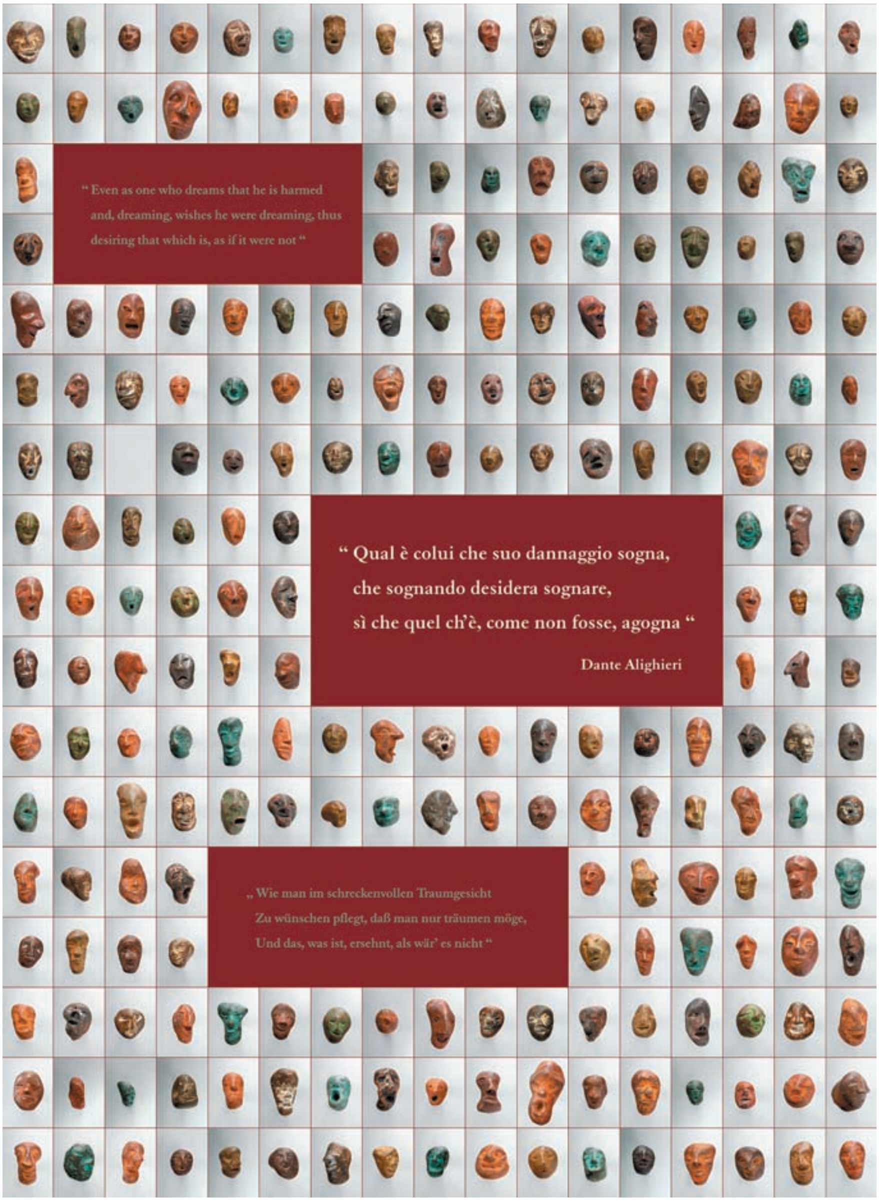

Edited by Yehuda Elkana, András Szigeti and György Lissauer

Concepts and the Social Order

Robert K. Merton and the Future of Sociology

CEU PRESS

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Book Concept and Preface

In June 2008, the 38th World Congress of the International Institute of Sociology was held in Budapest at Central European University. During the conference, very many high-level lectures were imbued with ideas that originate in the work of Robert K. Merton, yet Merton's name was seldom mentioned. I considered this phenomenon a case of "obliteration by incorporation" (OBI), and was looking for ways to counteract this, probably, inevitable trend. In the meantime I learned that Craig Calhoun is editing a very impressive volume of essays about the work of Merton, as is also Peter Hedström in Oxford.

Together with Craig Calhoun, Harriet Zuckerman, Helga Nowotny and Björn Wittrock, we decided to orchestrate a book, dedicated to the work of Robert K. Merton. Instead of choosing a restricting focus from among the many areas of the Mertonian oeuvre, like Sociological Theory, Sociology of Science, Sociology of Knowledge, Intellectual History and more, we thought of asking contributors to write about an idea or concept which influenced their work, but one that in addition to the personal, also points to future importance in the way Sociology in general is moving.

Harriet Zuckerman, in her paper in this volume, has convincingly shown what we observed may have been a purely local and incidental occurrence, and that actually Merton's name is mentioned very often, and his work much cited, in the case of the Matthew effect, but also in general. This may have disproved our generalization, but the results were surely worthwhile and intellectually rewarding. Reading through the whole volume in one go now, I feel very gratified and happy with the outcome.

The usual task of the Preface might have been to mention other important publications dedicated to the Mertonian oeuvre in the past thirty years, the last being Craig Calhoun (ed) "Robert K. Merton: Sociology of Science and Sociology as Science" Columbia UP 2010, but this task has been obviated by the fact that Arnold Thackray, Rivka Feldhay and S.N. Eisenstadt thoughtfully mention all these events and books.

Another task of the Preface would have been to give a brief outline of the papers included. This task too has been taken over by the Introduction of my young colleagues, who did the editing work for the book.

Two very remarkable papers, given at the workshop, could not be included in the volume—each for a different reason; Robert C. Merton: "Observations on Financial Economics drawn from Robert K. Merton's Concepts"; and Craig Calhoun: "Merton and Bourdieu: an Unexpected Convergence."

Two young artists: my composer son Amos Elkana, and the Berlin sculptor and painter, a friend of Amos's and mine, Alexander Polzin, whose paths crossed that of Robert K. Merton, and both of whom were so deeply impressed by him that they claim the meetings have influenced their life and work, decided to prepare a joint "Hommage to Robert K. Merton" which are included in this volume on the front cover and in the accompanying CD.

And, finally, if you allow me a personal note: the world of Mertonian ideas, and the deep friendship between us, had a deep impact on my own work, and therefore I thought it appropriate to work on this as my last project at CEU, my last before retiring, after having headed the institution for ten years.

This book is another dedication to Robert K. Merton: a great man, a wonderful intellectual mentor and a warm friend.

Yehuda Elkana
Berlin, 2010

Note to Sound and Sculpture

Amos Elkana and Alexander Polzin

Amos Elkana

Sound

In 1974, when I was seven years old, I received a letter from America. On the envelope was written my name which was preceded by the title Master. It impressed me immensely. It was the first time I was treated with such formal respect. The letter itself was even more impressing. It was beautifully typed in with a typing machine. The lines were all over the place but in perfectly coherent order—from top to bottom, diagonal and backwards. It was a very funny letter and in it was also a little ditty that I was instructed to learn by heart. The letter was signed: Uncle Bob. In connection with the work of my artist friend, and as I have done as a child myself, I recorded 23 friends on their first attempt at reading this ditty:

"A tutor who tooted the flute, tried to tutor two tutors to toot. Said the two to the tutor, 'Is it harder to toot, or to tutor two tutors to toot?'"

The composition is available at www.amoselkana.com.

Alexander Polzin

Sculpture

In connection to several very personally impressive conversations with Robert K. Merton, and in company with the work of my composer friend, I found this old bowl that was used as a wash basin and filled it with 237 unique faces. The heads lie on top of the following three lines from Dante's Divine Comedy:

Qual è colui che suo dannaggio sogna, che sognando desidera sognare,
sì che quel ch'è, come non fosse, agogna;

Even as one who dreams that he is harmed and, dreaming, wishes he were dreaming, thus desiring that which is, as if it were not (translated by Allen Mandelbaum);

Wie man im schreckenvollen Traumgesicht zu wünschen pflegt, daß man nur träumen möge, Und das, was ist, ersehnt, als wär' es nicht (translated by Karl Streckfuss);

Mint aki rossz álmát igaznak véli, és álomnak kívánja, s ami úgy van, minthogyha úgy nem volna, csak reméli (translated by Mihály Babits).
.

Introduction

This volume is many things in one. First of all, it is, as Yehuda Elkana has expressed in the Preface, a tribute to the scope of Robert K. Merton's work and the influence he has had on both the work and life of sociologists around the world. This is reflected in each chapter, showing the range of fields Merton has contributed to and the personal impact he has had on sociologists. The volume thus provides an introduction to Merton the sociologist and Merton the man. The combination of the personal and the scientific makes this also a study about the sociology of sociological knowledge. That is, to paraphrase Anna Wessely in chapter two, a social practice approach to doing sociology. Based on a workshop that brought together colleagues who have been collaborating for many years, the volume provides an insight into knowledge production in the field of sociology. This in itself is a tribute to Merton, as an analysis of knowledge production through the lens of a contextualized review of an author's life's work would be a very Mertonian enterprise.

The volume begins with a personal reflection by Arnold Thackray, juxtaposing his own and Yehuda Elkana's career paths with that of Robert Merton. This personal history is also a fragmentary history of the field, presented through "Paradoxes of Robert Merton." The paradoxes show how Merton's life mirrors his work, and vice versa, thus presenting the final paradox: that the historical and social context in which Merton lived is reflected in his own work, including his rejection of relativism and his simultaneous faith in scientific objectivity.

Through the next three chapters, the volume continues with history and social context, an exploration of sociology in three very different countries, different in the way their scientific communities have developed and evolved. First, Anna Wessely provides a picture of sociology and the role of Merton's influence—or lack thereof—in communist Hungary. One of the themes of this book, as it is clear from Yehuda

Elkana's *Preface* and the concept of the workshop, is the extent to which the phrase coined by Merton, *obliteration by incorporation*, holds true in Merton's own work—paradox 2 of chapter one. Wessely shows that this, indeed, applied in the case of Hungarian sociology, while pointing out key particulars in the way the field developed. In the third chapter, Jean-Louis Fabiani opens with a similar question: "Why do French sociologists seldom quote Robert K. Merton?" In order to explain, Fabiani takes us on a tour of French sociology during the post–World War II period, describing what he calls the "double rejection of Durkheim" and "the history of science *á la Française*." His conclusion is similar to Wessely's: "Although [Merton] is almost absent from contemporary debates in France, his concepts have permeated the practices and the implicit epistemologies of many of us. We are Mertonian without even knowing it…" But what is also evident from the two chapters taken together is the dividing line between European and American sociology. While Merton tried to bridge the gap between the two, Wessely says that he "[failed to serve] up the European heritage of the sociology of knowledge in a dish that could whet the appetites of American […] students..." and Fabiani notes that in Europe "[h]is work was implicitly included in a very nebulous vision of 'American sociology'" and thus, on the face of it, ignored.

In another country, on another continent, the history of sociology takes yet a different turn. Dhruv Raina, in chapter four, explains how in India, the history of sociology was embedded in the country's colonial heritage. Even after independence, the intellectual links with Britain were too strong to give any significant space to other influences. Raina suggests that in addition, Merton's concerns, at least until the 1970s, did not overlap with the concerns of sociologists in India, thus leaving Indian sociology largely free of Merton's influence. While sociologists were left cold by Merton, technocrats developing India's science policy were very much attracted by two related but separate Mertonian concepts: The first, that science is both a social and an autonomous institution, suggested to them "the promise of ensuring … the immanent development of science and society." The second, scientometrics, developed from Merton and Sorokin's work (1935), provided a tool to evaluate and award scientific research. Through the rest of the chapter, Raina adapts the Mertonian question on the relation between science and religion to the non-Western, colonized context of

India and explores both the results and the issues involved in such an investigation.

The authors of chapters five through nine consider a number of Mertonian themes and concepts, re-evaluating them, adapting them, highlighting their continued relevance and thus opening a well of possibilities for new research. There is no better place to start than with Cynthia Fuchs Epstein's chapter on the contribution of Merton's key concepts to the analysis of sex roles in society. Through her own work, Epstein shows how the concepts coined by Merton can be applied to the analysis of any given group in society. That is because, as Epstein explains, Merton did not "advance predictive models" like "many grand theorists," but instead his "directive was to discover and understand possibilities." Predictive models are determinist, and thus if they fail, the whole theory crumbles. On the other hand, to understand the possibility of something is to discover what it is now and the opportunities it opens up. Epstein, building on Merton's "analysis of micro processes by which individuals and groups maintain their advantages of power and control," shows how, often unwittingly, we maintain the structures that set in place the role of women and men in society but also the opportunities those very same patterns of behavior offer for change.

Merton, Epstein notes, has been "labeled a theorist of the status quo." In chapter six Rivka Feldhay reminds us of Merton the historian. Merton's doctoral thesis, the thesis that must be in the race for the greatest number of references in this volume, concerned Protestantism and science in seventeenth century England. The "hidden presupposition" in the thesis was that "science lagged behind in the Catholic world" during that period. Feldhay goes about meticulously dissecting two commentaries on Thomas Aquinas's treatise on faith, showing a perceptible change in Catholic epistemology. By re-situating 'authority', Feldhay concludes that the "historization and relativization of all knowledge relevant to faith" have become possible. While she only makes a passing reference to the fact that her study focuses on a time that the "Church establishment saw as [the height of its struggle for survival," the hidden presupposition is that, contrary to the implications of divinity, the sociology of catholic knowledge is and should be a rich field of study.

Building on Merton's thesis, Rivka Feldhay strengthens the view that understanding the epistemology prevalent in a society can explain the extent and the way in which science is hindered or enabled in its 'autonomous' development. In chapter seven, Helga Nowotny suggests a more dynamic, two-directional view of the relationship between science and society. She takes another Mertonian concept, sociological ambivalence, and shows how it has been and continues to be the cornerstone of the relationship between society and science. The passion of scientists pit against moral values and economic requirements "generate the circumstance in which […] the conflict between contradictory norms […] can [only] be accommodated by oscillation of behavior." New structures and modus operandi are constantly created to facilitate the sometimes contradictory needs of scientists and expectations of society. Ambivalence, however, is never resolved, only displaced. But, Nowotny argues, "[r]ather than seeing in every manifestation of ambivalence immediately the 'dark side' of science or interpreting it as an inherent ethical deficit that calls for new ethical guidelines on how to translate expertise in the natural order into virtue in the moral order, we should first analyze ambivalence as what it still is: incompatible expectations and demands that arise from contemporary changes in the social structure of science." [pp.??]

In chapter eight, Gabriel Motzkin demolishes what has been the essence of the relationship between science and society. It is the ontology of science that she questions, but social epistemology must change as a consequence. We had understood science, and thus valued it, in its modern incarnation: as an attempt to seek out the truth about our universe. To be precise, the complete truth. Motzkin argues that in our post-modern state we are in a position to recognize that completeness is not within the reach of science. Science cannot give us the truth, certainly not the complete truth. It shows us how things work, within the realm of what is knowable, and thus acts as a tool to achieve our goals. It is then science as technology and not science as an explanation of life or existence itself. "The consequence," Motzkin concludes, "of repositioning science as a technology is then the rebirth of rational metaphysics as an enframing activity." That is one. The other is that "truth-seeking is [an] esoteric" activity, not empirically available, and thus "not accessible to most people." [pp??] This does not, of course diminish the argument about the ambivalence in society's relationship with science, but it is arguable that it is this recognition, that science is a tool, that has given voice to the view that society should have greater control over scientific activity.

In the second part of his essay in chapter nine, Yaron Ezrahi provides further flesh to Motzkin's ontological repositioning of science. He points out that although the "idea that seeing is the safest way to factual reality [...] has dominated our culture mostly since the seventeenth century [...] it has never been really defensible." Experience is only meaningful within the conceptual world. Without a theoretical reference, and thus a normative context, factual evidence has little truth value. Both Motzkin and Ezrahi muse over the effects on society of loosing science as a neutral reference guide, an external authority on which democratic politics can lean on. Both appear to point in the direction of what Ezrahi describes as "the reemergence of beliefs about causality and reality that are more audaciously unaccountable to material standards of evidence and to reason as it was understood and cultivated by Enlightenment culture." But Ezrahi does more. In the first part of the chapter he reminds us of the "normative structure of science" as described by Merton (1973), which, in the words of Merton-Nowotny, acted as the levers through which sociological ambivalence was handled. These, "universalism, communism, disinterestedness and organized skepticism," have all, as Yaron Ezrahi shows, been dismantled both in the way that science is organized internally and in the way it is perceived externally. He meticulously describes the processes through which this paradigmatic shift in the relationship between science and society has come about and suggests that "civic epistemology" has become the means through which, as Nowotny would put it, society handles the underlying ambivalence. As the result of the ontological shift and the new epistemology responding to it, science has lost its position—as Motzkin puts it, it has been repositioned as technology—and thus, Ezrahi concludes, "the uses of contemporary science as a political and policy resource" have been "significantly devalued."

Chapters ten and eleven continue to pursue Mertonian concepts, the "Matthew effect" and "repetition with variation" respectively, but employ a particular Mertonian tool to do so: sociological semantics. In chapter ten, Harriet Zuckerman takes us on a contextualized history of the Matthew effect. Beginning with her own involvement and the possibility of having suffered from the Matthew effect at its inception, Zuckerman goes on to suggest that while the Matthew effect may not be an example of OBI, it has been subjected to the effect of other Mertonian concepts, such as "the serial diffusion of ideas and terminology

[…] via mediated source" or the "Palimpsestic Syndrome." This is, as Zuckerman shows, an almost inevitable consequence of the fact that the "[d]omains of social life claimed to exhibit Matthew Effects are as different as public health and health care, nominations and awards of Oscars, the effects of education and acquisition of reading skills, career attainment in science, the clergy and prostitution, sports and organized competitions, bibliometrics including the distribution of citations among nations and in citations, the origins of pay differentials, the effects of taxation and the distribution of wealth, and as an explanatory variable in the reception of ideas." This may just be the result of "cumulative advantage," but more likely, as Zuckerman puts it, "[i]t exemplifies [Merton's] eye for a telling social phenomenon that, despite its generality, goes unnoticed by others, his skills at laying out its distinctive properties, his ability to recognize and to elucidate the mechanisms that bring it about and perpetuate it and then […] his skill at inventing evocative terms that make the phenomenon visible, comprehensible and usable." [pp. ??]

As Harriet Zuckerman, Charles Camic in chapter eleven undertakes a study of a "linguistic term ['repetition with variation'] […] from two sides: first, by examining the focal term's *social origins*, identifying which social groups used the term, along with when, where, and how they did so; and second, by charting the term's *paths of diffusion*, i.e., its reception and subsequent evolution, changing meanings, dissemination or disappearance." We find that "repetition with variation" was a term in use prior to its being introduced by Merton, in fields other than sociology. Camic shows how "Merton […] sociologically broadened the concept, imparting to it a more generalized meaning than it had in the various separate literature where the expression had previously circulated." He goes on to show how Merton's name has ceased to appear along the scholarly use of the expression. By now, we would come to expect this to be a case of OBI. However, Camic's analysis points out that the continued use of the term is derived not from Merton, "but from *other sources*, among them some of the very strands of work that formed part of Merton's own encounter with 'repetition with variation.'" This is thus a case not of OBI, but of VFB, "the trajectory of a 'vanishing family branch.'" Camic attempts to reconstruct why the Mertonian concept of repetition with variation had fallen into disuse almost immediately after its first appearance. However, perhaps more importantly, the chapter may serve "[i]n the spirit of Merton's remark that 'the resurrection of a term fallen into disuse is an integral part of the development' of the social sciences."

This volume demonstrates the extent to which Robert K. Merton transformed the study of the sociological mechanisms of knowledge production. It is therefore fitting that the volume concludes, in chapter twelve, with Shmuel N. Eisenstadt's review of Merton's contribution to, and transformation of the sociology of knowledge. In chapters two and three, by Anna Wessely and Jean-Louis Fabiani, we began to see the split between American and European sociology. Eisenstadt provides more depths to this division, showing how sociology of knowledge developed in the inter-war period in Europe and to some extent in America. He then goes on to show how sociology of knowledge has almost ceased to exist in the post–World War II intellectual atmosphere, as a result of, among other things, the ever greater particularization of science. Having shown the field's disintegration, Eisenstadt goes on to point to a path that may regenerate and bring to life a more differentiated and integrated sociology of knowledge. This is Eisenstadt's inspirational legacy to a new generation of sociologists. Shortly before submitting the manuscript we learned that Shmuel N. Eisenstadt passed away on September 2nd, 2010. He will be missed by students, colleagues, and friends, many of whom are contributors to this volume.

