

the ascent of the German university.[12] The second, in Ben-David's view, was the "organized scientific research" in the form of the laboratory:

> "The rise of empirical science starting from the late 1820s (due to the work of such pioneers as [Justus] Liebig, Johannes Müller, and their disciples) was not a result of the new university, but of a conscious revolt against its philosophy and of an important, although not deliberate and conscious, modification of its structure.
> The superiority of the German to the French system is to be sought, therefore, in the capability of the German system to change itself according to the needs and potentialities in scientific inquiry in spite of the wrong ideas (from the point of view of empirical science) of the university's founders. By contrast, institutions in the French system, even if originally well conceived, were incapable of adapting themselves to change" (Ben-David 1984/1971, 118).

The German university became the center of the academic world because of its invention of "organized scientific research" and because of its adaptability, its capacity to change itself. But it retained, in a sense, its dual character: on the one hand, a devotion to a "romantic *Naturphilosophie*" (Ben-David 1984/1971, 117) with its critical distance to quantification, model building and empirical work, which is still discernible in the grand and verbose œvres of the German philosophers (and social scientists) of the 20th century; and, on the other hand, a focus on science in the strictest sense with a focus on laboratory or experimental work (i.e. in physiology, psychology, chemistry or experimental physics) or abstract sciences (mathematics, theoretical physics).

It was in particular, but not exclusively, this second character of the German university which attracted scholars outside of Central Europe and which led them, eventually, to try to transfer particular features of the German university and to emulate the German system (Turner 2001; Miyasaka 2001). The emergence of the US research university around 1870[13] is clearly tied to the model of the German university. Many American students, mainly of an advanced standing, studied in Germany for a time, and upon returning tried to implement some of the attractive features of the German academic system in their home institutions. In the US the Graduate School was introduced, molded on a German model, at least in the perception of the American scholars: the elective system took hold,[14] laboratories were introduced, and the general erudition of German scholars set standards for scientific and schol-

[12] One should not read 'German', hence, in a too constricted way. The German—Humboldtian—university was the role model in Prussia and Germany in general, but also in the Austro-Hungarian Empire, and in bordering nations not governed by the Napoleonic system.

[13] The Massachusetts Institute of Technology admitted its first students in 1865; Johns Hopkins University, the first full-fledged research university in the US, was founded 1876; Harvard initiated a university reform under President Eliot who took office in 1869.

[14] President Eliot of Harvard introduced the elective system, emulating a German praxis, and the subsequent continent-wide implementation of a credit system by the *Carnegie Foundation for the Advancement of Teaching* at the beginning of the 20th century preceded the introduction of a European credit system by roughly 100 years.

arly work. As late as 1930 Abraham Flexner (1930), founder and first president of Princeton's Institute for Advanced Studies, was enchanted by German science.[15]

While the German university was the undisputed center of the scientific world for much of the 19th century, the same forces that had once propelled the German university to its prominent position, Ben-David reasons, were now responsible for its "standstill", for its "strangulation", around the turn of the century (Ben-David and Zloczower 1962, 129); the reasons for this development—growth followed by stagnation and decline—shall be explored, and sketched, in the following. The center moved eventually to the US, and it has remained there ever since.

Disciplinary Development

At the end of the *ancien régime*, the 'scientific' community may have distinguished roughly among six different fields (or faculties): theology, law, medicine and philosophy within their universities, plus perhaps two fields outside this framework in engineering (including mining) and architecture. Today, roughly 220 years later, the *Essential Science Indicators* of ScienceWatch distinguish between 242 disciplinary fields within engineering and the sciences (including the social sciences), not counting architecture and the humanities.[16] Ben-David hypothesized on the specific causes of this diversification, and he postulated that "role-hybridization" was

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[15] For a critical review of Flexner's position, see Clark Kerr (1994b). It is clear that the German university was held in high esteem in the 1930s in the US, and when Abraham Flexner (Bonner 2002), a respected scholar and educator, published a positive assessment of the German university he had just studied in 1930, it was well received. But the reception of Flexner's book was misguided and it did not have, fortunately, lasting effects. Clark Kerr (2001a,b), the 'architect' and the president of the University of California System (1958–1967), assesses the situation with the following words (Kerr 1994b, x):

> "[...] Flexner was so wrong about the German universities he so revered, so wrong about how good they really were—they had collapsed by 1933 and partly of their own doing; and so wrong about the American universities that he so scorned—they were on their way to becoming the best on the world".