We wish not to take sides in the Elkana-Wittröck vs. Zuckerman debate played out on the pages of this book about whether, in a classic case of a self-fulfilling prophecy, Robert K. Merton has been Obliterated by Incorporation in contemporary sociological scholarship. Instead, looking ahead, this volume proposes to add to the cumulative effect of Merton-inspired publications, ensuring that concepts developed by Merton will continue to bear his name and that a new generation of sociologists will be Mertonians and will know that they are.

György Lissauer
András Szigeti
Budapest, 2010

The Paradoxes of Robert K. Merton: Fragmentary Reflections

Arnold Thackray

Yehuda Elkana's intellectual agendas for the first decade of his faculty, professorial life, like my own, took place a generation ago in the benign context of Bob Merton in his sixties—that fruitful, pre-senescent decade, through which in our turns Yehuda and I have recently journeyed. Together with Bob we participated in numerous conferences and publications, ranging the Anglo-American world from Palo Alto to New York and Oxbridge to Jerusalem. Those activities were driven by Yehuda's restless energy and intellectual curiosity, but grounded in the bedrock of Bob Merton's deep erudition. The earliest enduring fruits of our intellectual journeying together were the conference in 1970 in Jerusalem resulting in *Science and Values* and the conference in 1974 at the Center for Advanced Study in the Behavioral Sciences at Stanford University, with its outcome *Toward a Metric of Science* (Mendelsohn and Thackray 1974; Elkana et al. 1978).

In my own career, this decade concluded with a paper jointly-authored with Bob, on the "Paradoxes of George Sarton." At the time I was the editor of *Isis*, and the reviver of George Sarton's *Osiris*. And, of course George Sarton had been Bob's own mentor—or more accurately, Bob was the first person to escape from Sarton's study in Widener Library with a Ph.D. in his hand; a triumph compounded by Sarton's publication of Bob's thesis in *Osiris* itself. In the "Paradoxes of George Sarton" Bob and I sought to come to terms with this erudite, prolific, protean figure in both our pasts (Merton and Thackray 1972; Merton 1970).

In homage both to Bob and Yehuda—two erudite, prolific, protean figures in their turns—this paper offers some preliminary, fragmentary thoughts on the "Paradoxes of Robert Merton," in the hope of stimulating discussion.

Paradox 1. He's dead but he won't lie down!

My own serious engagement with the history of science, as someone slowly discovering the existence of the field—then abandoning my secure job in Yorkshire, England, for precarious, full-time graduate study—covers the years 1962 and 1963: "the last years of the 1950s." (The Free Speech Movement at Berkeley did not erupt until the fall of 1964.) In England, as in the USA, the history of science first began to be a populated field of Ph.D. study at this time. The city of Philadelphia and its Jewish community feature largely in this narrative for—unlikely as it sounds—it was a Jewish, Marxist-sympathizer and native Philadelphian who introduced me to the study of the history of science in Yorkshire, England. Jerome Ravetz has upended norms of science history and policy analysis from his *Scientific Knowledge and its Social Problems* (1971) to his most recent work as an advocate of "Post-Normal Science."[1] At the urging of Ravetz, I moved to Cambridge, and in 1966 I became the proud holder of the second Ph.D. Cambridge University ever granted in this field.

The 1962 launch of a UK–based annual called *History of Science* provided a further signal of this newly-stirring, Sputnik and Baby-Boomer facilitated, awareness. The inaugural article in its second volume—required reading for any English student launching their study in this field—was by England's leading professional figure in this new academic specialism, A. Rupert Hall: "Merton Revisited." The opening phrase—referring to the 1938 publication in *Osiris*—was "A quarter of a century ago. […]" However, its burden was that:

> In 1939, one year after Merton's monograph, there appeared the *Études Galiléenes* of Alexandre Koyré. No contributions to the history of science could be less alike […] as Merton summed up one epoch, that of the socio-economic historian, Koyré opened another, that of the intellectual historian. […] Among the younger historians of science his [Koyré's, is] the dominant influence. (Hall 1963, 10)

The message was clear and stark: from an intellectual point of view, Merton was dead! And of course to a twenty-four-year-old, like myself

[1] "Post-Normal Science" is "a mode of scientific problem-solving appropriate to policy issues where facts are uncertain, values in dispute, stakes high and decisions urgent," complete with its own notational system for quantitative data. See, for example, Risbey 2005.

at the time, "a quarter of a century ago" literally meant "before you were born," which was functionally equivalent to "dead." By the fall of 1967, at the urging of my Cambridge, England mentors, I was in Cambridge, Massachusetts for a one-year visit—it turned out to be a very long year—to acquire what they called my BTA or "been to America" degree. With Yehuda in the next-door study at Harvard, I began my real education in professionalism, American style, and in intellectual curiosity, Elkana-style. Falling in love with the opportunity of American life, by the summer of 1969 I was a newly-tenured 28-year-old on the faculty of the University of Pennsylvania, in Philadelphia, which town became my home for more than 40 years—and was, of course, the birthplace and youthful home of Bob Merton himself.

An October 1969 conference at the American Academy of Arts and Sciences—for dialogue with a visiting delegation of Soviet historians of science—found me back in Cambridge, Massachusetts, as one of a hastily-assembled group of American experts.[2] Solemnly seated around the sides of a square of tables, we opined and commented. I was immediately and intensely struck by the erudite, intelligent comments of a lively, distinguished-looking American seated across the room, who was deeply engaged in the discussion. "Who is that?" I whispered to my neighbor. To my astonishment, the reply came, "it's Bob Merton." Convinced that individuals of antiquated views live short lives, I could only croak, "But surely he's dead!"

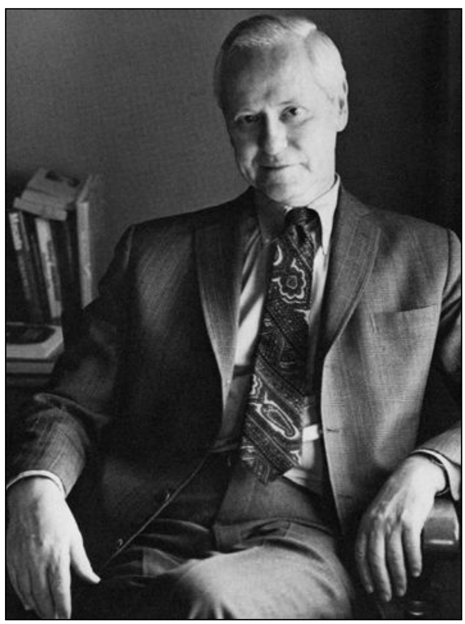

Illustration 1: *Robert K. Merton circa 1970*.

[2] October 16–18, 1969, Soviet American Conference on the History of Science, American Academy of Arts and Sciences. Six Russians and sixteen Americans, including Robert Merton, Arnold Thackray, Everett Mendelsohn and I. B. Cohen were listed as participants. Evidence found in unprocessed archives at the American Academy of Arts and Sciences by its dedicated archivists.

I tell this tale at length, because it offers a metaphor for Bob Merton's paradoxical relationship to the history of science as a discipline.

- The Merton thesis is the most famous dissertation in the field;
- The Merton thesis has remained in print for over 70 years;[3]
- The Merton thesis has been honored and discussed in numerous publications and conferences—beginning with Hall's article of 1963, and stretching through the conference Yehuda convened in Israel in 1988, a 50th anniversary symposium in *Isis* and the 400 plus page volume *Puritanism and the Rise of Modern Science* edited by I. Bernard Cohen in 1990, to name only the most obvious.[4]

Surely, one says, Merton is alive! And yet, if you consult any historian of science and ask about the vital intellectual traditions, you will hear not of Merton, but of the sequence from Koyré to Herbert Butterfield and Hall, on to Bruno Latour, Barry Barnes and Steven Shapin, or at Harvard from L. J. Henderson and James Bryant Conant, to Thomas S. Kuhn and again to Shapin.

How to explain this? Perhaps the key is that Bob Merton saw science itself—the intellectual content of the subject—as unproblematic, in the sense that it is given by nature and awaiting discovery. In contrast, science not as the *ding an sich* but as evanescent human perceptions has been and does appear contingent, troubling, and troubled to every influential stream of discourse among those coming of age in the six decades since World War II—the scientists' war—began.[5]

Compounding this ineluctable background reality, the foreground of historical scholarship has shifted. George Sarton—lone pioneer—could imagine compiling a work that would stretch at least from Aristotle to the physics of the recent past. Bob Merton had only eight predecessors as recipient of a Ph.D. in sociology from Harvard, and no predecessor from Harvard, or anywhere else in the English-speaking

[3] In the twenty-first century alone there have been another paperback edition (New York: Howard Fertig, 2001) of Merton, *Science, Technology and Society* (1970) and a Chinese translation (2000), which was digitized for online use (2005).

[4] International Workshop on Fifty Years of the Merton Thesis, May 16–19, 1988, Institute for the History and Philosophy of Science and Ideas, Tel Aviv University, in cooperation with the Van Leer Jerusalem Institute; Rosenberg 1988; Cohen 1990.

[5] This unease is apparent in several of the other contributions to this volume, especially and eloquently those of Yaron Ezrahi and Gabriel Motzkin.

world, as a Ph.D. in history of science. For Bob Merton, to tackle *only* seventeenth-century England for his dissertation research was to take on a narrow subject, suitable to a serious if fledgling scholar. No more!

In 1967 when I first attended its sessions, the History of Science Society drew less than one hundred people to its annual meeting, and *Isis* had few companions in its role as an academic journal. Today, historical scholarship concerning science, or any other subject, is very different. It is voluminous, vigorously proliferating, and necessarily highly specialized, sharply focused, and sensitive to every nuance and particularity of life as lived in particular places, at particular times, by particular people. If this were not enough, quantitative approaches have been selling at a deep discount for some time, in the world of professional history.

As late as the middle 1970s, the advent of computers and languages like Fortran seemed to offer purchase—witness Robert Fogel's *Time on the Cross* (1974). The body counts of Vietnam, the gargantuan growth of scientific activity itself, the necessary but forbidding efforts to capture its realities in quantified streams of Science Indicators, and the ubiquitous mathematics-based views of available reality, from Long-Term Capital to the sequences impoverishing us in the last several years, have all taken their toll. Historians know in their bones that elegant hypotheses and their mathematical supports have only pitiful purchase on human enterprise. Hence the paradox of the Merton thesis—simultaneously cited and ignored.

Paradox 2. Obliteration by Incorporation.

As the meeting at the American Academy indicates, much of the power of Bob Merton was in his presence—the man himself. This is something to which an endless stream of students, colleagues and collaborators can eloquently testify. It is signaled in events that range from the early harbinger of his appearance as "Mr. Sociology" in a *New Yorker* profile in 1961 through to the unprecedented award of the National Medal of *Science* to Merton, a sociologist, in 1994 (Hunt 1961).[6] I can

[6] Winners of the National Medal of Science are chosen from a pool of candidates submitted by the United States scientific community, winnowed by a committee of twelve scientists appointed by the President of the United States and two *ex officio* members, the director of the Office of Science and Technology Policy and the president of the National Academy of Sciences.

testify to the powerful expression of the man himself in his deeply thoughtful editorial interventions, personal counseling, and career promotion—the father-like care one longs for in a mentor, but so rarely finds in academic life. The paradox of course is that the enormous, long-term impact of such activity is hidden forever—obliterated by its incorporation in the lives of others.

Paradox 3. The local boy as cosmopolitan.

Bob Merton was born on the street where I live—to be more precise, across the street and five blocks down from Society Hill at 828 South Third Street, in Philadelphia, Pennsylvania. At the start of the twentieth century, this was the Jewish ghetto area. In Bob's own quotation he was born "'almost at the bottom of the social structure' in the slums of South Philadelphia, to working-class Jewish immigrants from Eastern Europe" (Merton 1994, 3–4). Confirming evidence comes from historian Maxwell Whiteman, who cited a 1902 study of a "Jewish block" in South Philadelphia.

Illustration 2: *Children on a street nearby Merton's childhood home, circa 1918.*

> "A few minutes walk south of the original Society Hill tract arose one of the vilest Jewish immigrant neighborhoods [...] notorious for their decaying wooden 'bandboxes,' the offal and animal excrement encrusted in their wooden sidewalks and cobblestone alleyways. [...] Eight bathtubs were in the 75 houses 'of which 3 were only used in the summer.'" (Whiteman 1973, 241).

Bob's given name was Meyer R. Schkolnick. Schkolnick pére ran a precarious milk-butter-and-eggs shop in the ground floor of 828, but later was further reduced in circumstance, and became a laborer in the nearby Philadelphia (U.S.) Navy Yard. Meyer R. Schkolnick—that is, *the* Bob Merton—lived within a 15–20 block area of inner city Philadelphia for the first twenty years of his life, and Temple University, his undergraduate college was within walking distance of his home.

Illustration 3: *Meyer Schkolnick dressed like Little Lord Fauntleroy.*

The paradox is that this apostle of the cosmopolitan was truly local — aside from his subsequent foray to Tulane University in New Orleans to land a job in the depth of the Depression, he lived his long life within the Boswash corridor—and almost all of it within one hundred miles of his birthplace.

Paradox 4. The Jewish WASP intellectual.

In the most literal sense, Philadelphia is the home of the WASP—the White, Anglo-Saxon Protestant—named, intensely studied and dissected by that classic example of the breed, Digby Baltzell (1964).[7]

[7] During much of his long career at the University of Pennsylvania subsequent to this classic work, Baltzell chose to investigate his own social class and the decline of its influence. In contrast, his mentor Merton paid virtually no scholarly attention to his own class and ethnic background.

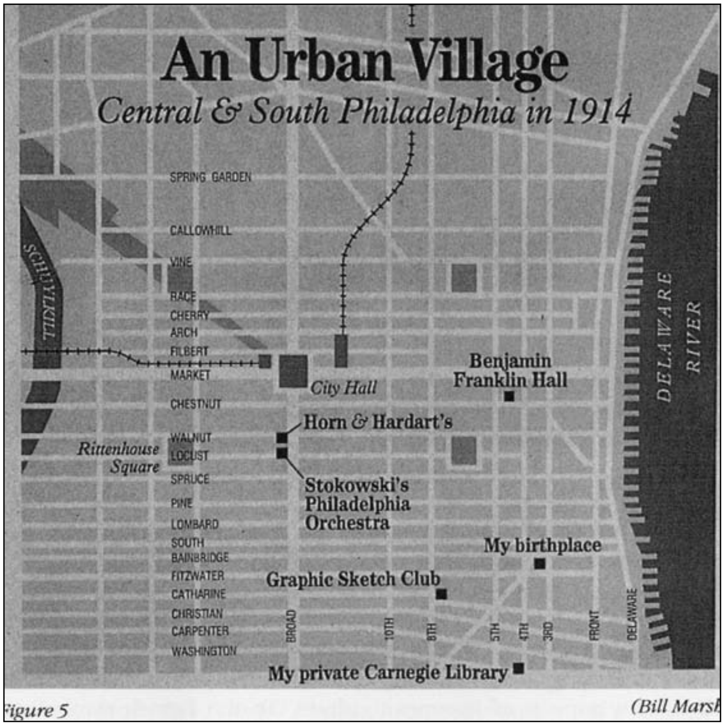

Illustration 4: *Merton's neighborhood*.

Baltzell was of course a very early student of Bob's at Columbia in the late 1940s and himself the very epitome of the Protestant Establishment he described. If Digby Baltzell sounds a not-quite–WASP enough name, no such qualms can surround Robert King Merton—the

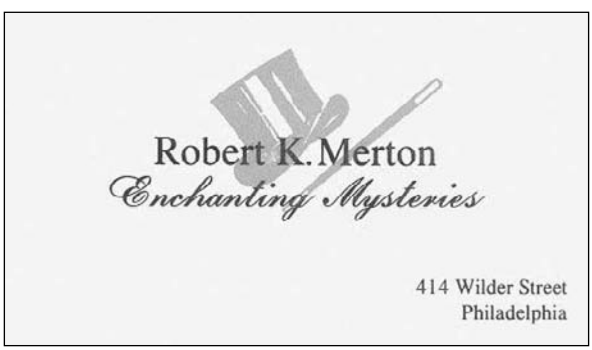

Illustration 5: *Bob's business card as a magician*.

name with which Meyer R. Schkolnick had reinvented himself a decade earlier, while still an undergraduate.

In all of this we may trace the ambition and determination of his Jewish mother. Bob himself has charmingly recounted how MR became RM, and how in turn Robert Merlin, youthful magician, became Robert K. Merton. More mysterious is the K—King, the middle name. More in irony than paradox—the Jewish boy born at 828 South 3rd Street, that is in the blocks between Christian and Queen Streets, transmuted into Robert King Merton, the quintessential WASP (Merton 1994). First in the marvelous *New Yorker* profile of 1961 (tenured, already famous, and part of the new, post-war liberal intelligentsia who were making the world safe for democracy in the American idiom) and later in his marvelous Haskins Lecture delivered at the age of 83, Bob would slightly lift the veil on these mysteries.

Illustration 6: *Merton as a Harvard University graduate student clad in white linen suit.*

Here we may simply note how this son of an intensely, local urban, Jewish, Marxist tradition was remarkably comfortable simultaneously espousing the values and the manners of the ruling elite, as befits a king. The paradox is that he *was* a king—or at least a prince among men.

Paradox 5. The relativism of scientific absolutes.

There are interesting contrasts in the life of Yehuda Elkana—child of Budapest, the Holocaust and Israel. Like Merton, Elkana is an adopted name —"God has purchased". Yehuda Elkana is a name as unmistakably Jewish in its connotations as Robert King Merton is WASP, but equally redolent of spirit and the self-fulfilling prophecy of youthful determination.

A quarter century, an ocean, and a tragic chapter in the human story separate the adolescence of Yehuda from that of Bob. To Bob Merton, as to many of the emerging Jewish academic elite in the United States, in the 1930s through the 50s, Marxism and science were the two most promising candidates that might—objectively in their eyes—be chosen to replace the standard Christian sources of authority, and thereby soften ethnic or racial antagonisms. The story of *Science, Jews and Secular Culture* in mid–twentieth century American intellectual history is a major, complex unstudied area. Indeed the primitive state of our understanding is well captured in David Hollinger's book of this title, where he reports his own belated realization that Bob Merton—a major object of his attention—was in fact Jewish (Hollinger 1996, 80–81).

For Yehuda, the objective certainty in which science might serve as centerpiece was not available. Our earliest conversations together—in Cambridge, Massachusetts forty years ago—displayed his probing, restless quest to understand how *certain* knowledge might be possible in a world undeniably haunted by uncertainty, contingency and what Winston Churchill once called "the light of perverted science." That concern with relativism and yearning for clear absolutes has characterized Yehuda's discourse over the years and lies behind his own enduring flirtation with Bob Merton's writings.

Though it is not fashionable to admit it, the Roman Catholic Church is the most enduring large-scale organization in the world. Its leader—an academic of no mean capability—has recently and memorably spoken out against the "dictatorship of relativism" that has slowly engulfed Western, elite and academic life over the past several decades (Ratzinger 2007, 22).[8]

Were Bob here today, in all probability he, like Pope Benedict, would lament the prevailing "dictatorship of relativism" with its associated political correctness, and faddishness. Bob's belief in science as an absolute, with a social system that can therefore be analyzed, has proved immensely fruitful for policy studies. The final paradox is that Bob's faith in science is philosophically similar to the faith in revealed religion, with which it is often contrasted, and which it sought to replace. Whether it will prove as historically-enduring, time alone will tell.

[8] Ratzinger was soon thereafter elected Pope Benedict XVI. The Pope frequently alludes to this homily.

Looking for Shoulders to Stand on, or for a Paradigm for the Sociology of Science

Anna Wessely

The conveners of our workshop asked participants to "write about an idea or concept which influenced their work but that, in addition to the personal, also points to future importance in the way Sociology in general is moving." Since, as far as I know, Merton's sociology has had no impact on my work (a matter of fact that I am not proud of at all) and I cannot see either which way sociology in general is moving except that it seems to have lost its public appeal, I should have politely refused the invitation. However, being the only local participant, I thought I might be able to add a local touch to the meeting by trying to offer a tentative explanation for this *nonrelation*.

1

I began my studies in sociology in Hungary in 1978. In this country, sociology had no continuous tradition; it practically ceased to exist in the 1950s, and began to re-emerge very slowly in the 1960s.[1] The Department of Sociology at Eötvös Loránd University—for a long time the only one in the country—was re-established in 1970, offering the first undergraduate courses in 1972. By the time I entered the department, there had already been identifiable personalities in the profession, but no schools of sociological theory or research to speak of. Most of our professors had begun their careers in other disciplines, switching to sociology in the 1960s–70s. As a consequence, each of

[1] There exists a very informative collection of 19 interviews with the generation of sociologists who began research in the 1960s. The interviews were made in 1987, but published only two decades later. In these recollections, Merton's name turns up only twice, moreover in contexts that merely reflect awareness of his fame and significance but do not suggest any impact of his work on the two interviewees who mention him, although one of them even attended Merton's lectures at Columbia in 1965 (Rozgonyi 2008).

them relied on a self-styled assemblage of fragments of social and sociological theories and research procedures that they had put together for themselves—based mainly on their own readings and, in a few cases, on occasional study trips abroad. In retrospect, the chief principles of theory selection seem to have been firstly, practical applicability in fieldwork and survey research and, secondly, ideological neutrality. One might say that, although probably unaware of it themselves, they closely followed Merton's advice put forward in 1957, namely, to create the systematics of sociological theory by way of a "highly selective accumulation of those small parts of earlier theory which have thus survived the tests of empirical research" (Jones 1983, 137 from Merton 1957). The legitimizing strategy of Hungarian sociology at the time of its re-establishment diverged from the usual path: instead of attempting "to identify a jurisdiction unshared by other disciplines" (Merton 1973, 51), its practitioners emphasized how sociology could support and deepen research in the already institutionalized fields of academic study and policy-oriented research. This collective career pattern and legitimization strategy resulted in a taken-for-granted trans-disciplinary orientation of sociology in this country. Moreover, sociologists could reckon at that time with sustained and lively public interest in everything sociological, particularly in regard to contemporary Hungarian social and political problems.

The curriculum at the university included the subject 'History of Sociological Theory' with Weber and Durkheim in the centre, and then proceeded to enumerate the various *ism*s. Here Merton was duly listed in the rubric "structural functionalism." Our professor, a philosopher turned sociologist, put particular emphasis on Merton's conception of middle-range theories that seemed to hold out the promise to be able to conduct theoretically informed research without being entangled in ideological disputes. It was at this time, in 1980, that a Hungarian translation of the 1968 edition of *Social Theory and Social Structure* appeared in a truncated form. The truncation concerned the chapters on the sociology of science. These were left out, on the alleged grounds that some of them had been already published separately four years before—namely in the form of a stenciled booklet under the auspices of the Marxism-Leninism Department of the Ministry of Education. The booklet was distributed free of charge among instructors in the so-called "ideological" disciplines in institutions of higher education, but not allowed into commercial circulation. (The history of the various series of such 'state samizdat' editions still awaits a detailed analysis.)

Judging from the scarcity of reviews and the number of citations in Hungarian social science journals, *Social Theory and Social Structure* did not have an appreciable impact in this country. But, as a matter of fact, by the time it appeared, Mertonian concepts like 'reference group' and 'anticipatory socialization' had already been integrated into the vocabulary of Hungarian sociologists; the chapter on social structure and deviance duly became a staple element in all introductory courses in sociology, authors regularly cited the book in the literature review sections of their academic papers, and every social science specialty had its own favorite sample of articles by Merton that they kept returning to. And, most importantly, the paradigm for functional analysis, the distinction of manifest and latent functions, in particular, had formed the core conceptual frame of sociological research since the late 1960s. It is no exaggeration to suggest that the identification of the latent function of social mechanisms, institutional arrangements, legal regulations, government policies, etc. was regarded as *the objective* of all respectable sociology worth doing. This was a clear classic case of OBI as defined by Merton: "an acronym which stands for Obliteration of the source(s) of ideas, formulations, methods, or scientific findings By Incorporation in current canonical knowledge" (Merton 1993, 311). In this case, however, obliteration was an honorific gesture as well in the sense that Merton's concepts were taken to represent the core of sociology as such—Merton was the discipline itself incarnate.

But the Hungarian translation of *Social Theory and Social Structure* did not catch on. In spite of its clear and didactically conscious style, it was no easy read for a Hungarian audience in the 1980s. It was not a textbook but a collection of major studies, revised, expanded, and sometimes commented upon by the author himself. For instance, the 1967 essay *On the 'History' and 'Systematics' of Sociological Theory* was "an enlargement of an introductory section of his *Social Theory and Social Structure* (1957); and that section was in turn an enlargement of the points made in a 1948 discussion of a paper by Talcott Parsons" (Jones 1983, 138). This authorial strategy may be fully justified by didactic purposes; moreover, its result was probably informative for people familiar with the scholarly and social discourse in the

United States from the late 1930s through the 1960s.[2] Without this background, however, it was not easy to fathom what the presumably changing targets and stakes were, who was being addressed and in what kind of situation, what the main points were that Merton wanted to make, and why there were all those caveats and circumspect qualifications. Thus people in this country tended to *use* the book *Social Theory and Social Structure* a lot, working through this or that chapter, depending on their research interests, but nobody seems to have regarded it as a major theoretical statement, a conceptual framework, the construction of which might demand close reading, systematic analysis, and confrontation with other works intensively read, discussed, and applied since the 1980s, i.e., Bourdieu, Habermas, Luhmann, and rational choice theory.

2

There was, however, another circle of scholars in Budapest in the 1980s where not Merton's general sociological theory but his sociology of science was in the foreground. That circle was constituted around the philosopher Márta Fehér by younger philosophers and sociologists interested in the philosophy of science. I think that practically everybody who works in the fields of the philosophy and sociology of science in this country today can be safely called either Márta's student or her student's student. The meetings and courses she organized discussed the "sociological" turn in post-Kuhnian philosophy of science, focusing on the strong program, social studies of science, and labora-

[2] Whoever wants to reconstruct the development of Merton's ideas concerning the sociology of knowledge, will have to face similar difficulties, unless the reconstruction is conducted in a fully equipped American library. The essay „Paradigm for the Sociology of Knowledge" (1945) in the collection edited by Norman W. Storer (Merton 1973) is a revised version of the 1937 paper published in *Isis* under the title „The sociology of knowledge." Storer indicates that this essay in his collection reprints the contribution of Merton to the volume *Twentieth-Century* edited by Gurvitch and Moore in 1945. It is then somewhat puzzling to find that it also includes a reference to a book published in 1952. Sztompka also publishes an essay entitled „Paradigm for the Sociology of Knowledge" (1945) in his collection (Merton 1996) that differs from the version to be found in Storer's collection (apart from the indicated omissions), noting that it was „revised from" the same 1945 contribution, but the date of the copyright is 1973.

tory studies. Her invaluable, fascinating lectures and untiringly patient explanations helped us to understand, follow, and eventually contribute to the discourse in contemporary science studies.

With a background in art history and research interests in cultural sociology, my fascination with science studies was linked to the ways it allowed me to grasp science as the practice and discourse of a particular subculture, as a specific cultural form. It was from this particular angle that I kept on reading the relevant literature, drawing my tentative conclusions, and teaching classes in the philosophy and sociology of science at the university. It was years later that I came upon two papers by Görgy Márkus on the author–text–audience relations in science and the historically changing images of science which made me realize that this "personal" viewpoint of mine had been already most clearly elaborated and formulated by Márkus as a specific research perspective on science (Márkus 1987 & 1992).

In the group around Márta Fehér, Merton's name was frequently mentioned but mainly as an important pioneer in the field, more precisely, as a backdrop against which the novelties introduced by Kuhn and post-Kuhnian authors could be best outlined. However, already within that small group, I had the opportunity to notice that philosophers and sociologists related differently to the new sociology of scientific knowledge. For the philosophers, the sociology of scientific knowledge (SSK) represented a novel and convincing form of *externalism* and thus they tended to regard it an updated form of the sociology of knowledge in the sense that it examined the theoretical *content* of scientific claims from a sociological point of view. For sociologists the main point and fascination of SSK were that it approached doing science as a *social practice*, that it investigated how "knowledge was produced in and through mundane interactions between people, as well as between people and reality" (Shapin 1995, 300), that is, how conducting scientific research involved the use of a channel of communication for several simultaneous dialogues: between scientists and their objects of study, between scientists and the conceptual frames of their disciplines, among practicing scientists, and between scientists and their wider audience. The difference can be better expressed in German (or, for that matter, Hungarian): the philosophers focused on *Wissen (tudás)* and epistemological issues, the sociologists on *Erkennen (megismerés)* as a system of social action. For a sociologist, Merton's advice that we had better bracket "the perennial problem of the implications of existential influence upon knowledge for the epistemological status of that knowledge" (Merton 1973, 13) did not seem to represent an unacceptable limitation. What we found constricting in the sociology of science prevalent before the emergence of SSK was most clearly formulated by Joseph Ben-David who declared that "the possibilities for either an interactional or institutional sociology of the conceptual and theoretical contents of science" were "extremely limited" (Ben-David 1971, 13–14). This major difference in our approaches remained unrecognized for a long time. For we all assented to the view that Merton's enterprise of constituting the study of science as a legitimate branch of sociology had been embedded in his efforts to institutionalize and legitimize sociology as social *science*. Since we regarded his ideal of science, that relied on traditional philosophical models of the "scientific method," outdated, we felt justified not just to reject what is sometimes referred to as his "black-boxism," but also to neglect his work on the sociology of science—looked upon as an impressive monument to a superseded stage in science studies. Thus it was only many years later, when I sat down to prepare for my classes in the sociology of science, that I really began to study and appreciate Merton's work. Finally, a last personal remark: I have read Merton's work in the wrong order. Had I begun with *OTSOG* or *The Travels and Adventures of Serendipity*, he would have been among my favorites.

3

When urged to give the title of my talk, I responded that it would be "Looking for shoulders to stand on." Not simply out of admiration for *OTSOG* but because I felt that it described very well one aspect of Merton's work in the sociology of knowledge and science, namely the attempt to turn it into a theoretically informed and empirically enriched, *cumulative* field of study. This effort required a critical review of the existing literature that Merton, in fact, accomplished with unrivalled perspicacity in the 1930s and 40s. But since the creation of a discipline is never a one-man enterprise, he strove to extract and systematize the durable elements in the theories he surveyed in a way that would enable newcomers to this research area to become veritable *contributors* to the emerging field of the sociology of science. As Lewis Coser put it, Merton tried "to salvage the usable intellectual products of a past thinker" by surgically removing "those layers and tissues of a thinker's thought that show the mark of his time, his place, his milieu, so as to be able better to expose that vital core of his message which transcends the various existential limitations that might have entered into his perspective." This operation seemed to be unavoidable in confronting "the diffuse, scintillating, but often confused and confusing heritage of European thought," the central core of which had to be disentangled and "transmitted to American students and practitioners alike if they [were] indeed to work within a living tradition" (Meja and Stehr 1999, n. 32 in Coser ed. 1975, 87).

The point I want to make in this paper is that Merton's efforts have not born fruit in this respect, that is, in serving up the European heritage of the sociology of knowledge in a dish that could whet the appetites of American—and not only American—students and practitioners of science studies. It is generally accepted, even by critics of his monopoly position in the field, that "it was Merton who first opened up science to sociological investigation" (Pinch 1992, 1132) and that, from 1945 up to the 1970s, his approach to science "as a social institution with a characteristic ethos" was "the only maturely developed framework for the sociological study of science" (Barnes 1972, 9–10). It is also beyond doubt that to this day "[c]urrent work on science, whether as institution, normative order, moral community, sub-culture, set of peers or status group, is indebted to Merton's account of the norms of science, or else to his analysis of the system of honorific reward through which the norms are sustained" (Barnes 2007, 179).[3] But the alternative menu that he tentatively offered in his early papers on the sociology of knowledge, particularly in reviews of the work of Mannheim and Znaniecki and in the "Paradigm for the Sociology of Knowledge," has not enjoyed a similarly eager reception, and Merton himself seems to have abandoned it as a research program, even though he kept alluding to it in papers that presented an overall survey of the state of the sociology of science or of the styles of sociological work.

Storer suggests that the narrowing of Merton's broad scope of interests in the sociology of science, that was still evident in the "Paradigm" essay, to issues more closely connected to the social structure of, and status struggles within, the scientific community was motivated by two main considerations. Firstly, that *Wissenssoziologie* "had fallen into a disarray by the 1930s," finding itself in "a maze of inter-

[3] Barry Barnes 2007, 179–192, 179.

nal contradictions, a cul-de-sac from which escape had to be sought by beginning anew with different questions" (Merton 1973, xiii); and, secondly, that "Merton evidently became persuaded that [...] without a sufficiently well-developed model of the social structure of science, there was no way to generate theoretically important questions that could use systematic data on scientific development to advantage" (Merton 1973, xviii).

It stands to reason that the question of how knowledge is shaped by past experience and actual interest is too general and vague to serve as the foundation of a research program. But Mannheim and Znaniecki offered something more specific and Merton's reviews show that he did recognize the fruitfulness of their formal sociological approach to the production, social use, and dissemination of knowledge. The term formal sociology is used here in the sense Simmel defined it as the analysis of the social forms of the relationships and interactions among individuals and groups, of the dynamics of their interactions, the mutual expectations and rival strategies, and the stabilizing roles and institutions that emerge out of these interactions.

In his 1920s works, Mannheim systematically applied formal sociological analysis to the structures of thinking; the most promising source for generating operational hypotheses for a research program in the sociology of science can be found in his 1928 paper on "Competition as a Cultural Phenomenon." Here he proposes to put aside epistemological concerns and to focus instead on the competition among individuals, professional groups, institutions, generations, and social strata for the authority to provide the "public interpretation of reality." The most valuable passages from the point of a formal sociological analysis of scientific production concern the various possible forms this competition will assume—among others, a polarization of positions with such possible outcomes as "thinking against each other," reactively growing one-sidedness or mutual adjustment, increasing reflexivity or learning from each other. This is followed by a discussion of the main types of theoretical and extra-theoretical strategies applied by the proponents of rival theories. Mannheim also examines how the political, social or economic competition among various groups will influence theory choice, the dissemination of ideas, the emergence of styles, the dynamics and rhythm of cultural trends. Finally, he returns to the stake involved in this competition, namely which knowledge claims succeed in becoming accepted as true. Here he suggests a formula that is very close to what Bourdieu proposed in his 1975 paper, "The specificity of the scientific field and the social conditions of the progress of reason"[4] that successfully defended Merton's position against the critique leveled at him by the protagonists of SSK: the more intensive the competition, the more merciless the mutual critique of scientists will be, resulting in an unanticipated consequence: the "natural selection" of justified true beliefs.

Although Znaniecki used a self-consciously functionalist language, nevertheless, what he elaborated and classified in his book on *The Social Role of the Man of Knowledge* (1940) is a formal sociological analysis of the interdependence of the various types of knowledge with the authority, competence, roles, and social practices of the various types of *savants,* as these would be defined by the social structure, needs, and expectations of their respective audiences. Merton clearly saw the merits and fruitfulness of Znaniecki's approach. His extensive review of the book amounted to a concise summary of Znaniecki's argument, including a classificatory table of the types of social roles of the men of knowledge, together with short descriptions of the types of knowledge each cultivates and of the types of relationships they maintain with their audience. It is such a perfect summary that I suspect it made most sociologists think they had learned here all they ever had to know of Znaniecki's sociology of knowledge. I can find no other explanation for the scarcity of citations of this truly admirable book.

Reading through Merton's *The Sociology of Science*, there are every now and then allusions, indirect references to the need for studies along the lines proposed by Mannheim and Znaniecki. The audience does not seem to have got the hint. And, beginning with the 1950s, Merton had already invested so much in the study and promotion of the sociology of science as the study of the institutional structure and reward system of science that he would not want to risk it by striking

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[4] This affinity has also been noticed by Dick Pels (Pels 1996). In earlier papers I argued that this affinity derives from their intensive study of Max Weber's work. In a personal conversation, Bourdieu declared that he had not been influenced by Mannheim at all, he had not read anything by him since his student years.

a very different path that would have resulted in putting forward an alternative paradigm for the sociology of science. In this respect, I tend to agree with what Joseph Agassi has recently said of Merton: "He was the insider who was at home in the commonwealth of learning at the cost of avoiding showing his cards" (Agassi 2009, 6).[5]

[5] Joseph Agassi 2009, 6.

R. K. Merton in France: Foucault, Bourdieu, Latour and the Invention of Mainstream Sociology in Paris[1]

Jean-Louis Fabiani

Why do French sociologists seldom quote Robert K. Merton? This paper is an attempt to analyze a recurrent paradox. Although Merton's operative concepts are known and used, his achievements are not really acknowledged in France. To explain this, one has to start with the rejection of Durkheimism that was especially strong in France in the post-WW II context. But one also has to take into account the strength of a philosophical lineage that shaped the whole intellectual field, the "French history of science," as described by Michel Foucault. No real space was allowed to the sociology of science developed by Merton. Last, the sociological mood of the 70s in France was mainly critical and created a kind of scapegoat, the 'American mainstream sociology.' Thus this paper is a tribute to Merton's fruitful notion of obliteration by incorporation, but also a contribution to the importance of creative misunderstanding in the international circulation of ideas.

1

Olivier Martin, a French sociologist who stands among the best in the young generation, noticed a few years ago (Martin 2004) that there was a paradox about Robert King Merton in France: although French sociology is significantly present in his works (particularly Durkheim's concepts), he was never recognized as a major sociologist in the country of the author of the *Rules of Sociological Method*. In another paper, published the same year in the *Revue d'histoire des sciences humaines*, Jean-Christophe Marcel raised a question, after a long research on post-war French sociology: "Why do Stœtzel, Gurvitch, Davy, but

[1] I am indebted to Craig Calhoun, Randall Collins and Laurent Jeanpierre for earlier discussions. I would like to thank István Adorján for his insightful suggestion.

particularly Friedmann, ignore Merton? In all my readings, I did not find any substantive comment of his work" (Marcel 2004). In this paper, I will try to show that the paradox is only apparent. Merton was read, partly in translation, but he was neither really used nor commented. His work was implicitly included in a very nebulous vision of "American sociology" that stressed the applied and ideological dimension of the social sciences. For the less ignorant, Merton occupied a kind of obscure position, intermediate between Parsons' grand theory and Lazarsfeld's applied research. The French sociologists who had had the opportunity to meet him in the USA—particularly his translator Henri Mendras or even Raymond Boudon, close to Lazarsfeld and extremely knowledgeable about American sociology, made very scarce comments.

Drawing on my own works on French intellectual history (Fabiani 1988, Fabiani 2010), but also on a study of critical literature about Merton I wrote for this workshop, and on the renewed interest of young French sociologists toward the historical development of their own discipline and the international circulation of ideas (Laurent Jeanpierre, Jean-Christophe Marcel, Olivier Martin, Romain Pudal, and Patricia Vannier among others), I shall try to understand Merton's presence/absence in France. Before I read Yehuda Elkana's proposal, I imagined that it was a situation that was specific to my country: now I know that it is not, and I'll try to go beyond the case study to offer a reflection on the invention and uses of the notion of "mainstream sociology."

Educated as a philosopher (political philosophy and philosophy of science), I had been attracted to Pierre Bourdieu's after dinner seminar at the École normale supérieure, a very uncommon practice at the time among young "normaliens". Bourdieu had just come back from one year at the Institute for Advanced Study in Princeton, and was still considered by the Althusserian "establishment" at the École normale as a smart but marginal intellectual.