Kerr (1994b, xxvii) sadly notes, referring to the autobiography of Flexner (1940), that as late as in the last years of the 1930s and—in Flexner's words—"despite the ravages of the war", Flexner still considered the German universities "the best in the world".

It is unclear what Kerr had meant with his allusion to the collapse of the German universities "partly of their own doing", but he may have had two factors in mind: the racial policies of the Nazi regime with their negative affect on the German university (we need not talk of the negative effects on the victims of the Nazi regime), and an inherent deficiency of the German university model as it entered the 20th century.

[16] If we were to hypothesize a linear relationship between the number of fields and the time for their emergence, this would roughly mean an addition of a bit more than one field (or discipline) per year. The assumption of a 1.7 % growth per annum in the number of scientific fields (or disciplines) is more realistic. This would translate into roughly 15 fields by the year 1845, 38 fields by 1900, 97 fields by 1955, and 245 fields today.

the direct cause, or motor, of this diversification. Since Ben-David's concept of role was tied to his concept of discipline (or profession), a role was discipline-specific (see also Chaps. 5 and 4). Role-hybridization took place when "the individual [was] moving from one role to another, [...] from one profession or academic field to another" (Ben-David 1991b, 61).[17]

That the spectrum of scientific fields, professions, or disciplines is growing, paralleling in a sense the growth of the number of individuals who are engaged in science and engineering, at least thus far, is clear—and was clear at the time Ben-David started his studies. It was also clear that basically four forces are responsible for this academic diversification process: (i) specialization within one discipline, giving rise to the emergence of sub-specialities and the eventual development of such sub-specialities into fully developed disciplines; (ii) the merging of sub-specialities of disparate fields into new disciplines[18]; (iii) the elevation of non-academic skills, trades, vocations or services to academic fields; and (iv) the occasional abandonment or downgrading of academic disciplines.[19] The far-reaching contribution of Ben-David was that he tied this diversification process to the specific academic system, to its rules and regulations (March et al. 2000).

Ben-David's role-hybridization focused mainly on cases (i) and (ii) above. He studied, for instance, the splitting off of physiology from anatomy (between 1850 and 1870), or the development of psychology on the basis of philosophy and physiology (around 1880; see in this context also Chap. 4 by Ilana Löwy).[20] If, Ben-David (1991b, 60) writes, "ideas become the end-product of scientific roles, they can be likened to genes which are transmitted from generation to generation through a reliable and natural process; under normal conditions, they will not only survive but increase". In other words, the propagation of ideas can be studied like genealogy, like epidemiology, and Randall Collins (1998), with whom Ben-David had written that article in 1966 and on whose M.A. thesis it was partially based, used that approach in his monumental study of his "Global Theory of Intellectual Change".

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[17] In today's language, we would perhaps talk of trans-disciplinarity instead of role-hybridization. In the case of trans-disciplinary approaches, conceptual frameworks or models are generally imported from a foreign discipline (such as from physics or biology into economics) whereas role-hybridization was seen by Ben-David primarily in terms of an export, as a shift from an established discipline to a new sub-discipline. However, trans-disciplinary approaches also take place in the form of exporting concepts and models, such as when representatives of one discipline (e.g. physics) move into another discipline (e.g. sociology).

[18] The information (or computer) sciences, formed through branches of mathematics and electrical engineering (between 1950 and 1970), may serve as examples; or the environmental sciences, formed through branches of chemistry, (organismic) biology, physics, and social sciences (between 1960 and 1980).

[19] This happens with some frequency in engineering when technologies disappear, or when technologies become so 'simple' that they find their home in lower graded (vocational) schools.

[20] Ben-David (1991b, 62) also talks of "idea-hybridization", i.e. "the combination of ideas taken from different fields into a new intellectual synthesis", and he continues "[t]he latter does not attempt to bring about a new academic or professional role, nor does it generally give rise to a coherent and sustained movement with a permanent tradition", an obviously false conclusion from today's perspective.

Ideas propagate, in one form or another, over generations of scholars, but Ben-David's specific contribution was in regard to disciplinary diversification and its relation to particular 'cultural' features of university systems. He postulated that new disciplines emerge at the fringes of established disciplines through role-hybridization, in that (aspiring) scholars who find no chance for career advancement in their own field are inclined to found new disciplinary orientations. He saw the conditions for such development particularly suitable in the German university system of the 19th century, with its specific rules governing faculty positions:

> "Mobility of scholars from one field to another will occur when the chances of success (i.e., getting recognition, gaining a full chair at a relatively early age, making an outstanding contribution) in one discipline are poor, often as a result of overcrowding in a field in which the number of positions is stable. In such cases, many scholars will be likely to move into any related field in which the conditions of competition are better. In some cases, this will mean that they move into a field with a standing relatively lower than their original field" (Ben-David 1991a, 61).[21]

The argument of Ben-David has an 'ecological' quality. Similar to the litter of wolves, once weaned and grown, that has to leave the pack and the common hunting territory to disperse and to find new ground, so have young scholars to find new turf. In the early Humboldtian system the new turf could be found at universities without a disciplinary orientation, i.e. a chair, in the specific field the aspiring scholar was working in. In the more mature Humboldtian system, the existing universities already had such chairs,[22] and new turf could only be found through role-hybridization and through the exploration of new disciplines.

The rules that governed this play at any university of the Humboldtian system, that each disciplinary field is represented by a single chair (and vice versa) and that only scholars with a formal post-doctoral training (*Habilitation*) were in a position to apply for a position as chair holders, had two effects: once a chair was established, there were doctoral and post-doctoral students around to secure the functioning of the research institute that was associated with the chair; and aspiring scholars eventually had to move elsewhere to assume positions in the same field or in associated fields (through role-hybridization). In this way decentralization took place, diversification of the disciplinary spectrum, and competition among all involved, i.e. factors which Ben-David attributes to be central for the eventually dominant position of the German university in the 19th century. However, the same rules and regulations, the

[21] From today's perspective, Ben-David's insistence on standing differentials is open to debate, and it appears to play a minor part in his legacy. Furthermore, there are a range of motives for scholars pursuing a line of research, but there are not that many outstanding scholars who move to a new field because "the conditions of competition [in the new field] are better": innovators like Alan Turing, John von Neumann or Donald Knuth clearly had other motives guiding their research.

[22] In the case of retirement of an existing chair holder (*Emeritierung*), a scholar was sought who could continue the tradition—and the specific research focus—of the particular chair. In this way, lineages of descendants could be drawn, like those of the royalty. Even today, if there is an open faculty position at a university with a strong Humboldtian tradition, reference may be made not only to the field of the candidate for such a position, but also to the person who held the chair before, and that his *Nachfolge* (successor) is to be sought.

same code of ethics, that Ben-David saw as instrumental for the ascent and dominance of German science throughout much of the 19th century, he also saw as being responsible for the eventual decline of the German system "toward the end of the century [when] processes of extending the scope of the university came to a standstill" (Ben-David 1991a, 129):

> "The usual rule that each discipline was represented by only one professor contributed much in the previous decades to the establishment of new chairs, because the expansion of the academic staff could take place only in this manner. After the development of the institutes, however, the same rule became a veritable strangling noose: research could be conducted only in the *Institut*, but only one person, the director, could be professor" [p. 129].

This basic, intrinsic limiting factor, constraining the competitiveness of the German university and the further development of a range of disciplines, particularly in the fields of the natural sciences and associated technologies, led to discussions towards the end of the 19th century regarding the role and values of the 'Humboldtian' university (Szölösy-Janze 1998; vom Brocke 2001). "Big science" was not only a theme which occupied people after WWII (de Solla Price 1963; Galison and Hevly 1992), it was a theme in the forefront of a discussion at the end of the past century as well, and is was tied to industrialization (Ben-David 1977a, 103). Standards and norms had to be researched, defined and eventually enforced, and various institutes in support of such an orientation were founded.[23]