During the first weeks, I heard many notions, completely new for me, that were constantly used by Bourdieu and his *garde rapprochée*, tough men with leather jackets, with the exception of the "artist" Luc Boltanski, a fragile, daydreaming and rather shy woman full of respect for the young master. *Sérendipité*, *prophétie autoréalisatrice*, or more often *prédiction créatrice*, *conséquences inattendues de l'action*, *l'effet Mathieu*. Sometimes Bourdieu would make some ironical remarks about the shortcomings of the *théorie à moyenne portée* (middle range theory), but I could not catch the humor in it. Merton's name, if I remember well, was never mentioned. We heard a lot about Abraham Kaplan (*The Conduct of Inquiry*), a little bit about Erving Goffman ("great observer of small things but poor theoretician" as we were told) and Michael Polanyi (praised for *Personal Knowledge*). The most frequently cited names were Durkheim, Weber, Bachelard, Koyré, Panofski, Canguilhem (known as "Le Cang"), Wittgenstein and Cassirer. American authors present in the textbook *The Craft of Sociology*, first published in 1968 (Bourdieu 1968), almost never appeared in the discussion: they were mainly Bennett M. Berger, Anselm Strauss, and C. Wright Mills. You might say these memories do not correspond with what was really taught at the Sorbonne in the meantime as the seminar located in the 46 rue d'Ulm was simultaneously prestigious and remote, and you would be right.

I should stop with the *Bildungsroman* at this point. I must now broaden the frame and be more precise about the very peculiar history of French sociology, which is not as clear as Terry Nichols Clark recounted it in his book *Prophets and Patrons* (Clark 1973, Fabiani 2005). In spite of its claims, the history of the social sciences cannot always be neatly distinguished from the old history of ideas, which tend to view the circulation of men and intellectual properties as a fairly simple matter. Conceptualizations migrate from one space to another, more often than not through individual actors and become dominant through reappropriations or new uses, which then must be analyzed in a fresh context. Such a view naturally entails some simplifications: theories become hand luggage and the study of circulation gains precedence over diverse negotiations (in the interactionist sense) giving way to the always provisional stabilization of conceptual constructs. Misunderstandings and ambivalences fade from the picture. Obviously, sociological theories are not mere commodities that can be analyzed along lines of a customs protocol.

The study of cultural transfer has enriched the analysis of conceptual migrations by taking into account different contexts and the concrete ways in which ideas are transmitted. Historians have availed themselves of this contribution far more than have sociologists. Analyzing the more or less explicit use of American sociological theory since the 1950s is still a work in progress. Most of what we call "American sociology" in France appears as a continental invention and we have to deal more with what I call “silent appropriations” and “tacit incorporations” than with explicit imports. The task, as it stems from my provisional remarks, is not easy, especially when we deal with Merton in France.

In the first part of this article, I will discuss what I call the double rejection of Durkheimism: first during the inter-war period, then during the reconstruction of French sociology in the early fifties. Then I will move to a very important feature, not so frequently taken into account, except by Michel Foucault, that is nevertheless inescapable if one wants to understand something about the reception of sociology of science in Descartes’ country: the long established and legitimate *histoire des sciences à la Française*, mostly identified with Gaston Bachelard but dating back, in its preliminaries, to Auguste Comte. In the third and last part I will propose an analysis of how the theme of a “mainstream sociology”—in France, *sociologie dominante*—was constructed in order to move almost all sociologists out of the mainstream, towards the margins of the picture. In my first book, more than twenty years ago, *Les Philosophes de la République* (Fabiani 1988), I showed how the crowning discipline, philosophy, had moved progressively from the legislation and the jurisdiction of all sciences to what I called at the time “institutional subversion,” which is undoubtedly another form of jurisdiction. The path was thus traced from the top to the margins. It is not absolutely different from the ostentatious centrality of critical sociology in France.

2. Durkheim’s Double Rejection

I would like to consider whether Durkheimism exists as such in the long term. Contrary to Terry Clark, I think that the Durkheim ‘school’ never reached a powerful academic status. The bachelor’s degree in sociology was created only in 1958 and during Durkheim’s lifetime *la sociologie* was an adjunct to philosophy, mostly allowed by the rapid development of the pedagogical sciences, largely due to the generalization of mandatory instruction in France during the Third Republic. On the one hand, there is a kind of conservative thread that goes from Gabriel Tarde to Bruno Latour over a century which considers Durkheim as the official and dogmatic thinker of the Third Republic, providing the new bourgeoisie with fresh ideological resources. Latour saw him as the organizer of a kind of ‘epistemological police’ (Latour 1998 and 2003) he links, curiously enough, with the history of science *à la* *française*. This included mainly Gaston Bachelard and his followers. Latour has reconstructed Tarde as the new French intellectual hero, because the latter's fluid and rather vague thinking allows room for the formers' "actor network theory." Such an antidurkheimism is not new: it is mainly rightwing, but in history, it has been reappropriated by leftist intellectuals, Paul Nizan being the best example (Fabiani 2006). The hostility of the fellow travelers of the French communist party to sociology in the fifties stems partly from this lineage. But it comes also from the dominant vision in France at the time: American sociology was of a practically and ideologically oriented discipline. In the meantime, quite a few communists got jobs as field sociologists at the CNRS, as sociology was still an illegitimate area of knowledge and did not attract the most elite young minds that still considered philosophy as the only noble occupation. In this way, sociology was a quite open field that many people entered just by chance, as Henri Mendras nicely reminds us in his memoirs reflecting on his own case, or because they lacked the capital to follow 'higher' tracks (Mendras 1995). The French sociologists of the fifties were either mavericks poorly provided with cultural capital or right wing intellectuals a bit nostalgic about glorious France, as Mendras undoubtedly was. There was also room for elite boys like Jean Stoetzel, who had come to Columbia as early as 1937 and who was impatient to toss out the great French intellectual tradition, and to replace it with an American model that was oriented to practical answers: Stoetzel, who funded the *Institut français d'opinion publique*, was a committed anti-durkheimian and scorned "speculative sociology." Unsurprisingly, an ambitious theoretical sociology was unthinkable at that time, and almost all the gifted young people in the *Grandes écoles* and at the Sorbonne preferred "to be wrong with Jean-Paul Sartre than to be right with Raymond Aron," a common saying in France borrowed from Cicero talking about Plato. Aron had arrived at the Sorbonne in the mid-fifties but was still a marginal figure in the academic life.

During the interwar period, Durkheimism had become largely unpalatable to main academic and literary tastes. In a politically polarized intellectual field, Durkheim was the target of both right and far left. The support he got from his direct surviving inheritors, since many of his young disciples had died during the First World War, was not really visible and was largely inefficient. On one hand, Marcel Mauss and François Simiand were marginalized at the Collège de France, which was never a centre of academic power. On the other hand, Célestin Bouglé, the prominent character in the group, had to cope, while serving as director of the *École normale supérieure*, with extremely hostile reactions of the old humanities vis-à-vis the still having to be recognized social sciences. He was more successful, but it was not seen as a Durkheimian victory, since he had always been considered as the most "ambivalent durkheimian." Bouglé organized the first trips to the USA for ENS students. Georges Friedmann and Jean Stoetzel, already mentioned, went there before the Second World War. The idea that things useful for the social sciences could be found in America grew slowly, but not very clearly. In the meantime, the voyage to Brazil started to be (in a more "colonial" relationship) a path for philosophers wanting to go away from philosophy: Georges Dumas, Claude Lévi-Strauss, Roger Bastide among other less famous men sailed there. The trip to Germany, that had convinced generations of young French people that intellectual salvation should be sought in the powerful organization of German University, was not yet a thing of the past. Sartre and Aron had spent a year there, the latter much more lucid and provided with a sociological eye than the former, who appeared more interested in Thuringer sausages than worried by Hitler.

Just after World War II, the trip to America became a distinctive feature of starting sociological careers: Henri Mendras in Chicago, Alain Touraine at Harvard and Raymond Boudon at Columbia, among the most brilliant. Bourdieu was an exception. He went to Princeton only in his early 40s. If some of them imported a lot of fresh resources back to France (one thinks of Boudon coauthoring a book with Lazarsfeld or Touraine bringing back home a gallic version of Talcott Parsons' theory of action), not that many were fully convinced by their US experience. More than that, they brought back authors and concepts mostly absent from the sociological canon—Moreno, Gallup, Linton for instance. World War II was of course a significant experience for quite a few French academics who were to play a major role in the fifties, Levi-Strauss and Gurvitch at the top (Jeanpierre 2004). Post-war intellectual life in France was, to a fair extent, shaped by the New York encounters, as Laurent Jeanpierre has beautifully explained. In the 1950s, sociology as such did not gain much legitimacy in France as Johan Heilbron has clearly shown (Heilbron 2001). It was not really an autonomous discipline, in the institutional sense, but also in Bourdieu's theory of field meaning.

Many sociologists took an explicit anti-theoretical stance: the first was undoubtedly Stoetzel, the most antidurkheimian character in French sociological history. He held the biggest power to reorganize French sociology on the empirical side, with an equal scorn for what was seen as the Durkheimian heritage abroad, namely Parsons and Merton, who were often confused in France. Georges Gurvitch was then the *homme fort* at the Sorbonne. He had developed his own theory, hard to remember now, that needed neither real empirical commitment nor discussion with other theories. To put it bluntly, real legitimacy in the social sciences could be located elsewhere, in Lévi-Strauss' anthropology and in the *Annales* School (Sartre and Beauvoir paid a lot of attention to both Lévi-Strauss and Braudel in their philosophical journal *Les Temps modernes*). Raymond Aron, appointed to the Sorbonne in 1955, was not a central character: although he knew the US pretty well, he seemed not to have been interested much in the American methodological and epistemological debates. Later, in the late sixties, the climate changed a bit. Starting from the first years of the fifth Republic in France and from the Kennedy era, the hostility against American sociology diminished. The US was welcome to bring their empirical rigor and methodological inventiveness, but their theory had to be tossed out and reserved for French thinkers. Two young philosophical minds went along the second option in the mid-sixties, Pierre Bourdieu and Jean-Claude Passeron, young Balzacian Rastignacs coming from remote and rural provinces to Paris in order to legitimate sociology and to become its intellectual leaders (Bourdieu and Passeron 1967). French sociology was re-theorized by both young philosophers, the first clearly a disciple of Max Weber and Raymond Aron and the second a Marxist and bohemian intellectual.

A partial translation of *Social Theory and Social Structure* was ready as early as 1953. It was done by Henri Mendras, University of Chicago student and not the most dedicated Mertonian, in spite of his references to the notion of group applied to the waning peasant society, it was completed only in the mid-sixties. A full translation came out a decade later: this came out first as *Eléments de méthode sociologique* and was completed in 1965 as *Eléments de théorie et de méthode*. Theory thus came later in the process. The book was not a real success, since it came out in the mid '60s, when Michel Foucault, Jacques Derrida, Gilles Deleuze, Pierre Bourdieu and Jean-Claude Passeron published their first books and 'frenchified' social theory. Merton's sociology of science was largely overlooked because it did not fit into a very powerful intellectual frame. France had developed a long tradition of history of science that was more epistemological than sociological. It reunited very different philosophers and sociologists around totemic figures like Bachelard and Canguilhem, which helps explain many features of French intellectual life.

3. The History of Science "à la Française"

In order to understand the strength of the French version of history of science, one has to turn to the description that Foucault left us. In the final days of his life, Michel Foucault was writing a tribute to his mentor and friend, Georges Canguilhem (1904–1995). In fact, he was too exhausted to write an entirely new text and had to recycle quite a large chunk of the foreword he had composed for the English translation to Canguilhem's *Le Normal et le pathologique*. He sent the text to the editor in the waning days of April 1984. It was the last text that Foucault submitted for publication. "La vie, l'expérience et la science" (Life, Experience and Science) appeared in the first 1985 issue of the *Revue de métaphysique et de morale*, one of the two most central philosophical journals in France. Foucaldian scholars consider it a minor text, and this perhaps because Foucault places himself in a kind of philosophical lineage or genealogy, which allows us to locate him on the philosophical map. Perhaps I always liked "La vie, l'expérience et la science" because Foucault brought in the word "sociology," a word and a discipline that he himself did not particularly care for. And he wrote very accurately about *l'institution philosophique*, the philosophical institution, the development of which I had studied in my early works. But let Foucault speak for himself:

> Everybody knows that in France there are few logicians but a fair number of historians of science. One knows too that they have played a considerable role in the philosophical institution, whether it was in teaching or research. But less well known may be something that existed in the last twenty or thirty years, on the margins of this institution, namely a work such as that of Georges Canguilhem. There were undoubtedly more noisy scenes: psycho-analysis, Marxism, linguistics, ethnology. But let us not forget that which pertains to the sociology of the French intellectual milieu, to the functioning of our university institutions as well as to our cultural value system, if you wish. In all political and scientific debates of that strange 1960 decade, the role of phi-

> losophy, and I do not mean here only those who studied philosophy at the universities, was important. Too important, perhaps, according to some. Directly or indirectly, all or almost all of those philosophers owed something to Canguilhem's teaching or with his books. Remove Canguilhem from the picture and you will understand nothing of what happened in French philosophy in the late 1960s, although Canguilhem himself never engaged in any of the debates. This is also true for French Marxism, French sociology (mostly Bourdieu, Castel and Passeron), and the Lacaniens. Moreover, beyond the manifest cleavages between Marxists and anti Marxists, Freudians, and anti Freudians, between philosophers and those members of other disciplines, between university people and non university people, between theoreticians and politicians, there is another and undoubtedly more important dividing line (*ligne de partage*), which exists between experience and knowledge, meaning and rationality, subject and concept. (Foucault 1985, translated by J. L. Fabiani)

Foucault does not hesitate to speak in terms of filiation and opposition, such as Sartre vs. Cavaillès, Bachelard and Canguilhem vs. Merleau-Ponty, and continues back to the nineteenth century: Bergson vs. Poincaré, Lachelier vs. Couturat, Maine de Biran vs. Comte. This cleavage was so entrenched that it structured the French reception of phenomenology. Husserl's *Cartesian Meditations* (lectures given in 1929) were instantly read in two quite different ways: The first reading—Sartre's 1935 paper "La Transcendance de l'ego"—radicalized Husserl through a pure theory of the subject. The second reading, which was mainly interested in the building blocks of Husserls's thought (formalism and intuitionism), was later undertaken by Cavaillès, and will lead to his two dissertations, *La Méthode axiomatique* and *La Formation de la théorie des ensembles*. In spite of later entanglements and increasing entwinement, it seemed to Foucault that these two threads remained deeply separate. There is a subtext here: Sartre did not take part in the Resistance, thus giving special significance to his famous saying that: "We were never freer than during the occupation." Cavaillès was one of the greatest heroes of the Resistance. Foucault underscores what might appear to be a paradox: historians of science, working on the periphery in small provincial universities or even lycées and apparently having no apparent immediate political stakes, still played an important role in the Resistance while the philosophers of the subject remained silent most of the time. Why? According to Foucault, those who had originally raised the question of the foundations of rationality could not then discard the issues relevant to its actual conditions in society when they appeared in such a brutal reality. Foucault sees in the French history of science an echo of the German debate about the Enlightenment: *Was ist Aufklärung*? Saint-Simon's criticism and Comte's positivism may be viewed as a French way of taking up afresh the question raised in the late eighteenth century in Germany by Mendelssohn and Kant in the late eighteenth century. What is at stake with French conceptual philosophy is an enquiry into rationality and reason, which, due to its structural autonomy, can be an agent of liberation only if it can free itself from itself.

Foucault's model is of course incomplete. Many philosophers are not present in his short list of names, particularly Charles Renouvier, the *criticiste* philosopher, who was so important in building the program of a republican rationalist philosophy. Where would you put Derrida in the framework? And Foucault himself? I was struck by Foucault's outline because in my first works I had tried to analyze French philosophy circa 1900 as a system of oppositions between spiritualism, the official French philosophy before the Third Republic, and a mix of rationalism and positivism (which just then was an emerging conceptual construct, linked with the development of universities and the desire to compete with Germany intellectually, following France's defeat in the Franco-Prussian War). As a young and brash sociologist provided with Bourdieu's methods I even identified many structural differences: spiritualists were more Parisian and more affluent, where as rationalist-positivists were more provincial and scions of a "petite bourgeoisie" whose sole capital was of an intellectual variety; they were the sons of primary school teachers, *officiers de santé*, like Charles Bovary or doctors without a dearth of patients. I do not wish to give the impression that Foucault could have a sociological mind. Throughout his life, he fiercely detached himself from social sciences. Consider for instance Erving Goffman, whose work was often compared with Foucault's in France in the mid 70s. Foucault was unambiguous:

> Some people have said that I tried to do the same thing as Erving Goffman in his book on asylums, the same thing, but not as good. I am not a researcher in the social sciences. I do not want to do the same thing as Goffman. He is mainly interested in the functioning of a special type of institution: total institution, asylums, schools, jails. I try to show and to analyze the relationship which exists between a set of power techniques and social forms. Goffman's problem is the problem

> of institution as such. My problem is the rationalization of individual management. My work does not aim to do a history of institutions or ideas, but to examine the history of rationality as it functions in institutions and in people's behavior. (Dillon 1979)

We can find in Foucault's works many asides about sociology, and they are never friendly ones. He is particularly harsh with Durkheim: "Durkheim's old realism is unthinkable for me," he said once.

Nikos Poulantzas, a Marxist thinker, who fought against the "interactionist" implicit dimension of the French philosophy of his time made a counter-intuitive link between Foucault and Deleuze and American functionalist sociology. He wrote:

> They [i.e., Foucault and Deleuze] here meet up with an old tradition of Anglo-Saxon sociology and political science, running from functionalism to institutionalism, from Parsons to Merton, Dahl, Lasswell and Etzioni—a tradition in which the centre of analysis is shifted from the State towards a 'pluralism of micropowers.' Despite the fact that they explicitly developed all the characteristic points of the above vision, these writers remain relatively unknown in France, where political thought has always focussed on the (juridical) State. Indeed, it is this very unfamiliarity, linked with the well-known provincialism of the French intellectual arena, which allows these most hackneyed of ideas to be presented as something new. (Poulantzas 1980, 44)

Poulantzas' viewpoint has interesting elements; French philosophers' effort to stay aside from sociology leads them to ignore some conceptual constructions that existed long before their own "creations." Nevertheless, it would be hard to deny the inventiveness of Foucault's micro-physics of power. It is true however that the French philosophical field has been characterized for a long time by its inwardness and its lack of interest for sociology and political science. Foucault, as other French philosophers, remained trapped in this idiosyncratic attitude.

Foucault's genealogy of the French history of science helps explain two things with respect to the French reception of Merton. The first is the fact that critical sociology is implicitly present in the political stance developed by the conceptual side of French philosophy; philosophy and the social sciences altogether have to contribute to human emancipation. The second is the fact that the reasons for the growth of science must be found rather in epistemological issues than in sociological ones.

4. Inventing the Mainstream in Paris

Pierre Bourdieu's first mentor was Georges Canguilhem, long before he met Aron and decided to become an anthropologist. Bourdieu, deeply installed in the *philosophie du concept* lineage was undoubtedly the first to raise sociology to the center of the French intellectual field. He did this by associating sociology and anthropology in his earlier works on Algeria, but also on Béarn, and in combining the image of a scientist so important in the comtean lineage with the figure of the angry philosopher, as his *Pascalian Meditations* clearly show (Bourdieu 1997). He needed a "mainstream" framework to distinguish himself from what was left from the stereotype of an imperial sociology, and he largely invented it. No single American sociologist had ever dominated the French sociological field, but although he had long been fascinated by the organization of American universities, as it clearly appears in his posthumous autobiographical notes (Bourdieu 2004), Bourdieu wanted to develop a genuinely new brand of sociology, that would be as "theoretically grounded" as the most demanding philosophy, but that would have the evidential strength of a science. There is little doubt now that in spite of ideological differences and opposite styles in public life, Bourdieu and Merton shared some views about what the social sciences should be. The most striking image of Bourdieu's imperial power has obviously much more to do with the well known figure of the public intellectual that Bourdieu quite feverishly impersonated in the last ten years of his life: but Bourdieu's whole theoretical endeavor is somewhat different from his youthful commitments and from his 60s involvement with the Fifth Republic bureaucracy. Besides, although he sometimes harshly criticized the condescending posture of the French philosopher, he kept the main features of it, and he considered theoretical sociology, except when he himself produced it, as second-rate conceptualization. His early knowledge of the functioning of the literary field allowed him also to approach the status of the grand écrivain: *Distinction,* published in 1979, was reviewed as a piece close to the achievements of Proust or Joyce. Bourdieu, who had begun his career as a persistent critic of literary and academic institutions, ended his life not so far from the *grand écrivain* long established model of the public intellectual sharing features with Victor Hugo, Emile Zola and Jean-Paul Sartre. French critical sociology, developed in the late 1960s, had much less to do with morphological dimensions, like in the US where its social base was clearly the result of a growing contradiction between PhDs' high expectations and the actual promise of their career. The most "critical" moment—if one refers to the theoretical meaning of the notion of French sociology—took place in the 1970s when it was still easy for sociology students to get jobs, as compared with more legitimate historians and philosophers. The critical stance was more ideologically oriented as a claim of the weak disciplines to get a seat at the high table. Bourdieu, who became in his last years the unchallenged hero of critical sociology, did not share the anti-science mood of the critical crowd at the time. On the contrary, he always believed that sociology could become as scientific as a natural science and never gave up his hope.