In this context the question was raised as to what extent these new institutional units (*Anstalten*, institutes) were ancillary to the existing universities or polytechnic institutions, performing tasks that were delegated to them by the respective governments, or whether they should form a new type of research institution separate from—but linked to—the existing network of higher education institutions. In the German case, the visions that eventually lead to the founding of the *Kaiser-Wilhelm-Institutes* (1911) clearly went beyond standards and norms and had research & development (technology) as a focus. These new institutes should be freed from the obligation and 'burden' to teach, a clear negation of the earlier Humboldtian vision (1809) of the unity of teaching and research.[24]

The foundation of the *Kaiser-Wilhelm-Institutes*, and the continuation of this tradition after WWII under the name of *Max-Planck-Institutes*, clearly marks a bifurcation in the development of the higher education landscape.[25] With the separation

[23]E.g. the *Institut für Baumaterialprüfung* (EMPA) of the Swiss Federal Institute of Technology (1880), the *Physikalisch-Technische Reichsanstalt* (PTR) in Berlin (1887), or the US National Institute of Standards (1901).

[24]This averting from the old ideal took place before the creation of the mythos Humboldt (Ash 1997). Ben-David (1977a, 103) argues that "teaching was institutionally separated from research, first within the university through the concentration of research in 'institutes' that were personal research establishments of professors virtually separated from the university, and subsequently—in 1911, through the founding of the *Kaiser-Wilhelm-Gesellschaft*—by establishing pure research institutions without any teaching function at all".

[25]Corresponding organizations were formed for instance in Italy (CNR in 1923) and France (CNRS in 1939).

of many institutes from the university and their incorporation in dedicated research institutes, the link to teaching, to the inter-generational discourse, to a constant flow of 'new blood' was severed, while the strictly hierarchical governance of the old German "chair system", with its implicit strangling-noose-effect, was maintained.

The Evolvement of US Higher Education

In Ben-David's view, hence, the German university declined way before the advent of the Nazi regime in 1933, and way before 'undesirable' people—Jews, leftists, liberals—were pushed out of academic positions, were forced to emigrate, or were simply killed. This view is amplified by that of a range of emigrant scholars, for instance Max Perutz (Medawar and Pike 2000, xii)[26]:

> "Had I stayed in my native Austria, even if there had been no Hitler, I could never have solved the problem of protein structure, or founded the Laboratory of Molecular Biology which became the envy of the scientific world. I would have lacked the means, I would not have found the outstanding teachers and colleagues, or learned scientific rigour; I would have lacked the stimulus, the role models, the tradition of attacking important problems, however difficult, that Cambridge provided. It was Cambridge that made me, and for that I am forever grateful. The art historian Ernst Gombrich feels the same way. We all owe a tremendous debt to Britain".[27]

While literature has given us some speculative what-if-pictures of America under an assumed appeasement with Nazi Germany (Roth 2004), there appear to exist no corresponding pictures of a European academic world today had Naziism not emerged.[28] What one can study, however, is the relation between society and higher education systems (Ringer 1990/1969), between higher education systems and Nazism (Weinreich 1999/1946; Hammerstein 1995), and between Nazism and post-war intellectuals (Klee 2003; Moses 2007; Herbst 2009).[29]

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[26] A related question emerges, not to be pursued within these pages, why certain emigrant scholars, such as Adorno or Horkheimer, decided to return to Germany after World War II. See in this respect also Wolin (2001).

[27] Harriet Zuckerman (1996/1977, 71) makes a similar observation: "In reckoning the extent of the Nazi effect, we cannot indulge in the conjectural history and suppose that the young Hitler-émigrés who left Germany and later did prize-winning [Nobel] research would have done work of the same significance had they stayed. Indeed, as more than one said in the course of my interviews with them, having being forced to leave Germany turned out to be the best thing that could have happened to them. The United States provided an attractive and hospitable climate for their work, and for many, ample resources as well."

[28] Max Perutz and Ernst Gombrich would have had no immediate reason to flee to the UK, the Vienna Circle would have remained active in its old location longer, et cetera. According to Ben-David (1984/1971, 138), "[i]t is a futile question to ask whether the shift [of the academic center, from Germany to Britain and the US] could have been reversed [or prevented], if the Nazis had not taken over the country, as the universities were part of the system which made the Nazi take-over possible".

[29] There is an extensive literature which deals with the themes mentioned, and the references given are just illustrative.

While the shift of the center prior to 1933 may be regarded as an open, 'academic' question, the shift after 1945 is basically undisputed and well documented. The reasons why the gradient in scientific viability which had separated the old from the new continent during much of the 20th century has remained in place, even half a century after World War II during which science and higher education have evolved markedly, are less well understood. Had the higher education systems on both sides of the Atlantic been equally effective, there wouldn't be such marked differences in performance decades after WWII (Herbst et al. 2002; Herbst 2004).

The shift of the center, from Germany to the US, may have had something to do with the preoccupations of European nations at the time (1914–1945), but this is not how Ben-David reasoned. The center moved because of constrains which Ben-David judged to be intrinsic to German higher education and science and extrinsic to the corresponding institutional systems within the US. We have already covered a range of constraints intrinsic to the German system, but these constraints cannot be properly understood—and the comparative advantage of the US versus the German system cannot be assessed—outside of a comparative context. Hence, we shall briefly have to focus on the US higher education system and its evolvement.

Within the territory of the US, colleges of various dominations and curricula were necessary and common in the course of the colonization of that vast continent, and they originally served the function of secondary—or vocational—schools. Because tertiary education institutions evolved, so to speak, from secondary education institutions, and because secondary eduction institutions were founded in great numbers to serve this wide territory, tertiary education institutions had, in principle, a large base from which to develop and the competition among these institutions was, from the very beginning, a basic feature. Some colleges gained status as tertiary education institutions and developed eventually into prominent universities.[30]

The emergence of research universities dates, as we have seen above (p. 195), from the period just after the Civil War (1861–1865), and it was inspired by the model of the German university. The German model, however, was not simply transplanted: it was fused with the English inspired college system. Ben-David refers to the fact that many American students went to Germany to be exposed to German scholarship and to German laboratories, but he also makes clear that most of these foreign students were not bothered too much by the inherent—constraining—features of the German university which only affected scholars with plans to remain part of the German system (Ben-David 1984/1971, 140). The US research universities were based on intra-institutional graduate schools alongside their colleges, a "crucial step in the importation of the European model" as seen by Ben-David (1984/1971, 139), and they retained a focus on teaching, on close student-teacher-interactions, which eventually proved to be beneficial—perhaps even decisive—when the focus on research became more prominent in the face of mass higher education and retrenchment in the 20th century.

[30]Harvard (founded 1636), William & Mary (1693), Yale (1718), Princeton (1746), University of Pennsylvania (1749), Columbia (1754), Brown University (1764), University of North Carolina (1789, the first state university).

I have referred to, above, to the irony that "modern science had its first blossoming in the scientistic inclined societies of England and France, but it bloomed [...] in the 'ascientific', and at times even 'antiscientific' environment of the German university" (p. 194). A second convolution, ironically enough, appears to have occurred in a system designed to foster learning but that eventually excelled in the field of research. This transformation of a teaching oriented to a research oriented institutional setup was mainly unintended and unplanned, but it is interesting to note why it did occur.

In Ben-David's notion, expansion and the associated competition were forces which drove science and which were seen as primary factors for the dominance of German science during much of the 19th century. These forces were equally at work in the US as soon as the form of the research university was established. Once US science had reduced part of the gap which separated it from German science, US science became a strong competitor. Early in the 20th century when the biotope of German science did not provide for an easy expansion of the species 'science', in spite of the new institutes of the *Kaiser-Wilhelm-Gesellschaft*, US science, by the sheer size of the region within which it operated, was still in a position to expand. Everything else being equal, it was just a matter of time until the center of the scientific world would move to the US.

Once US science gained a dominant role, and as soon as English had established itself as the *lingua franca* in the world of science and had relegated other languages, particularly German and French, to its fringes, US science was in a firm position to perpetuate or expand its dominance. American professional or disciplinary societies and their associated journals, many of which were founded in the 19th century, acted as platforms for the worldwide exchange of ideas and information among the respective scholars or professionals, and these platforms may have had the side effect to shape the associated discourse. However, US science did not become dominant just because of its size and the new hegemonic role of the English language. Size may have played a role in the first part of the 20th century, but in the second part of the past century neither size nor scientific societies or journals can serve as an explanation for the continued dominance of US science *vis-à-vis* the combined science community in Europe. It was the departmental structure and the collegiate culture of the US research university, according to Ben-David, which acted as a competitive edge against the German university and its chair system.[31]

[31] One should add here that the US university, in contrast to the German university, was also much more liberal regarding the fields or specialities which could be assembled under the umbrella of a university. Harvard university, for instance, included Landscape Architecture in its program one year after the American Society of Landscape Architects was founded in 1899. In the late 1990s, the Swiss Federal Institute of Technology in Zürich decided against a program in landscape architecture on the ground that such field is not 'academic' enough (the decision was reversed a few years later). See in this respect also Chap. 9 by Ivan Chompalov.