His relationship with Merton was ambivalent. On the one hand Merton was the man at the center of the "Capitoline Triad," the heart of the ideological power of American theory (Bourdieu 1991). Craig Calhoun and Jonathan von Antwerpen have shown that Bourdieu's view of American sociology was far from being precise: "Bourdieu exaggerates the cohesion and dominance of sociology's postwar elites and neglects the extent to which the mainstream only became visible in the 60/70's clashes over it, and in attempts to impose authority that became more effective in the 70s and 80s" (Calhoun and von Antwerpen 2006: 371). And one can be skeptical about the definition of mainstream sociology by Bourdieu. He considered its members as "the organic intellectuals of the dominant class." This statement is far from being grounded in any evidence. I am not sure that the illusion of cohesion and ideological stringency of American sociology is a result of his relatively late actual contact with the US. He had read for a long time big chunks of American sociology and was quite fascinated by its public recognition, and was too clever to overlook the differences between Parsons and Merton. But at the time, Bourdieu was busy constructing his very complicated intellectual space between the top and the margins. On the one hand, he got much more institutional resources from Fernand Braudel who was president of the *École des hautes études en sciences sociales* and from Clemens Heller who was the administrator of the *Maison des sciences de l'homme* than his main rivals, Raymond Boudon, Michel Crozier, and Alain Touraine, although he was one of the youngest of his contemporaries. On the other hand, Bourdieu wanted from the start to appear as a disturber of the dominant order, although his early work was far from any political agenda.

His critique of the *Homo academicus* (Bourdieu 1984) is an example in this respect. He made, through the gathering of diverse data, a clear-cut opposition between the routinized academic and the marginal innovator. It was not necessary to give a formal account of what "mainstream sociology" was: in the mixed bag, one could find Harvard-Columbia, Parsons-Merton, but also a majority of French sociology, devoted to practical tasks or poorly loaded with theoretical insights. In the Bachelardian tradition, Bourdieu's fight for science was a moral combat, a permanent struggle against the everlasting evils and renascent dangers of the epistemological "obstacles" and the ideological traps hidden along the road (Fabiani 1991). Dominant, mainstream ideas were the most salient manifestation of such evils. At this point, it was no longer necessary to develop any kind of historical knowledge about American sociology: naming it "mainstream" and dominant was largely enough. That is the reason why Bourdieu mistook Merton for an eminent member of WASP society, the middle name King adding a touch of royalty to the dominant mainstream line. A King is not a friend for a French republican intellectual; on the contrary, it may give murderous ideas against the mainstream dominating power. As a matter of fact, Merton nicknamed himself King when he played the magician in his childhood. His real name was Meyer Robert Schkolnick, which would have fitted in the picture less nicely. One of the main features of Bourdieu's sociology, the sociological objectivation of the social sciences, was not far from Merton's proposal about sociology of science, and we have a hunch, although there is no real evidence about it, that Bourdieu was aware of this link. However, it was easier for him, especially in the late 1960s and 70s, in order to create his own sociological niche, to appear as the fiercest opponent of American mainstream sociology, a real fiction on the banks of the Seine, since as Olivier Martin and Jean-Christophe Marcel have clearly established, Mertonian sociology was never really activated in France. It was easier for French sociologists, even when they were real admirers of American sociology, to use a very vague and even blurred definition of it. The best example is Raymond Boudon's use of the notion: he evokes quite obsessively "American sociology (Parsons, Merton)" in his books, as if it existed as a cohesive set of theory and method (Boudon 1987). Boudon had first hand knowledge of sociology at Columbia but deliberately chose to give an inexact account of its diversity. In France, Merton appeared most of the time as a sort of an intermediary between grand theory and applied research, like an automotive mechanic who would fix the complex parts of Parsons' theoretical engine. It was of course a very inaccurate portrait of the author as an intermediary. On the contrary, Merton, while focusing the sociological attention to explanatory issues and epistemological problems of concept building, developed a type of sociology quite different from the standard structuro-functionalist corpus. It helped open an original space in the social sciences based on transposability—a central notion in Bourdieu's general theory—on explanatory power and on comparative construction. Such a space was largely autonomous from Talcott Parsons's big system, but French sociologists did not care. The sociologist who gave the more precise attention to Merton was Philippe Besnard, who did a complete study of the concept of anomie, from Durkheim to Merton and gave a rather pessimistic conclusion about the merits of the concept, insisting on the numerous flaws of Merton's definition (Besnard 1986, Besnard 1991). This highly scholarly study was definitely serious and accurate, but it did not capture the real spirit of Merton and did not address his sociological inventiveness. So the genuine interest in Merton manifested by Besnard did not lead to anything fruitful. It was a real *rendez-vous manqué*.

Asserting that Bourdieu was the only one to take Merton really seriously in France might be considered as a gentle provocation; in fact, it is not, and this time, I have some evidence for my statement. In the late 80s, Bourdieu paid an original and quite counter-intuitive tribute to Merton with his *Animadversiones in Mertonem* (Bourdieu 1989). Why Latin? Bourdieu, trained as a philosopher, had worked on Leibniz in his pre-sociological youth and wanted to remind us of the German philosopher's own *Animadversiones* about Descartes: this reveals a lot about the importance given by Bourdieu to Merton. There is no doubt about the central position occupied by the author of *Science, technology and society in seventeenth century England* (Merton 1970) in the history of sociology:

> One of the great merits of Merton is to have established that the history of science must be analyzed sociologically… In contrast to his radical critics, Merton has established furthermore that science must be examined in its two-fold relation, on the one hand to the social cosmos in which it is embedded—the external reading—and on the other to the social microcosm constituted by the scientific universe, a relatively autonomous world endowed with its own rules of functioning, which

> must be described and analyzed in themselves—the internal reading. On this point the proponents of the "strong programme" represent in reality a regression. (Bourdieu 1989, 288)

Although Merton has not entirely broken with the analytical categories imposed on him by the field of scientific production itself, he is, according to Bourdieu, much more advanced than the new and trendy young people who have created the field of science studies. Of course, the tribute to Merton appears as a Bourdieuan strategic move toward established sociology when he feels threatened by arrogant newcomers as Bruno Latour. But I think that there is a deeper connexion with Merton as it is shown in one of Bourdieu's last texts, *Science de la science et réflexivité* (Bourdieu 2002) that has undoubtedly Mertonian overtones.

In providing the sociological professional world with workable tools and a set of epistemological insights that are still enormously useful, Merton has irrigated the field much more than a citation index could tell. Although he is almost absent from contemporary debates in France, his concepts have permeated the practices and the implicit epistemologies of many of us. We are Mertonian without even knowing it and this is rather a kind of Mertonian statement than a Molieresque one. In fact, Merton had forecasted his own fate in the history of sociology. His notion of "Obliteration by Incorporation" (OBI) clearly corresponds to the current status of his workable tools: we all use his operative notions and build up things like "middle range theories", but we do not name them like that. Of course, the study of the OBI of Mertonian concepts in France deserves real study, and it is another story, but it would be certainly worth doing, since the ignorance of one's own history is the best way of misunderstanding where we really stand now in the social sciences. Merton's "pragmatic orientation" as Calhoun and von Antwerpen put it, does not help much to reassert Merton's legacy. But European neo-pragmatist sociologies should think about it.

Merton in South Asia: The Question of Religion and the Modernity of Science

Dhruv Raina[1]

1. Introduction

My own engagement with the work of the sociologist of science Robert K. Merton arose in relation to the set of nested questions that could be referred to as the Weber question, the Merton question or the Needham question. More recently I delivered a lecture in Istanbul on what the priority dispute meant at the periphery of science that began with a detailed discussion on Merton's landmark paper on the subject (Merton 1957a, Raina 2008). The present paper briefly essays a genealogy of the social studies of science in India to understand the limited presence of Merton's influence till the 1970s. En route, it also raises some issues that have to do with the late arrival of Merton's work as sociologist in India that are related to the form of institutionalisation of the discipline in another context. Finally, I take up a discussion of some Mertonian themes that have resonated in the Indian research context, and then proceed towards explicating that context. However, as the other contributors to this volume deal with Merton's theoretical evolution in greater detail, it should be possible to ride piggyback on those discussions.

Several genealogies of the social studies of science in the West could be uncovered but all these genealogies encapsulate three socio-cognitive movements within which they were embedded. These have been referred to as the: [1] academic, [2] technocratic and [3] critical movements (Elzinga and Jamison 1981). In South Asia, while the first attempts to conceptually integrate science within society go back to the early decades of the twentieth century, the technocratic and critical traditions influenced by the "low church" of Bernalism blossomed throughout the 1950s and 60s (Raina 2003, Ch. 2 and 6; Vishvanathan 1997). The academic tradition within the social studies of science in

[1] The first draft of this paper was written while the author was a Visiting Fellow at the Max Planck Institut für Wissenschaftsgeschichte, Berlin.

South Asia remained a marginal one till the 1970s. The visibility of the technocratic and critical dimensions was an outcome of the mobilisation of science in the task of nation building and decolonisation that had begun to be planned since the mid-1930s (Abrol 1995). Both these projects were deeply connected with criss-crossed questions, themata and ideas flowing several ways.

2. The Reception of Merton and Context of his Reception/ Non-Reception in India

Since our central question here is the life and reception of Mertonian ideas in India, there are two axes along which this cognitive movement should be explored: namely the half life of Mertonian ideas in the sociology of science and the locations of Mertonian theory within sociology proper. By and large, the field of disciplinary history in India is still to be developed, though of late some important works on the social sciences and sociology have emerged (Assayag and Bénéï 2005; Uberoi, Sundar, and Deshpande 2007, 2). As far as the disciplinary history of sociology and social anthropology are concerned contemporary studies in the history of the discipline were preceded by two rather well known reviews (Mukherjee 1977; Vidyarthi 1978). In Western universities sociology and social anthropology are institutionally separated, and differentiated in terms of theory and methodology while in India the evolution of the two disciplines is deeply entangled. As pointed out by India's noted sociologist André Béteille: "This way of making a distinction [between sociology and anthropology] can lead to confusion. For if applied consistently, what anthropology is to an American will be sociology to an Indian, and what sociology is to an American will be anthropology to an Indian. The distinction will work only so long as all societies, Western and non-Western are studied only by Western scholars. It becomes meaningless when scholars from all over the world begin to study their own as well as other societies" (Béteille, quoted in Uberoi 2007, 7). The two disciplines then are inextricably intertwined in India. The most recent disciplinary history of sociology and social anthropology in India entitled *Anthropology in the East* explores through a number of essays the writings of the founding fathers of the discipline in India in order to explore the connections between knowledge, institutions and disciplinary practices (Uberoi 2007, 3). A cursory examination of the index of the book reveals two references to Merton. One mentions his being on the panel of referees for G. S. Ghurye's doctoral dissertation (Upadhya 2007, 238), and the other points out that the anthropologist S. C. Dube, in a reconstituted department of sociology and anthropology, proposed "[…] new broad-based courses on communication, modernisation, development and sociological theory," that discussed the work of Merton, Wright Mills, Parsons and others (Dube 2007, 478).

This however does not suggest that the influence of Merton was lacking in the Indian academic world, but, rather, that the concerns of Indian sociology from the 1950s onwards were quite distinct from that of Merton. While suggesting above that sociology and social anthropology are intertwined disciplines in India today and have been that way for sometime now, the beginnings of the two disciplines are quite distinct. As an academic discipline sociology was established during the period of late colonial rule, and grew with the expansion of the university system and research institutes. On the other hand anthropology was "an adjunct of the colonial state" (Upadhya 2007, 194). Sociology retained a degree of autonomy from political authority in terms of its concerns since it was drawn towards the nationalist struggle, but was subsequently shaped by the immediate postcolonial agenda of economic development and national integration (Ibid., 198). This does not suggest that the "structure-functionalism" debate was never part of the Indian sociological-anthropological discourse.

Mertonian sociology, it has been argued, acquired visibility as a paradigm just as the social studies of science began to emerge as a wider cognitive movement. This sociology of science amongst other things focused upon the institutionalization of modern science in the West, which meant interrogating the formation of modern science as a social system. Inherent to the normative framework that developed, these studies provided a frame and perspective from which to pose questions for science policy; and this inherent possibility within the theory added to its attraction as it was linked with altering the future of both science and society. Merton's work in the sociology of science extended over several lines of research, commencing with research relating to the famous "Merton thesis" that addressed the relationship between ascetic Protestantism and the fostering of science in 17th century England; another engaged with the ethos of science; a third had multiple discoveries and priority disputes as its focus, and there were several others (Hargens 2004).

Merton's sociology of science did not find an echo in the concerns of Indian sociologists until around the 1970s. Nevertheless, there were two fundamental sets of concerns around which these interests manifested themselves within South Asian science studies. This had to do with the fact that in India the social studies of science had its origins elsewhere, inspired through collegial ties of Indian scientists with members of the Cambridge Left such as Bernal and Needham, and Haldane, the last of whom finally settled down in India (Raina 2003; Dronamraju 2009). These ties dated back to the period of colonial rule —namely the 1920s and 1930s. Interestingly enough, as I. B. Cohen has pointed out, both Bernal's *The Social Function of Science* (Bernal 1939) and Merton's *Science, Technology and Society in Seventeenth Century England* (Merton 1938) were more or less of the same vintage. But it was the English tradition that struck roots earlier in India because of the peculiar networks of scientists that crystallized within India's tryst with colonial modernity. In fact, in 1989 a workshop was organized in New Delhi to commemorate 50 years of Bernal's *The Social Function of Science* and the papers presented were published in a special issue of the journal *Social Scientist* (*Social Scientist* 17 190–91 [1989]). I do not know any department of sociology in India that did the same with Merton around the same time. As discussed elsewhere, the Bernalian tradition went on to constitute the low church of the social studies of science in India, and played a significant impact on policy making almost till the early 1970s (Raina 2003).

The first Indian edition of Merton's 1957 classic on *Social Theory and Social Structure* was published in 1972, which means that it became available to students, as the international editions were then mainly accessible for university and college libraries. Apparently, the department of sociology in the Delhi School of Economics had introduced Merton's work on social theory and social structure and Mertonian sociology of science was taught in the early 1970s and the other universities in the country must have followed suit. Conversations with teachers and students from that era reveal that Merton's work on latent and manifest functions resonated within the prevailing functionalist perspective of the times.[2] But while Merton's sociology was taught, we do not see in sociology the work of a significant number of practitioners who were inspired by and subsequently elaborated upon it.

[2] Conversations with André Béteille, Surinder Jodhka and Shiv Visvanathan.

However, it was Merton's work as a sociologist of science that found a home in the social studies of science that began to develop in the 1970s in India, outside the umbrella of Bernalist science of science and science policy.

It could be said that Mertonian sociology of science was squeezed between the Bernalian science of science that dominated the 1950s and 1960s not just in the social studies of science, but the whole of science policy and post-Kuhnian philosophy of science and the new sociology of science whose ascent in India can be traced to the early 1980s. The 1970s could be seen as the beginning of the decade of the disenchantment with science; and it was here that post-Kuhnian philosophy of science, the new sociology of scientific knowledge, and social anthropology rushed in to fill the vacuum left by the decline of Bernalian tradition (Raina 2003, Ch. 2; Visvanathan 1997). Thus by the mid 1980s Ashis Nandy, Shiv Visvanathan, J. P. S. Uberoi and several others mounted a civilizational critique of science and the political hegemony of the West (Nandy 1998; Uberoi 2002; Visvanathan 1997; Shiva 1988). The argument has been construed variously as anti-science (Nanda 1991) but more nuanced readings have identified in it processes for the cultural appropriation of science and technology (Elzinga and Jamison 1986). In fact this civilizational critique did not attempt to reject science, but to raise the possibility that there existed alternate visions and cultures of science. At the time the scientific community or science-of-science networks failed to provide an appropriate response to this critique of science. This did not in any way derail the activity or the projects of science but within social movements of science it catalyzed the process for the democratization of public interest science (Bandyopadhya 1980). At this juncture Mertonian sociology of science had little to offer the neo-Gandhian networks of sociology and politics of scientific knowledge in India or elsewhere. These developments were framed in an environment wherein a new generation of studies on science and society would emerge. But it was during this period when new images of science began to be constructed. I, and many others who were commencing our careers, came across, at this juncture, Yehuda Elkana's paper on the anthropology of knowledge (Elkana 1981).

However, it was also around this time that a larger institutional response to a crisis in dysfunction within the world of science was manifest within departments of humanities and social sciences located at the Indian Institutes of Technology. This began to provide openings for the Mertonian sociology of science. Vinod Jerath, a product of the sociology department of the Delhi School of Economics, joined the Indian Institute of Technology at Kanpur and introduced a course on the sociology of science wherein Merton was taught.[3] His own research interests focused upon the formation of scientific community in India and he set the pace for what has been a preoccupation of sociologists of science since then. Some of his students and colleagues took up the task of teaching sociology of science at the Institutes of Technology and the universities that they subsequently migrated too. A relatively new university at the time, the Jawaharlal Nehru University established a Centre for Science Policy Studies. At these academic locations Mertonian sociology of science became an integral part of the curriculum. Very recently, as part of a study my students and I have undertaken on the disciplinary history of the science studies in India, one sociologist insisted, that Merton's sociology of science was introduced in order to stage the SSK critique of Mertonian sociology of science.

However, there were certain fundamental concerns that dominated Indian studies of science and society even before the advent of Mertonian sociology in the Indian disciplinary context. These concerns seemed to have dominated the Indian landscape from the 1950s to the 1970s. As indicated at the beginning of this essay, there is an important intersection of questions raised by the scholarship of Weber, Merton, and Needham. But we shall now discuss two other important concerns. The first of these concerns was derived from Merton's central contribution to understanding science as a social system, and as an autonomous institution. The normative features of the Mertonian paradigm explained its influence. One of these features was its ostensible sensibility to the possible dysfunctions of science. This perspective lent itself to the forays of policy makers prescribing the rectification of malfunctioning national systems. The system displayed several other attractions, and perhaps the most crucial of these was its central dogma that science's autonomy makes it independent of social influence, and that society has to have a certain form in order for it to fruitfully nourish the immanent development of science. For policy makers Merton's sociology bore the promise of ensuring the possibility of the

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[3] Conversations with Prajit K. Basu and E. Haribabu, Central University, Hyderabad.

immanent development of science and society. This modality of prescriptive reasoning about science betrayed an all-pervasive commitment to the hegemonic idea of science.

A second aspect to Merton's work in a different context resonated in India. In 1935 Merton had published a paper with Pitirim Sorokin on Arabian Intellectual Development between 700–1300A.D. The paper inaugurated a new quantitative trend in the history and sociology of science that involved estimating the extent of scientific activity by measuring the number of men of science from across nations and periods. In the words of Merton and Sorokin: "If such a procedure is regarded as scientific, there can scarcely be any objection to, or necessarily any subjectivism in the systematic utilization of these estimates as a basis for the organization of quantitative indices which would recapitulate the course of movement of a given culture or a given social process. The numerical indices obtained may be used to chart the intellectual development of a culture with attendant heuristic advantages…" (Merton and Sorokin 1935, 516). This trend was further developed and amplified in Price's *Little Science, Big Science* (Price 1963), a work that was later to inspire the gargantuan scientometric project of Garfield and others (Garfield 1982). In the late 1970s scientometrics comprised one of the largest sub-disciplinary networks involved in the study of the evolution of the sciences in India. In a manner of speaking this tradition was quite divorced from the other concerns of the sociology of science as it subsequently evolved in India—it became a kind of arithmomorphic game that science bureaucracies reveled in. Amongst a variety of other quantitative instruments that it offered policy makers it most importantly, in the form of the citation index and the impact factor, provided research councils and funding agencies with a metric for evaluating the scientific research of a variety of communities, no matter how problematic these indices were. In other words, scientometrics combined well with the technocratic imagination embedded within the institutions of science policy (Raina 2003, Ch. 2).