The Atlantic Split

There is a certain consensus on both sides of the Atlantic that the perceived performance and quality gradient that separates US research universities from most of their European peer institutions is real, i.e. "basically undisputed and well documented".[32] In 1968, Jencks and Riesman (2002/1968, 513) state that "The American graduate school has become the envy of the world, a mecca for foreign students and a model for foreign institutions". The notion of the "envy of the world" was taken up 1991 by James J. Duderstadt in his statement before the US Subcommittee of Postsecondary Education (SOPE 1991) and in his later publications (Duderstadt 2000, 61); and the notion resonates further in the essays of Charles M. Vest (2005, 259) and Harry R. Lewis (2006, xi). The statements corroborate Ben-David's position (Ben-David 1972).

On the European shore, one does not find corresponding statements by reputable authors. This is not due to the bashfulness of Europeans, it is due to the common perception that the European universities have lost their former preeminence, certainly after 1933, and have not been able to regain it since. The eminence of institutions is not easily assessed properly: it will depend on the viewpoint one assumes and on the methodology one employs.[33] However, Nobel prices, bibliographic indicators, 'foot votes',[34] and a range of qualitative assessments by reputable observers point to the fact that the "Atlantic Split" is not a fiction (Herbst 2004).[35] Knowledgeable scholars will use such measures to form their own comparative perception of quality, and on the basis of such perceptions, communicated over the years by a range of scholars, one can readily assume that the reported assessment of US scholars and senior administrators cited above is not far from the truth.

To respond to this perceived performance gap, a joint declaration of European Ministers of Education, convening in Bologna in the summer of 1999, was issued which called for the creation of a European higher education area by the year 2010. This Bologna Declaration had the objective "of increasing the international competitiveness of the European System of higher education", primarily through the promotion of mobility of students and faculty, and the promotion of cooperative

[32] There are, of course, scholars and senior administrators who tend to belittle this quality gradient, or they claim that educational systems are difficult to compare because they tend to serve different aims.

[33] A detailed argument along these lines clearly lies outside the scope of this Epilogue.

[34] The 'foot vote' of academics may or may not be taken as a proper indicator of institutional quality: academics migrate towards the more prestigious departments, but they can also be 'bought' or lured to move by prospects of higher salary, lower teaching loads, better working conditions or homesickness. Max-Planck-Institutes, for instance, are prone to lure (back) well established senior scientists by offering them extravagant employment conditions to the detriment of the junior scholars (Münch 2007).

[35] Additional measures exist which might serve as quality indicators: quality of incoming students, drop-out (or graduation) rates, quality of graduating students, academic employment prospects, student-faculty and staff-faculty ratios, degree of internationalization of faculty and students, et cetera.

measures regarding quality assurance and institutional development. To support this objective, a range of measures was called for, which were refined in subsequent biannual ministerial conferences,[36] to be adapted and implemented locally.

In the following year, the European Council convened in Lisbon in March 2000 and set the agenda "to make the European Union the most dynamic and competitive area in the world" by the year 2010. Both the Bologna Declaration and the Lisbon Agenda initiated an ambitious 10-year plan to revitalize and improve the economic base of the European Union and to use education—and higher education, innovation and R&D—as engines and production factors in this process. As part of this modernization effort, the old idea of an International Institute for Science and Technology (IIST), molded on the famed MIT and first discussed and approved by the North Atlantic Council in 1960 (Brumter 1986, 180), was taken up. A European Institute of Technology (EIT) was to be founded, but like its predecessor, the IIST (Krige 2006, 208–225), it could not gain momentum within a Europe with national ambitions and could not be implemented in its 'red brick' version. Instead, the project is now being pursued in virtual form (General Secretariat 2007).