3. The Merton Question and its South Asian Response

I now would like to discuss the work on science and religion in India using Merton's landmark paper on science, technology and society in seventeenth century England, to launch my discussion on the reign of both modernization theory and Marxism in the Indian context. Merton's paper argues that ascetic Protestantism offered social attitudes or values that were particularly conducive to the advance of the sciences. This Protestantism-science thesis was presented by Merton in the context of one form of ascetic Protestantism (Becker 1984, 1065). The task itself, it has been argued, was inspired by the Weberian proposition that the influence of ascetic Protestantism could possibly have extended beyond its contributions to the rise of the spirit of capitalism (Weber 2002). Merton's paper on "Puritanism, Pietism and Science" (1936) went on to link German Pietism and English Puritanism, which provided a profound and sustained impetus to the development of science (Becker 1984, 1065). Further, it could be argued that the legitimacy of modern science was "an unintended consequence of values and practices of ascetic Protestantism." These values incorporated mathematically rationalized empiricism. Consequently, ascetic Protestantism motivated and canalized the activities of men in the direction of experimental science (Hess, 1997). In other words, the development of a social institution such as science had to be supported by group values.

The Merton thesis was proposed as a response to two questions. The first asked why science emerged so tenaciously in the third quarter of the seventeenth century, the second, why a particular institution such as science flourishes at a given historical juncture (Cole 2004, 837). The question posed for social theory then becomes: "if we know what caused a particular society to have a scientific bloom then, assuming that this cause is generalizable, we might be able to apply that cause to other societies and do things that will purposely help the development of science" (Cole 837). This form of counterfactual sociology had a certain appeal for policy particularly in the developing countries where newly emergent nations were in different stages of institutionalizing their national science systems.

Before turning to the relationship between science and religion in South Asia, we must ask how this relationship is conceived in the West. Do we accept the standard picture that portrays science and religion as two contradictory, oppositional, conflicting pursuits? How has the relationship between the two changed in historical times? The changing relationship between science and society is in consonance with the interests they serve within society. Consequently, it is not meaningful or rewarding to conceptualize science and religion as historically unchanging. Taking a historical perspective Hedley Brooke had proposed that there are three historically contingent models of the relationship between scientific and religious movements serving different social, political and religious ends (Brooke 1996, 764).

The three models or broad historical patterns that Brooke has identified are those of isolation, integration and conflict. The integration model is encountered in societies where an emergent scientific community is seeking reconciliation with powerful religious interests, or where religious interests attempt to redefine themselves in the light of scientific reason or protect themselves from perceived external threats. The protective strategy is also evident in the isolation model, inasmuch as it postulates a separation of the two realms of science and religion. Further this is a marker of the separation of professional science from that of academic theology. The conflict model appears in sharp focus whenever the scientific community seeks to consolidate new scientific developments or professional changes and in the process excludes the religious community, or when the religious community feels frightfully threatened by these developments (Ibid., 764). These broad patterns or models themselves emerged out of the Western European historical experience over the last three hundred years. When extended to non-European contexts and experiences we reckon with the likely interfoliation of these models. It could even be suggested that this may be equally true of the European or Western experience as well. Histories of science in non-Western cultural areas frequently ascribe the non-emergence of modern science in these regions to be a consequence of the over-bearing presence of religious interests that have stifled the scientific march towards truth (Chattopadahyaya 1959, 1979 and 1986). On the other hand, the processes of modernization in India have been studied from a variety of perspectives but the first three decades of the era of decolonization were committed to some version of modernization theory. In fact it could be argued that the colonial writing on science and history in India shared a common ground with the modernist writing of the first two decades of decolonization.

I shall not dwell on the nature of science and religion in pre-colonial India, even though the standard historiographies of science have suggested that science in the pre-modern world was ensconced within the sacred realm. Once we enter the modern era the central question that has been posed is a variant of the Needham question: why did India that had such an elaborate tradition of the sciences not witness something like the scientific revolution of the West? The question addresses the phenomenon of non-emergence. This in turn involves foraying into the status of counterfactuals and counterfactual history. By and large most scholars who have responded to this question—which in a manner of speaking they were doing even before the Needham question acquired canonical status—have done so naturally within a frame of an overdeterminationalist theory of history; for modern science is held to be paradigmatic of all science (Fuller 1998; Raina 2003). Furthermore, the framework also operates within what a leading historian has called a theory of absences. Thapar once pointed out that Weber writing on India gestured towards the absences in Indian society that purported to explain the non-emergence of capitalism (Chakrabarty 2007; Thapar 1993, 30). The presumption of path invariance underscores this scholarship.

Turning back to the early decades of colonial India, it has been argued that the officials of the East India Company, opposing the suggestion of British orientalists who believed that modern science could be grafted onto a Sanskritic base, decided to pursue a policy of "westernization" and reframe the traditional schooling system (Baber 1996; Visvanathan 1997). This meant the gradual withdrawal of patronage to the existing schooling system, and the promotion of English as the medium of instruction to the detriment of the classical languages of instruction namely Persian, Sanskrit, and a multiplicity of vernacular languages. This also meant new school and college texts, the production of which entailed a complex set of activities in cultural translation. It is in this intermediate space that the textbooks themselves became instruments for Christian religious propaganda (Raina and Habib 2004, Ch. 4 and 7; Venkateswaran 2002).

While the motives for Western education were many, for the utilitarians it was the civilizing mission and for the missionaries the extension of the Christian domain, yet both believed that an introduction to the sciences would destabilize the foundations of Hindu religious beliefs and result in either a process of secular modernization—as the utilitarians desired—or extend the dominion of the Kingdom of the Lord—as the missionaries would have wanted. As historical research and sociologically-oriented history have subsequently indicated, both camps were sadly disappointed. By the last decades of the nineteenth century it was discovered that the Indian pupils in modern schools were enthusiastically taking to the sciences and demanding more of it, without in any way abandoning their core religious practices and beliefs (Gosling 1976). Interestingly enough, by the 1870s through the efforts of British positivists such as Congreve, positivism had anchored itself in the city of Kolkata. Soon enough a Bengali Positivist Society had been established (Forbes 1975). The population of cultural amphibians called the *bhadralok*, from whose ranks India's first scientists would graduate, were drawn more towards the idea of a positivist church than towards positivism as an expansive methodological program.

Missionary and colonial education drew legitimacy from two ideas that they sought to instill amongst colonial subjects and pupils. The first was the notion that science was foreign to the Indian ethos and way of life. Secondly, that the superiority of the West over the East resided in its scientific superiority. This has been discussed in great detail by Michael Adas in several colonial contexts (Adas 1990). There was something paradoxical about this project. The idea that India had an ancient and active scientific tradition was implanted within the European imagination through the efforts of French and Italian Jesuit scholars stationed in India and later by French and English Orientalists (Filliozat 1951). These explorations endowed the modern educated Indian class with a new sense of history and historical destiny, projecting them into a novel relationship with their own past (Kaviraj 1993; Chatterjee 1993; Stokes 1959). Armed with this new sense of history, in part inspired by the writings of the British Orientalists, the first generation of Indian scientists would proceed to neutralize the cultural connotation of modern science as a purely Western invention. In another move by the end of the nineteenth century they quickly domesticated the discourse of science as a cultural universal (Raina and Habib 2004, Ch 4).

Similarly, in a study a colleague and I undertook on the reception of Darwinian evolutionary theory in India towards the end of the nineteenth and twentieth centuries, we were surprised to observe how quickly Darwinian evolution was culturally redefined into the Hindu cosmology and in fact was seen as an instantiation of that very cosmology. But, and this is important, there was belligerent resistance to theories of social evolution that were part and parcel of late colonialism's ideological panoply. In the realm of the social, these *bhadralok* intellectuals were deeply critical of narratives of progress and the decadence of Indian civilization (Ibid., Ch 7). The roots of Gandhi's oft quoted, uttered or unuttered, remark about Western civilization, lie here.

Within this new discourse however, there was a salient self-critique of Hindu religion and practices, and the writings of Pramath Nath Bose, Benoy Kumar Sarkar and the chemist P. C. Ray elaborated an extensive and trenchant critique of the caste system. This critique dating back to the end of the nineteenth century could possibly pass for an early version of a proto-Zilsel hypothesis; that through the divorce of the head and the hand, the theoretical learning of the Brahmins and the richness of artisanal crafts and skills, India had the lost opportunity to modernize its sciences (Ibid., Ch. 5 and 6; Raina 2003). This re-visioning of the past acquired momentum with the spreading tide of nationalism; and with the end of colonialism, in the new era of post-colonialism, crucial challenges were posed for social theory as a whole, and more specifically for disciplines like economics, history and sociology. The 1950s were thus marked by an attempt to re-imagine the past by radically departing from the so-called colonial construction of Indian history and society. As far as the history of sciences was concerned one important break was to depart from the characterization of India as a spiritual civilization, and initiate its study as any other civilization (Raina 2003, Ch 2). These recuperations, while exceedingly insightful from the perspective of social theory, had their problematic aspects, particularly those constructions that bordered on some kind of indigenism.

The project of decolonization and development in the new democracy was entangled with that of the mandate of the Nehruvian state. In 1958, the Scientific Temper Resolution was inscribed in the Indian Constitution, and one of the goals was to build temples of science in sovereign modern India. Within this frame the slow progress of so called modernization and the path to modernity was explained in terms of the resistance posed by pre-modern practices and ways of knowing that in turn impeded the realization of an authentic modernity (Rahman 1972 and 1977). In other words the inability of scientism to make inroads into the value system of Indian society was the cause of the non-emergence of an authentic modernity (Gupta 2000). And all this while the scientific research system was well institutionalized and grew constantly and India became a model for other third world countries in its commitment to science.

On the other hand, if we were to adopt Merton's approach and proceed to examine the biographies of Indian scientists in the *Dictionary* *of Scientific Biography* (Gillispie 1970–1980), or in the memoirs of the different science academies, we would find very little about their personal lives and beliefs. However, we might be surprised to find how many of those who entered the career of modern science in India maintained their religious beliefs at least during the first half century. But a few of the decorated scientists, more articulate and less self-conscious of their scientific and religious commitments, have clarified that their work in science deepened their appreciation of the sense of the spiritual. I have elsewhere discussed the work of the Pakistani Noble laureate physicist Abdus Salam (Raina 2005) and the twice denied particle physicist E. C. G. Sudarshan on the subject (Raina 2008). Both, believing physicists, have reflected upon the question whether their religious background influenced their science. Salam denied this in an interview with Lewis Wolpert, though he did not rule out the possibility that his religious background might have stimulated ideas of unification in physics. Sudarshan claimed that while his religious commitments did not directly shape his physics, they certainly had a role in ethically guiding his research career.

The response of Salam to the problematic of science and religion is quite typically characteristic of a number of scientists from the Indian sub-continent, who strived to promote the cause of science in the region as well as their respective countries. What Salam did share with another believing Western scientist, the Christian Marxist Joseph Needham, was an "ecumenical picture of science" (Needham 1973). Thus Salam was to write in 1984: "My own view has always been that science is the shared creation and joint heritage of all mankind and that as long as a society encourages it, Science will continue to flourish in that society" (Salam 1989, 280). This idea that science was the shared creation of all mankind did not merely parry the thrust of Western exceptionalism but legitimated science as a culturally anchored activity and not as something gifted by the West to the non-Western peoples. His biographer Jagjit Singh informs us that for Salam "[…] science and religion refer to different worlds; religion to the inner world of the human mind and science to the outer world of matter. To explore his inner world of 'soul' and Allah one needs faith and to explore the outer world of matter, reason" (Singh 1992, 157).

Where did the ethical problem of science arise from? According to Salam, the problems of the developed world could be understood to be a consequence of too much science. The problems of the less developed world and the developing world arose from too little of it. In order to bridge the gap between the two worlds, a larger portion of the developing world's citizenry would have to be drawn towards the world of science. This would have to be done without creating the sort of cultural dissonance that the colonizers had inaugurated, and without alerting the population to a conflict between science and religion.

In a piece of work pursued a little over a decade ago, a colleague and I had suggested that Indian scientists and political figures of the end of the nineteenth century effectively argued that while the external world was governed by the laws of science, the inner world, the divine one was known and comprehended by the Indian religious and philosophical traditions and that there was no clash between the two worlds of science and their religious beliefs (Raina and Habib 2004). While this might be seen as an instantiation of the isolation model, as scientists attempted to legitimate the pursuit of modern science portrayed as a Western cultural enterprise by the colonizers, the attempt of the newly educated intelligentsia was to neutralize the cultural import of this hegemonic ploy. And in doing so they would push the trope of isolation in a direction where the outer world was enveloped within an inner world. Thus while the Kantian dualism of the "heavenly skies above and the moral law within" held its own, for our late nineteenth century Indian interlocutors this dualism was subsumed by a greater unity of the divine. While science was morally worthwhile and economically beneficial, modern science revealed to us the laws that governed the external world. Another science that the Eastern religions had grasped revealed to us the inner world of man. This dual separation of the two realms enabled them to protect science from religion and to shield their own culture from the cultural imperialism of the colonizers.

4. Intellectual Legacies and Institutional Contexts

In conclusion, following the introduction of Western science, the Indian equivalent of the Merton question and its variant the Needham question would not deal with the emergence of modern science but with the introduction and reception of modern science. And from the framework of modernization theory, the question would be: how did religious beliefs and predispositions arrest or impede the spread of modern science? Postcolonial theory and the history of science have since signaled that political factors relating to the expediencies of colonial rule and control arrested the expansion of modern sciences in nineteenth century India (Baber 1996; Bayly 1997; Kumar 1995; Raina and Habib 2004; Sen 1991). In fact, the central question posed by historians of science, which I have labeled the Sen question, asks: why was the expansion of modern science so tardy in nineteenth century India, when this was the century of most rapid expansion everywhere else (Raina 2003, Ch 5)? I do not think historians of science working on India have seriously tried to explore a response in terms of the conflict model between religion and science. In that sense, Merton might not have been surprised by their research findings. But as to the possibility that religion might actually have created the conditions for a scientific bloom, the matter, as far as South Asia is concerned, remains undecided. But even to get a glimpse of the issues involved we would have to look for clues in the works of another leading American social anthropologist, Bernard Cohn, and his path-breaking work on the anthropology of civilization, colonialism, and epistemology (Cohn 1971 and 1997). It is Cohn and the new post-colonial theorists of science who figure in the writings and analysis of sociologists of scientific knowledge in Indian today.

If one were to venture a broad conclusion it could be said that the late nineteenth century Indian intelligentsia was predisposed to carving out separate domains for science and religion. This fragmentation had its origins in the first stages of decolonization that had begun in the last years of the previous century. The first step in the process of neutralizing the cultural imperialism of the colonizers was to establish that science was part of the patrimony of all civilizations, that it was a cultural universal. This legitimated science as a cultural activity. In the world of science splintered by the dichotomy of fact and value, the isolationist strategy helped in protecting religion and culture from the inroads of scientific cultural imperialism.

Finally, the reception of Mertonian sociology of science in India from the 1970s onwards was closely linked to the preoccupation with sub-optimal efficiency of the science system, reflected further in structural reforms introduced during the period. This preoccupation was manifest in the proliferation of research interest into the existence, structure and evolution of the scientific community. Under the umbrella of these sociological concerns, scientometrics too evolved along directions wherein it became separated from the concerns of sociology and became a quantitative instrument in its own right. On the contrary, it became an instrument in the hands of evaluators and policy makers, using metrics to measure the health of science in India, despite the substantial criticism of the use of scientometric indicators.

The Contribution of Robert K. Merton's Key Concepts to the Analysis of Gender Differentiation in Society

Cynthia Fuchs Epstein

Sex and gender are among the subjects and fields of study most resistant to impartial inquiry in the social and physical sciences. Because no individual is free from impressions or perceptions based on personal experience or philosophy, applying the rules and standards of science to these subjects is not only difficult but is often resisted by those who come to scientific study carrying paradigmatic, social and personal biases. Further, regarded as less important than other domains of analysis, the work done on these subjects is often glossed over or not subjected to the kind of rigorous analysis applied to other subjects.

The concepts and methodology advanced by Robert Merton have been of great utility to my work and that of others in analyzing and contesting some of the accepted and privileged work in these fields. Actually, only a few sociologists (with the exception of some of his former students and academic colleagues) who have explored sex and gender issues have specified the use of Merton's ideas in their analysis. They include, of course, Harriet Zuckerman, Mirra Komarovsky, Alice Rossi, Rose Laub Coser, and Lewis Coser.

Many others have quietly benefitted from Merton's ideas that have been absorbed into popular thought—a process labeled by Merton as *obliteration by incorporation*. Merton's influence has been incorporated though certainly not obliterated in my own work over the past 40 years. His ideas have been a keystone to my own throughout my career, most recently in the analysis prepared for my presidential address to the American Sociological Association (Epstein 2007) identifying "Great Divides: The Cultural, Cognitive and Social Basis of the Global Subordination of Women."[1] This paper will address some of the ways

[1] See also Epstein, 1970a, 1970b, 1985, 1991, 2004. I also include references to Merton's concepts from my own class notes from his course "Social Theory and Social Structure" given during the years 1961–6 at Columbia University. His concepts are specified with the use of italics throughout this paper.

in which this has occurred, how valuable his work has been to my own analysis, and how it might further inform scholars in the future.

First and foremost I want to note that Merton's basic contributions were simple, fundamental, yet revolutionary. His injunction: "Ask the question: Is it really so?" poked holes in some very commonly held assumptions about the workings of society and the assumed functions of various institutions. Merton (1984b) warned that "What everyone knows to be true often turns out to be not true at all," noting the perils of popular acceptance of the "latest word"—often only a new version of an old idea. Merton encouraged a critical and self-critical outlook in everyone who attended his extraordinary classes on "Social Theory and Social Structure" at Columbia University over the years and instilled in his students the taste for "organized skepticism."

Unlike many grand theorists who predicted specific changes in society or who advanced predictive models such as Karl Marx, Vilfredo Pareto or his own teacher, Pitirim Sorokin, Merton's directive was to discover and understand possibilities rather than to make predictions. He urged sociologists to analyze why institutions and groups, and therefore the individuals who are members of them, tend to behave in patterned ways. And further, how people may perceive the behavior of members of groups according to their stereotypes of them, and turn a blind eye to their actual behavior. Merton brought the concept of "selective perception" from the individual psychological level to the collective social one. His call to *establish the phenomenon* was a reminder to sociologists not to accept common assumptions.

Nowhere are these questions more pertinent than in the analysis of the ubiquitous social divisions based on sex—a phenomenon occurring in every society across the world and across all eras of history. The dichotomous division of male and female[2] has led many social analysts to insist that the cause for the division must be grounded in evolutionary, biological, or basic psychological universals. To this day, even tiny statistical differences found between populations of girls and boys on such measures as mathematical or verbal ability (a difference that has virtually disappeared, according to a recent report by the

[2] I first described this process in detail in my Presidential address to the Eastern Sociological Society in 1982 (Epstein 1984); and further developed it in my Presidential Address to the American Sociological Association in 2006 (Epstein 2007).

National Academy of Sciences [Dean 2009]), reinforce the notion that there is scientific evidence of basic differences that result in the universal highly stratified division of labor, and differentiation in authority, interest, demeanor and emotion. However, nowhere in the world are the demonstrations of such behaviors left to chance. Everywhere in the world, the first glance at the genitals of the baby emerging from the womb defines many of the new individual's social roles and opportunities for life. As the Nobel Prize winning economist Amartya Sen (1990) has noted, for those marked as female, in utero or at birth, in China and India, for example, the designation may be a death sentence by abortion or infanticide. And elsewhere it might mean a life of slavery, a life without access to education, full citizenship or the right to go where they wish. Of course, in societies where females are treated relatively well, designations of sex track individuals into the niches of the informal and formal labor markets defining women's work and men's work (Charles and Grusky 2005).