The redefinition of the EIT-project goes hand-in-hand with a redefinition of the entire Lisbon Agenda. Midway to the 2010 target, it was acknowledged by the European Commission that the original objectives were too ambitious. In the Spring of 2005, after realizing the futility to meet the original objectives of the Lisbon Agenda, the strategy was "relaunched [. . .] after initially moderate results". The relaunched process was now "streamlined and simplified" to focus on just a few main targets, particularly those regarding science funding and the raising of employment levels. But irrespective of these developments, we should register the fact that two large and top-down initiated movements are underway, launched through the Bologna Declaration and the Lisbon Agenda, in an attempt to address deficiencies of European higher education and research and to close a perceived "competitiveness gap with the US".[37]

The Shaping of Universities

In their comment on the English university, Halsey and Trow (1971, 68) state that "Ideas, once built into a social organisation, tend to persist and to resist organisational change. Universities are no exception [. . .]". In the face of a pervasive globalization, or of what we perceive as such, we tend to assume that technology or customs are 'progressing', replacing old and deficient modes of operation by superior modes. But this is not necessarily—and moreover fortunately not—so. Culture

[36]Prague (2001), Berlin (2003), Bergen (2005), London (2007), Louvain-la-Neuve (2009), Budapest and Vienna (2010), and Bucharest (2012).

[37]There are national top-down initiatives as well; in the UK, the Research Assessment Exercise should be mentioned (see Chap. 8 by Andrew Abbott); and in Germany, an Excellence Initiative was launched (see Chap. 6 by Richard Münch).

is part of the collective cerebrum that our societies require to function. But we can also state that higher education systems are culturally entrenched, perhaps to such degrees that systems become partially dysfunctional.

The import of the German model of the research university had an impact on US higher education which would last for at least half a century (Kerr 1994a; Parsons and Platt 1973). But the German model was not simply transplanted, it was superimposed onto existing structures (Graham and Diamond 1997): the colleges remained, and new graduate schools were formed. This duality within the same institution, undergraduate education on the one hand and graduate education on the other, appears to be one of the main distinctive features which separated US higher education from their continental European peers. The European—or German—notion of general education, once the central focus of the 'minor' faculty, philosophy, and subsequently relegated in large measures to the German high school, the *Gymnasium*, made undergraduate and liberal education almost an alien feature of the German university. In contrast, general education is still a part of the undergraduate curriculum of the American research university of today, in spite of more prosaic tendencies (Lewis 2006; Khurana 2007).

The duality of undergraduate and graduate education is not the only feature which separates US institutions from continental European universities: faculty-student ratios—and by implication also faculty-staff ratios—developed quite differently on the two sides of the Atlantic. Friedrich Paulsen reports that in 1830–40, faculty-student ratios at Prussian universities were around 1 : 18 overall,[38] and in 1900 1 : 21.[39] The general picture is that faculty-student ratios tended to deteriorate over time. At the University of Zürich, for instance, the figures deteriorated overall between 1930 and 1980,[40] and the same applies to the Swiss Federal Institute of Technology in Zürich.[41] By the year 2000, leading European universities were characterized by very unfavorable faculty-student ratios.[42]

The picture just sketched stands in contrast to the situation we find in the US.[43] The leading state universities attempt to keep the faculty-student ratio around 1 : 20, and the private research universities around 1 : 10, in spite of mass higher education

[38] With 1 : 7 in the faculty of philosophy, 1 : 12 in the field of medicine, 1 : 32 in law, and 1 : 40 in theology (Lexis 1904; Paulsen 1920/1906).

[39] With 1 : 16 in philosophy, 1 : 15 in medicine, 1 : 59 in law, and 1 : 17 in theology.

[40] From 1 : 18 (1930) to 1 : 28 (1950) to 1 : 47 (1980) (Stadler 1983).

[41] The corresponding figures are the following: 1 : 19 (1930), 1 : 35 (1950), and 1 : 23 (1980) (Bergier and Tobler 1980); the reversing of the trend was short lived, as the subsequent figures show.

[42] RWTH Aachen (1 : 83), Technische Universität Darmstadt (1 : 50), TU Delft (1 : 54), Universität Karlsruhe (1 : 48), Technische Universität Wien (1 : 67), University of Zürich (1 : 63), Swiss Federal Institute of Technology in Zürich (1 : 34) (Herbst et al. 2002).

[43] At the time President Eliot took office (1869), Harvard was characterized by a faculty-student ratio of 1 : 13; at the time he left office (1909), the corresponding indicator was 1 : 12 (Lewis 2006, 35). Today Harvard University has a faculty-student ratio of 1 : 14 (Harvard University 2008). In the case of the Stanford University, the ratios were: 1 : 16 (1900), 1 : 17 (1930), 1 : 21 (1950), 1 : 10 (1980), and 1 : 10 (2000); see: http://www.stanford.edu/about/facts/chron.html.

(Trow 1970) and in spite of current retrenchment tendencies. Faculty-student ratios are considered quality indicators (Astin 1993), and it is the declared policy of most institutions to improve them or to keep them at acceptable levels.

In contrast to European institutions, American universities were able to retain a structural—or morphological—setup which had existed since the formation of modern research universities. The primary force behind this tenacity was a focus on teaching and an ideal of learning which rated personal development higher than the ability to absorb facts, demanding more coaching than lecturing, more diverse exposures than doctrine. But the focus on teaching and learning had unexpected implications. It helped to foster and eventually to retain a research culture which generally depended on close face-to-face contacts, small research teams, and flat hierarchies. The consequences were that the imported German culture started to thrive in the US, while in continental Europe the Humboldtian culture proved more and more fragmentary or hollow (Ringer 1990/1969) (see Chap. 9).

Towards the end of Ben-David's life, the "Golden Years" of US higher education had given way to a new act in the theater of learning. The world and its political climate had changed slowly, a new era opened up under the leadership of Margaret Thatcher (1979–90), Ronald Reagan (1981–88) and Mikhail Sergeyevich Gorbachev (1985–91), bringing about *Perestroika* (1986) and *Glasnost* (1988), the fall of the Berlin Wall (1989) and the dissolution of the USSR (1991), but also a range of changing perceptions on how to understand and guide economies, markets, natural monopolies and financial systems. Milton Friedman, who had influenced Ben-David's thinking after the two had met in the late 1950s,[44] became a sort of guru for many who thought to redefine capitalism, the Chicago School of economics exerted great influence on modern economies and, in due course, higher education itself changed or had to redefine itself.

In 1986, the year that Joseph Ben-David died, these changes were not yet visible to the extent they are now. Technological advances, coupled with the emerging globalization of economic activities, allowed for the thought to abandon the 'red brick' versions of universities and research institutes and to replace them with virtual forms (Herbst and Schmitt 2001) which could affect Ben-David's notion of center versus periphery. Virtual institutions do not yet compete with their 'red brick' sisters, but there is a clear tendency in this direction (see Chap. 9 by Ivan Chompalov). The development of information technologies has opened opportunities to separate, to some extent, laboratory work and data analyses; to concentrate data in globally accessible data repositories; to communicate almost in a face-to-face form when separated by distance; to work "around the clock" on the same project by various teams of different 'red brick' institutions.

However, with all these technological advances, with all exchange programs and international cooperations, teaching, learning and researching remains foremost a locally—culturally—determined activity. Because technology is ubiquitous in many regions of our globe, it is not the technology that will generally make the difference,

[44] Friedman was a Fellow at the Center for Advanced Study in the Behavioral Sciences, Stanford University (1957–58).

but the local—non-technological—conditions.[45] Academic freedom, invoked today frequently, will play a role in the shaping of the local, non-technological conditions. It is a form of “institutionalized individualism” (Parsons and Platt 1973) which allows the members of the university, its faculty and students, to pursue their roles and functions and to protect them from undue intrusions (Connelly and Grüttner 2003). Academic freedom is far from being normalized, and there should exist localized conceptions which impact on the curriculum, on the way electives are handled, courses offered or students admitted, or on the way institutions are financed or resources are allocated. The ‘center’ may not shift to another location in the 21st century, to China, for instance. Rather, we my have an opportunity of a distributed center, diffused over the entire globe, with institutions where kindred minds could meet.

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[45] It is clear that certain science is technology intensive and can only be pursued with the proper equipment. On the other hand, there is also ample evidence that technology can be used as a substitute for thinking.

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