I am using the word *sex*, and not *gender*, which has become conflated with sex in common parlance, because I am referring to the basic physical characteristics that differentiate males and females, not to the social designations that the word "gender" should characterize.

Because Merton was always interested in the life histories of concepts and their seepage into communities of thought, I thought I would note the events and contexts of life that made me especially receptive to his ideas and their use in my analysis of the sociology of sex and gender. Growing up in North America in the aftermath of World War II and observing my parents' activism in organizations aimed at social change, I hoped to play a role in creating a better world. I attended the exceedingly liberal Antioch College and was particularly influenced by my Political Science professor, Heinz Eulau, who assigned Merton's writings, as part of his campaign to change the study of government into a social science of political behavior. (This was in the late 1950s, and Eulau left Antioch for Stanford University shortly afterward.)

But after Antioch I did not quite know what path to take or indeed if there was one open to me. Recall Mirra Komarovsky's striking analysis in her article "Cultural Contradictions and Sex Roles" (1946). Unlike other middle-class women of my generation, and certainly my mother's generation, I did not think it was enough to marry, raise a family and volunteer. Like other women college graduates, I found it difficult to find employment promising a real future. Women typically were excluded from the news staffs of newspapers and magazines and found it difficult to be admitted to law or medical school or to practice either profession. They had limited access to fellowships in universities and thus found it difficult to engage in scientific research or run organizations that made policy. And of course, they were hardly to be found in the Congress of the United States. I found work after college at a women's charitable organization. There I was struck by some logical inconsistencies. Its executives were women who worked full time for no pay. They were highly competitive and eager for recognition.

It was this observation: women as workers; women executives who worked full time but who defined themselves primarily as housewives and mothers but who were aggressive and ambitious—qualities regarded as male—that was a paradox. If, as was commonly believed, women were not naturally suited for competitive tracking; if women were not oriented to leading, and uninterested in attaining executive positions, how did one account for the achievement-hungry women I knew?

It was only after I became fed up with my job that had no potential and I was encouraged to go to Columbia for a PhD[3] that I found a way to think about the paradoxes I had witnessed and others I was to find in later observations. The theoretical orientation Merton offered and his conceptualizations of social processes gave me both a vocabulary and a "tool-kit"—the term suggested by Ann Swidler (1986) for cultural and social analysis. I realized the apparent contradictions could be explained using Merton's notions of *status-sets* and *status sequences*; of *anticipatory socialization*, of *reference group* theory, of *sociological ambivalence*, of *opportunity structures* (1995) and his theories of *anomie* and *socially expected durations*; and his conceptualization of *dysfunction*—a term rarely used in the sociological literature these days in the way in which Merton conceived it.

My earliest research and writing investigated the reasons for women's virtual exclusion from the prestigious professions of law, medicine and engineering, the spheres referred to in the Professions Project led by Merton at Columbia's Bureau of Applied Social Research in the late 1950s. Although I came to Columbia after the project ended, I chose women in the professions as a dissertation topic and started to

[3] Thanks to Professor Henry Lennard whose courses I took at the New School built on his research at Columbia's Bureau of Applied Social Research.

compare the percentages of women in professional work across the world, data not easily obtainable at that time. I found that the pattern exhibited in the United States and Western Europe was very different from that of socialist countries. The analysis of careers in these different structural and ideological settings resulted in my first book, *Woman's Place,* published in 1970. I pointed out that in socialist societies medicine and law were considered woman's work—more than 70 percent of doctors in these countries were women—and in the United States and the West, the same fields were regarded as men's work. Therefore, by local custom and restrictions such as quotas on their entry, only a very few women chose to apply or were admitted to medical and law schools or to certain graduate departments of universities. In the West there were not only legal and informal barriers to training but rationales legitimating the exclusionary practices. Merton's conceptualizations of the normative nature of status-sets and their reflection in the statistical regularities that made them appear normal, created insights into women's differential ease or difficulty in choosing a career path that was considered abnormal. Merton suggested that *functionally irrelevant* criteria could be at play in the process of the accumulation of advantage that men from certain backgrounds benefited from, and that *functionally relevant* criteria might not be applied if a person's status-set did not conform to the preferred configuration.[4] I found it productive to note the coercive quality of institutionalized status sets, especially when they typically were composed of dominant statuses (such as woman or Black) that determined whether an individual could or could not choose (purportedly on the basis of interest or ability) additional statuses not commonly associated with the dominant status. At the time, Merton had only engaged in what he called "oral publication" of many of these ideas and did not publish them. I included this analysis, and referred to Merton's oral publication of the idea in *Woman's Place* (Epstein 1970a, 87).

I also saw how expectations about the composition of women's status sets, that is, the statistically predominant collections of statuses they could acquire in their lifetimes—excluded high-level professional

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[4] Zuckerman (1977) in a footnote in her chapter on "The Social Origins of Laureates " notes the process and refers to Merton's development of the idea in his classes from 1955–71, although at that point the only published reference was in my book *Woman's Place* (1970) in Chapter 3.

roles. Instead, for those who had to work outside the home, a condition not particularly favored, the work should be occasional, not demand much preparation, and be easily substitutable because women were expected to drop out of the labor market when they became wives and mothers. Indeed, many theorists who leaned on the human capital paradigm explained this pattern according to rational choice theory, justifying it as the product of women's choices, rather than the result of discrimination by men acting to protect the boundaries of their field and to limit access to it. (This kind of analysis brought a Nobel Prize to the economist Gary Becker, and the Economics Department at Columbia was oriented to this point of view.) Merton's caution to consider whether statistical regularities were, in fact, a sign of some positive functional mode in the society put me on high alert to look for other elements in the system that might explain the statistical pattern. Further, Merton suggested that views about women's interests and capacities—even if they were false—could cause women to be tracked along paths that resulted in statistical regularities that were accepted as normal. It was this process that investigated for my doctoral thesis (Epstein 1968), which focused on American women in the legal profession.

I narrowed the investigation to women lawyers because there seemed to be no point in documenting educated women's occupational choices that conformed to well known patterns. Working in the mid-1960s it was the case that most white middle-class women did not work after they married and had children, and for those who did, their career choices usually were in the spheres of teaching, social work, nursing, or the other "helping professions." So instead, I followed the model of deviant case analysis chosen by Merton's former students, Seymour Martin Lipset,[5] James Coleman and Martin Trow, when they studied the printers' union, a labor union distinctive because it was not characterized by "the iron law of oligarchy" and was instead governed by democratic process (Lipset et al. 1956). Thus, I did a "deviant case analysis," of women who chose to study and practice law, a gateway to other careers in business, and politics, all male dominated professions and activities.

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[5] Lipset (1988) wrote some years later that the initial impetus for the study was a paper he wrote for a course given by Merton while he was a graduate student at Columbia, and that he was joined by Coleman and Trow, who succeeded him as graduate students in the department.

The sample of lawyers was chosen at random from the Martindale Hubbell Law Directory. At the time, there were only 7,000 women lawyers in the United States, about three percent of the profession. Those I found in the New York area did show some patterns. They had invested in their human capital by finding law schools that would admit them, and did well scholastically. They tried to get jobs appropriate to their education and performance, but they found it difficult or impossible to get jobs as lawyers. Some were offered jobs as legal secretaries, as was former Supreme Court Justice Sandra Day O'Conner, although she had placed third in her class at Stanford Law School; and, of course, Justice Ruth Bader Ginsburg, who was first in her class at Columbia University, had similar difficulty in getting a job after graduation. What all of these women faced was, in Merton's terms, the *lurking presence* of a prejudice that placed them on other, less rewarding routes than those open to men who possessed the same credentials. I found that they could either find employment in firms in which they had family ties as a daughter or wife; or as volunteers working for organizations such as the New York Legal Aid Society, or, in a very few cases, they might find a progressive man who would take a risk in hiring or appointing them to a suitable legal position. Such was the climate in those days.

I also found in my interviews that women's status in the law could be better understood analytically by applying Merton's typology of *anomie* (Epstein 1974). Recall his four-fold table in which the common and predicted progression was that individuals who internalized the value of achievement in the United States used the means prescribed to accomplish it. But three other boxes in the table remained. Individuals might accept the values but have no means to accomplish them; or they might reject the values and the means.

What was curious about women lawyers was that those who accepted the values (that is to say, hard working and seeking professional status) and who found the means (that is, admission to a law school and doing well), faced *disapproval* rather than the rewards given to men who followed the same course. Indeed the women might even face punishment. How and why was this played out? Women lawyers reported in my interviews that even when they did well in law school they could not get jobs; those who did get jobs said they were often isolated or ridiculed in the performance of their roles; or they found their characters were assailed as being "ballsy, acerbic or unwomanly."

This was not only so in the United States, where a survey conducted by the *Harvard Law Record* (Abel 1963) showed that women lawyers were the group *least* favored by law firms of all sizes, even less than lawyers of color, or those who were at the bottom of their class, no matter what their work performance. It was also the case in Great Britain, where a survey of professional women showed that managers characterized them either as "nice mice" or "dragons" (Fogarty, Rapaport and Rapaport 1971). Even in the sciences, where presumably objective criteria are used to evaluate scientists, women often faced irrelevant evaluations. For example, the scientist Rosalind Franklin, credited with the key research that led to the discovery of the genetic double helix, was characterized as tough and cranky by James Watson (1968) the winner of the Nobel Prize based on her research, a view quite contradictory to that of her biographer Anne Sayre (1975). Both my study of lawyers and the British study of managers found that if women professionals asserted authority and conformed to the norms of assertiveness expected of lawyers in a courtroom, or managers in industry, they were considered interpersonally impaired, but if they conformed to norms that specified that a lady be soft-spoken and offer her opinion only obliquely, they were considered inadequate for the job. They were "damned if you do and damned if you don't," a concept articulated by Merton (1957) to illustrate how social expectations were linked to status sets in which the statuses were expected to be normatively congruent and created an environment which promoted or retarded an individual's ability to be accepted or rejected in a particular social setting. I saw, through my interviews, a no-win situation in which society's attitudes resulted in the *self-fulfilling prophecy*. Merton coined this term to describe the phenomenon of when "in-group virtues become out-group vices" (Merton 1957, 426). For example, assertiveness was a desired trait in a male litigator, but assertive women were devalued as potential partners in litigation practices, leading to the "evidence" that they were unsuited. Thus, women were rarely found in them, reinforcing the idea that they did not want to do this sort of work and were unsuited for it.

Merton, building on Harriet Zuckerman's (1977) findings in her study of Nobel Prize laureates, drew on a passage from the Bible to formulate "The Matthew Effect," illustrating the probability that "to he who has shall be given," to show how the more advantages individuals have the more they will acquire later, and he also noted the second part of the Matthew observation that "from those who have not, it shall be taken away."

Merton was interested in the interplay of role and status theory applied to women's position in society and asked me to write a chapter on "Sex Roles" for the fourth edition of the textbook *Contemporary Social Problems* (Merton and Nisbet 1976). As he wrote in the introduction, the chapter centered "on the social problems raised for both women and men by various forms of sex-typing of social roles and the cumulative disadvantages that often go with them" (Merton 1976, vi).

This work concentrated mainly on Merton's concept of status-set typing and indicated how women could be (and were) prevented from activating the professional statuses they had struggled to acquire by ignoring their preparation and intellectual gifts. Interactions between men and women holding statistically infrequent status-sets often focused on statuses inappropriate to the interaction. For example, the woman who managed to become a doctor might encounter rejection from a male patient who, though ill, focused only on her sex and not her professional competence. Or, alternatively, a male kindergarten school teacher might be feared by parents as a potential sexual pervert. A focus on male sex roles, begun by Mirra Komarovsky in the 1940s and revived in the 1970s never caught the attention of the sociology profession although a few male scholars have continued the work with distinction.[6] However, Merton's notion of "role models," another concept that has seeped into the common vernacular, also informed another part of the process of role acquisition. Because women doctors, lawyers, scientists and engineers were in such short supply, young women had no role models who they might try to imitate during the process of "anticipatory socialization," another of his important insights.

Merton and Zuckerman's idea of the *accumulation of advantage and disadvantage* was clearly depicted in my subsequent research on black women in the labor force. Human capital theory did not offer a credible explanation for their inability to rise in the occupational structure. For one thing, compared with white women, black women were more likely to work, and to work continuously throughout their lives. Even those who were mothers of young children worked—and yet these human capital investments did not pay off—at least not then. This was

[6] Such as the work of Joseph Pleck (1976), Michael Kimmel (1996), David Collinson, Jeff Hearn (1996) and R. W. Connell (1995).

because a lack of advantage and simple prejudice segregated them in employment ghettos composed of the least skilled and lowest paying occupations.[7] A study I conducted on black professional women (Epstein 1974) was a deviant case analysis of those women who had sidestepped an accumulation of disadvantage to become lawyers, doctors and business managers. Employing Merton's analysis of the dynamics of the status-set, I serendipitously observed that, in these few cases, having two negatively valued statuses sometimes gave an individual a positive return. That is, a foot in the door. For example, the few black women partners I found in large law firms reported that because they were black, white males did not regard them the same way they did white women and attributed to them a greater commitment to work. But because they *were* women, the men did not feel threatened by them or feel that they had to include them in their male-dominant inner circle. In the case of African-American women in medicine, there was the possibility of rising in a black institution (possibly because their community did not find working women to be unusual), and indeed, a number of the women physicians' appointments were made at New York's Harlem Hospital, a black institution. This hospital gave one woman the opportunity to do ground-breaking research in hematology.

The research that was intended to describe the reasons for restrictions on women's opportunities was to develop into a larger analysis of social change.[8] Merton's *anomie* paradigm pointed in a fruitful direction by describing the possibilities that arose when one accepted the values of society, but was cut off from means to success and was forced to devise alternative routes. *Innovation* was the name given to that alternative, and Merton gave the example of people engaging in social movement activity. Of course, another possibility was *retreat*, which is what most women had done in earlier generations with the

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[7] Yet in the early 1970s, polls showed (Harris 1972) that they were less ambivalent than white women about working and scored twenty points higher in favor of efforts to change women's status in society (67 percent of black women as opposed to 45 percent of white women polled). (Merton and Nisbet 1997, 441.) Further, black women constituted a larger percentage of the population of black professionals as contrasted with white women.

[8] For many years this paper was misinterpreted by a number of minority women sociologists who claimed I posed this as an observation about black women professionals generally, rather than one illustrative of a subset of "deviant cases."

result that they suffered from "the problem that had no name." It was a condition labeled and defined by the journalist Betty Friedan in her book *The Feminine Mystique* (1963), inspiring a generation of women to found and build a social movement to improve their chances in society. Because of the impact of Friedan's book, an African-American civil rights lawyer, Pauli Murray, and a government lawyer, Sonia Pressman Fuentes, urged her to create a formal social movement on the model of the NAACP (The National Association of Colored People) to show there was political will for implementing civil rights legislation. Here Merton's functional paradigm should be invoked to alert the scholar to the fact that a change in one part of the social system invariably creates change in other parts of the system. Presidents, starting with John Kennedy, Lyndon Johnson and Richard Nixon, moved first to investigate the status of women through various commissions and then to construct legislation to eliminate discrimination in employment. Under Johnson's administration, Congress passed Title VII of the Civil Rights Act of 1964, forbidding discrimination in employment on the basis of sex in addition to race, ethnicity and nationality. Subsequent legislation provided for implementation. Friedan, along with a number of other activists including Alice Rossi, a former student of Robert Merton's and his co-author of the chapter on reference group theory in *Social Theory and Social Structure* (1957), created the National Organization for Women in 1966, and almost immediately a cascade of new organizations erupted in the professions and the academy promoting women's access to training and entry into strategically important job tracks in spheres formerly believed inappropriate for women.[9]

The outcome of this social movement activity, much of it made effective by implementation of the law, through the Equal Employment Opportunities Commission and Title IX of the educational provision of the Act, opened many gates and tracks to women, creating major shifts in the *opportunity structure*—a concept to which Merton (1995) was to devote a good deal of attention in the years to come, although he did not personally explore the position of women in a major way.

[9] I was a founding member of New York NOW in 1966, and Rose Coser, another of Merton's students became active as well in the women's movement at this time. Friedan was also a friend of William J. Goode, another member of the Sociology Department at Columbia who also joined the organization when it was founded.

Following Merton's theoretical paradigm, I wrote in my book, *Women In Law* (Epstein 1993 [1981]) that it was particularly interesting to see how changes in the opportunity structure altered the expectations of women and changed their perception of the statuses they could now acquire without opprobrium. In my view, these changes dealt a blow to socialization theory as it was then conceived, since social psychologists had predicted it would take a generation before girls might change their expectations regarding professional careers. Further, the developmental stage theories of psychologists such as Lawrence Kohlberg (1981) and Erik Erikson (1963) did not seem to hold, because one could not see a definite pattern for females of this generation. The theory of Carol Gilligan (1982), who suggested (along with other "standpoint" feminist theorists writing in the 1980s) that women and girls had a distinctly different sensibility than men and boys, was undermined by the fact that thousands of girls who were socialized similarly to their not so older sisters, swiftly abandoned the usual paths to elementary school teaching or nursing and instead chose to go to law school, medical school and into engineering (although, of course, in fewer numbers). Thus one saw, as I documented in *Women in Law* that women's representation in law schools went from only 3 to 10 percent of law school recruits beginning in the mid-1970s, now rising to about half of each new class. Barriers to their entry were eliminated by a succession of Supreme Court decisions ending the sex-selective recruiting practices of law schools and high profile law firms, many of which had refused to interview women law graduates.

Further, instead of isolation in the legal specialties once deemed appropriate for women (often the ones men regarded as low prestige, low profile and routine), women now were brought into the formerly exclusively male specialties of litigation, mergers and acquisitions, tax, and bankruptcy law. Similar changes were happening in medicine, breaking down the barriers that prevented women from getting residencies in such male-dominated spheres as surgery. And in science, laboratories began to open opportunities for women to do research. In a later study of women's advancement in the legal profession (Epstein et al. 1995) I found that barriers remained although women had equal access to the most prestigious firms in the United States at the recruitment level. Their representation in the ranks of partnership in these firms was sharply lower; about 18 percent. This reflected both glass ceilings imposed on women by still resistant gatekeepers in the professions, and the hesitance of some women themselves to aim for the top because of ambivalence about reaching for the highest stratum (Merton and Barber 1976).

Although the law changed the opportunity structure and permitted women for the first time to enjoy the rewards of the Matthew effect, economic pressures also opened the profession. Merton always instructed his students to be aware of such variables as numbers of individuals and size of organizations as important sociological dimensions. Massive changes occurred in the American legal system and then spread to the rest of the world. The economy was expanding and the financial sectors were engaged in the business of mergers and acquisitions and other complicated financial transactions. When the sociologist Erwin Smigel (1964) described "the Wall Street firm," alluding to the largest firms in the country, they were composed of 100–150 lawyers. Today many number upward of a thousand to two thousand lawyers. Smigel also documented the tight connections among upper class Protestant men who went into law, and their banking counterparts. These firms were to change, challenged by an explosion of opportunities to work in new financial spheres—spheres they formerly disdained. Further, over the years, new firms had been established, many by lawyers from different religious and social backgrounds, in a tradition forged in the 1940s and 50s by lawyers who had worked in Franklin D. Roosevelt's "New Deal." Now, given the unprecedented growth of the economy, the new firms hired legal talent from formerly reviled groups—Catholics, Jews and African-Americans. Finally, bowing to changes in the law and the profession's new norms, the WASP legal establishment began to employ lawyers from groups it formerly regarded as unacceptable.

Of course, today we are witnessing a diminution of the opportunity structure as even leading law firms face the global meltdown. As business has decreased, some have even offered inducements to their new law associate hires to take a year off from work and do something else while being paid a third or more of their agreed upon salaries; or even being paid large sums to not come to work at all.

This comes at a time when once again women have also been persuaded that they should fill their time-honored role obligations to become mothers—and not only to bear children, but to spend years of "quality time" with them. Such notions as "attachment mothering" have acquired a following of educated women who might have been in line to compete for high level jobs; and many women are persuaded they might be morally deficient if they hire surrogates to care for children, exploiting women of a lower social class (Ehrenreich and Hochschild 2003). Using Merton's ideas about time sequencing and *socially expected durations*, one can surely see the clash between these competing cultural mandates—a point I made at an earlier conference about Merton's contributions to cultural sociology (Epstein in Calhoun, forthcoming).

Merton's ideas about time have also strongly influenced my work on time norms as an instrument of status and role enforcement (discussed in a book I edited with Arne Kalleberg, *Fighting for Time* [2004]). Building on Merton's work (1984a), I showed how cultural views about how much time ought to characterize the stages of a career, or a life, have multiple effects. It is important to note how different this concept is than those of such psychologists and psychoanalysts as Erik Erikson (1963), Lawrence Kolberg (1981) and Daniel Levinson (1978), who (as I noted before) believed there was some natural progression of stages in people's lives. Merton outlined the normative aspects of life stages, those that are prescribed and institutionalized, and further noted how linked they are to the acquisition of prescribed status-sets. Later, his former students who became distinguished sociologists, Lewis Coser and Rose Laub Coser (1974), were to describe in detail the time restrictions exacted by such "greedy institutions" as the professions and the family.

Merton's seminal papers on time also frame the process whereby time norms have consequence for the stratification system. They may serve to include or exclude pools of potential recruits to high-level jobs who cannot conform to them. For example, consider the prescribed time sequences used to move a student from science major to medical school student to intern to resident to doctor. Such time prescriptions have the consequence, even if unintended (*unintended consequences* were an important component of Merton's analysis of social change) of eliminating individuals whose social circumstances do not permit conformity to the fixed time mandates. I found in my analysis of women's career tracks that putting Merton's ideas about *role strain* together with his concept of time sequencing showed how women, could be (and were) sidelined in the professions; even now as they can gain entry they find it difficult to delegate responsibilities attached to their roles, and to compartmentalize their roles. This observation came from hearing senior lawyers admit that their junior female lawyers were smart, dedicated and worked hard, but they understood that their priorities had to be different from those of men if they were responsible mothers, intimating that perhaps women's priorities had to be different from those of men (Epstein et al. 1999). Further, if women did their jobs swiftly in order to leave work and get home to see their children, they were not given as much credit as men who may have produced the same amount of work, but ostentatiously did it off hours, in the evening and at night, thus demonstrating their dedication. Further, men could delegate child care to their wives (and were urged to do so), but women were seen as exploitative if they delegated their parenting duties to a surrogate. All proof that, as Merton pointed out, norms governing how individuals ought to spend their time were powerful mechanisms in society and difficult to alter.

I conclude this article with the observation that Merton's insistence on the interplay between culture and structure has hardly been given full due. There is, first of all, the appeal of what he called "the Thomas Theorem"—W. I. Thomas's observation that "when men believe something to be real, it is real in its consequences" (Merton 1957). We see this every day as we open the *New York Times* and read another account of new (or old) restrictions on women's opportunities and freedoms in various places in the world, legitimated by beliefs in women's basic psychological and intellectual difference from men, the pollution of their presence, or their behavior as a mechanism by which honor of a tribe is upheld or diminished.

Merton has often been labeled as a theorist of the status quo. Yet his analyses clearly showed the dynamic processes in the maintenance of practices in society that are unjust. His examples of stereotyping and the self-fulfilling prophecy, referring to African-Americans and Jews are forgotten. Perhaps this is because his language was not peppered with accusations of exploitation and domination in the ways that subsequent theorists have excited generations of sociology students. Nor did he identify or deride those responsible for implementing the norms that insure the compliance that keep us all living as social actors conforming to social norms that prop up the stratification system.

His gift to social scientists was in the ways he illuminated and analyzed the micro processes by which *implementation* takes place, far below the radar of society. He often saw and understood what other social scientists fail to see or understand. Many younger sociologists today work with the concepts he provided without knowing he is the source of them, since they have become part of the vernacular, and thus their genesis has been "obliterated through incorporation" another concept he coined. Others cling to old stereotypes in the new clothing of cultural studies and other fashionable frameworks. Robert Merton always credited those who went before him as he traced the genesis of ideas in his writings and in his lectures. But his ethos seems to have been replaced by a drive to devalue or simply ignore his and many of his contemporaries' contributions by what he would have called "status judges" prominent in Sociology today. I hope this review of the powerful and evocative concepts he authored will insure that today's scholars will recognize how strongly influential Merton's contributions to social thought are when it comes to understanding the very basic processes by which individuals and groups maintain their advantages of power and control, not only with regard to the gender issues I have pointed to in this article, but also in many other diverse spheres.

A Tribute to Robert Merton: Protestant and Catholic Ethics Revisited

Rivka Feldhay

My small tribute to Robert Merton is done from the point of view of a historian believing, as I think he did, that neither historians nor sociologists can afford an easy choice between grand narratives—that is theoretical or conceptual frameworks—on the one hand, and precise empirical studies on the other hand. Both are essential for presenting a sound argument in the humanities and in the social sciences. I was a student of Professor Yehuda Elkana in the early eighties in Jerusalem, and it was Professor Elkana who introduced Merton's *oeuvre* in his classes. Moreover, Elkana knew Merton personally and created the opportunity for us—his students—to meet and discuss with Merton in person. Thus, the sociological-historical writing of Merton has become for us paradigmatic for the tradition he himself coined the "middle-range" theory, the term he chose to denote the cross-fertilization between the theoretical and the empirical aspects of research in our field (Merton, 1957). In my historical studies I translated the term to mean conceptually guided intellectual history and history of science. Very soon after completing my dissertation on Galileo and the Church I was keen to join Elkana in organizing an international workshop on the fiftieth anniversary of the publication of the "Merton thesis." This thesis is remembered for its focus on the relationship between Protestantism and science. We decided to re-examine it by thematizing Merton's more or less hidden presupposition that science lagged behind in the Catholic world of the seventeenth century, especially compared to its role in the Protestant environments of England and Holland. The title of the workshop and the thick volume of *Science in Context* that followed it was: *"After Merton": Protestant and Catholic Science in Seventeenth Century Europe* (Feldhay and Elkana 1989).

It would be fair to say that since then, science in Catholic countries in the seventeenth century has become a topic of discourse. I am thinking not of monographs on philosopher-scientists such as Descartes and

Galileo—who have been subject for research and debate for some time —but about minor figures such as Guidobaldo del Monte (Van Dyck 2006), Mersenne (Dear 1988), Clavius (Lattis 1994) or Kircher (Findlen 2004); about correspondence networks such as that of Peiresc; about practicing science in the Churches (Heilbron 2001); about the dissemination of scientific knowledge by missionaries especially in China (Elman 2005); and above all about science teaching in the Catholic colleges, especially by Jesuits—a teaching that was often the most advanced in Europe in the seventeenth century. With this kind of research, our picture of the Scientific Revolution is gradually changing not only through a theoretical and ideological debate, but mainly through uncovering much new material that has been considered irrelevant as long as science was mainly identified with the Protestant Ethics on the one hand and with England, the Puritan revolution and Latitudinarianism (Shapiro 1983) on the other. It turns out that the monumental event defined as a "revolution" by Alexandre Koyre and his followers, which required about 150 years to be accomplished, was only locally linked to the Protestant religious creed and the particular politics of England. In fact, it was connected to more global European conditions, namely: a) a process of re-constituting religious identities in the sixteenth century, in which both Catholic and Protestant faith took part (Reinhard 1977; Reinhard 1981; Reinhard 1989; Schilling 1983; Prodi 1982; Prodi and Reinhard 1996; O'Malley 2000). Both had to cope with the deep conceptual consequences of the "rhetorical turn (Moss 2003)" and the rise of the "self" as a source of moral authority brought about by humanism. And b) both had to deal with the emergence of sovereign states in which the place of religion in the public sphere had to be re-conceptualized and re-organized (Maravall 1986).

Underlying these two processes one concept looms large: by which I mean the concept of "authority" (*auctoritas*). Many writers on Protestant ethics—including Merton—believed that the main thrust of Luther's rebellion—expressed in the slogan *sola fide*, *sola Scriptura*, *sola gracia*—consisted not only in rejecting the Catholic church's pretence for ultimate authority in the reading of the Bible and giving the sacraments, but actually entailed a rejection of authority as a cultural principle. Thus, in intellectual history Protestant ethics has been depicted as a preliminary condition for the autonomy of knowledge by freeing knowledge from traditional *auctoritates*.[1] Similarly Luther's strategy of accepting political authority in a well defined political realm in exchange for freeing individuals to practice their faith in private has been considered as a precondition for religious tolerance (Dillenberger 1960, 130). In contradistinction, Catholic culture has been perceived in this context as buttressing itself in the notion of tradition and the infallible authority of the Church. But any attempt to understand the role of religion and church, compared to the role of state in the development and growth of science, should clarify and historicize the concept of authority that has so many meanings in different contexts. Moreover, the epistemic role of authority in scientific discourse itself cannot be separated from the understanding of the notion of authority in the religious and political context.

On this occasion, I would like to dedicate my contribution to a short examination of the concept of authority in the heart of Catholic discourse of the seventeenth century, in fact in its discourse on faith. The sources I have chosen are two commentaries on Thomas Aquinas's treatise on faith, written by two Jesuit scholars: Franciscus Toletus (circa 1533–1596), who lectured in Rome on Thomas Aquinas's *Summa* (Aquinas 1947) in the 1560s, and Gregorius Valencia (1549–1603), who taught and wrote in the University of Ingolstadt in Germany. My examination will show that Catholics in that period tended to historicize not only the meaning of the Scriptures, but also the notion of authority itself. The theologians' conceptual and political work with the notion of authority—so it seems to me—was constructive in assisting Catholic teachers of science to accommodate the new science within the traditional Aristotelian conceptual framework to which they were committed by their Jesuit faith. It also allowed the Catholic establishment to accommodate itself within the political realm of absolutist states such as France under Louis XIV and the Habsburg rulers. Toletus' interpretation proceeded through a series of distinctions. Commitment to ultimate Truth on the part of believers, he argued, has three conditions: a) that the thing we believe truly exists (*modo in*

[1] On personal judgement—the duty of "libre examen" in Merton's words—and its role in the development of seventeenth century science see: Merton 1970 [1938], 136; Jones 1965 [1936]; Mason 1953, 66; Westfall 1958, 219; Hill 1965, 113.

essendo); b) that the speaker of truth is absolutely trustworthy in his speech (*modo in dicendo*); c) that the message is asserted in such a way that there is no gap between message, meaning, and intention (*modo in asseverando*). According to Toletus, then, any truth—including ultimate Truth—has three dimensions; something to which truth refers has essence and existence. That is "veritas" (truth). For the believer, however, truth only exists as far as it is represented, or spoken. Hence the second dimension of truth is embodied in the trust accorded to the one who represents it—a speaker—who must be truthful (*verax*). Last, the meaning and intention are not the same as the words or representation. Truthfulness (*veracitas*) only exists when such gap is closed, and this is the third dimension invoked by Toletus. Thus he claimed that:

> Applying therefore what we have proposed, we say: since by faith we believe that which is said and revealed by God, we can treat three aspects: the propositions in which we believe—these are the truths and conclusions of faith to which we give our assent; the one whose words we believe—this is God himself; the reason why we believe such propositions and bring up faith to God, which is the veracity of it in saying and asserting. (Toletus 1869, 12)

Toletus' text discloses a fascinating shift in the concept of truth. His emphasis moves from Ultimate truth as an object to truth as a complex interaction between the object, its representation and the believer. This critical analysis of truth must have been anchored in the humanistic practices of Toletus' age that drew attention to the interconnection between the reference and significance of concepts, and the context of their communication. What is most relevant for the present paper, however, is to clarify how the shift in the concept of Ultimate truth redirected Toletus' thinking to the different modes of the believers' reception of that truth: through the beatific vision in the world to come, by means of theological science, and through faith. The beatific vision, the science of theology, and faith, he claims, share first truth which is God as their object "*de quo*." However, they differ in that the saints have access to this truth "by vision," that is in an immediate way; theological science gives assent to its first principles but from them moves by demonstrations to further conclusions; whereas faith is wholly anchored in authority, and that is how it differs both from knowledge and from belief: "Faith, as faith, is distinguished from opinion and science by the fact that it rests on the authority of the speaker alone" (ibid., 13).

I claim that the distinctions made by Toletus between the three aspects of first truth—"*prima veritas*"—and between the three modes of reception of that truth, enable Toletus to ground the authority of the Church as the necessary proponent of the truth of revelation to the believers. From Toletus' perspective it was not enough to say, with the whole tradition, that our inability to apprehend God in His essence (*modo in essendo*) is the reason for him revealing Himself to us through Christ and Scriptures, in order that we give our assent to his words (*modo in dicendo*). It was moreover necessary to insist and instruct the believers to think that apart from direct vision that is beyond humans in this world, in the holy message given to humans here and now a gap always exists between the message and its meaning or intention (the difference between "*modo in dicendo*" and "*modo in asseverando*"). Therefore, the message is always in need of authoritative interpretation. And this authority, which is found in the words of revelation in an immediate way, finds its mediate embodiment in the Church. In other words, it is in the Church that the authority to authenticate first truth's veracity in asserting it, "*modo in asseravendo*" is invested: "[…] There is no authority in the Catholic faith apart from the divine authority, be it directly, by itself, or indirectly, by the Church; authority has its foundation in veracity" (ibid.).

Authority, he actually says, has its origin in the distinction between "*veritas*" (truth) and "*veracitas*" (truthfulness). Furthermore, by identifying the object of faith—"*Ultimate truth"*—not only with God, and not only with his revealed word, but also with the Church, Toletus is also able to clearly differentiate the *habitus* of faith both from the immediate way of knowing and believing expressed in the beatific vision on the one hand, and from sacred doctrine—or theology—on the other hand.

Toletus' *Enarratio* (Toletus, 1869) seems especially significant for grasping the new epistemological sensibilities concerning words and their meaning common among theologians of his age and milieu. The emphasis on a thorough humanistic education prior to the study of philosophy and theology in Jesuit schools resulted in a deep intellectual conviction that meaning could not simply reside in discourse for everybody to see or understand. Rather, meaning was intimately connected to the community of speakers, to the cultural and historical situation of discourse, to the identity of deliverers and receivers. Toletus' analysis was particularly useful for Catholics in their attempts to undermine the Protestant thesis according to which, with the aid of grace, the Holy Scriptures contained everything needed for an act of faith, without any need for mediating the holy message to the believers.

The political implications of Toletus' account of the problem of faith may be well traced in Gregorius de Valencia's *Analysis fidei* (de Valencia 1585). In striking contrast to Aquinas's confidence in the superiority of faith as the most secure source for the first principles of sacred doctrine, Valencia interpreted faith as a particularly obscure and insecure form of cognition. First he attempted to develop Toletus' argument about the difficulty to decide what belongs to faith (de Valencia 1585, pars IV.). The main argument that he brought—and that probably reflected the consensus among Catholics of the period—was the difficulty inherent in any human attempt to understand the Holy Scriptures. In fact, according to Valencia, the frequent failures and errors of interpreters of the Scriptures are practically unavoidable, for "one place which seems to have one sense for one person seems obscure for the other" (ibid., 88). Such difficulties, among others presented by Valencia, served as strong arguments for the necessity of an authority capable of presenting the Holy Scriptures to the believers and explicating the oral traditions, in accordance with the decree on Scriptures of the Council of Trent:

"The human authority of those who died, and declared in time past infallibly a divine sentence, is contained partly in the sacred scriptures themselves, and partly in the apostolic traditions." (ibid., 82–83) But what is the nature of the apostolic traditions' authority? Is it divine authority, as Aquinas plainly insisted, referring to "first truth" without problematizing it too much, or is it also anchored in human institutions like the Church as Toletus chose to emphasize? Valencia claimed that this authority was neither wholly human nor wholly divine. Rather it was human authority divinely inspired (ibid., pars V). This reveals his acute sensitivity to the human dimension of authority, in spite of his reference to divine inspiration. Thus, from the beginning, an element of hermeneutic dialogue is explicitly woven into the practice of faith according to Valencia. However, it is precisely the human element inserted into Thomas Aquinas's notion of "divine faith" that feeds and perpetuates a quest for closure: "But neither Holy Scripture, nor even tradition alone (if you separate from it the infallible authority which is present in the church; this is the way we speak about tradition now) is that infallible authority, teacher of faith and judge in all questions." (ibid., 83) In other words, in order to decide controversial matters of faith, an authoritative act of judgment is needed. Valencia compares this act to the exercising of judgment in a human court. Just as in human affairs, he says, there is a difference between the law as an abstract, general rule, and the living act of exercising the authority of the law, so in matters of faith the Church has the authority not only to define the articles of faith, but also to exercise its authority in a living manner in order to achieve closure in controversial matters. Just as the letter of the law are not sufficient in judging human affairs, so the letter of the Scriptures and the traditions is not enough in judging about matters of faith (ibid., 113–114). In both cases the exercise of infallible authority in the act of judgment is actually a moment of closure of the dialogical process that constitutes the inherent logic of the practice of faith. Hence such authority must be centered in the hands of one person—the Pope (ibid., pars VII). No wonder, then, that the assent to the principles proposed in the articles of faith is re-interpreted by Valencia in terms of "obedience," "obedientia mentis in assentiendo" (ibid., 72), whereas an heretic is defined in terms of conscious refusal to truth, or non-obedience: "he who denies what he knows or what he has to know" (ibid., 71).

And yet, Valencia's text also illustrated the process by which the initial idea of representing divine authority through Papal "fullness of power" (*plenitudo potestatis*) was translated into a concrete discourse about the conditions of the possibility of exercising such authority and its limitations. First he gave vent to the absolute prerogative of the Pope to make the final judgment in controversies related to faith, to exterminate errors and to propose the articles of faith (ibid., 313). There was no doubt that his judgment was infallible in everything related to the well being of the Church (ibid.). With Toletus, Valencia rejected any attempt to restrict the Pope's authority to matters connected with salvation (ibid., 323). Likewise he thoroughly treated attempts to restrict Papal authority in matters of canonization, as well as in whatever concerned the elaboration and confirmation of new religious life-forms (ibid., 315–322). Nevertheless, a multiplicity of qualifications immediately followed, one of which is particularly relevant for my theme. Valencia explicitly distinguished between the Pope's *infallible* judgments in religious matters that concern the well being of the Church, and the Pope's *restricted* authority in the field of the sciences. Thus he stated:

> If it is said that the authority of the Pope is infallible in those things that can be seen as related to piety and religion (and those related to salvation), it is not so in others that are in some sense mathematical and physical things, this is rightly said. For it seems that the authority is attributed to the Pope in all things that concern the well being of the Church, those related to piety and the salvation of souls, but not in other things related to the knowledge of the sciences. (ibid., 323)

Following Melchior Cano (1509?–1560)—a Dominican theologian at the University of Salamanca, Toletus and others, Valencia constructed a hierarchy of textual "places" in which revealed truth had been preserved and articulated along the centuries, from Scriptures, to apostolic traditions, Papal definitions, and Council's decrees, the consensus among Church fathers, doctors, and opinions of scholars, and finally the common feelings of believers (ibid., 332). Although Valencia basically accepted the thesis that the Pope embodied Church authority in matters of faith, he also maintained that the authority of the fathers who formulated the decisions of Councils was not properly divine or, rather, it was human, though deriving something from the divine. As for the authority of Church fathers and doctors, Valencia preferred to speak about the consensus among them as the source of that authority, although it was evidently presupposed that they were somehow inspired by the Holy Ghost. His remarks about the way consensus was to be achieved testify that for him the consensus was not passively infused by the Holy Ghost, but was rather constrained by historical conditions, negotiations and all other social and human strategies that could bring about a collective authoritative decision. And finally, in the far end of the list of sources necessary for the *magisterium* to attain its authority was the consensus of the believers. This means that Valencia, like any other modern ideologue, was well aware that no authority could be maintained without attention being paid to the sensibilities of the public for which and on behalf of which a decision was being made, in this case a decision about the boundaries of faith.

In trying to assess the contents and structure of Valencia's concept of authority—developed at the height of what the Church establishment saw as its struggle for survival—a good point of departure is the realization that the term "authority" has more than one reference. It is not the undifferentiated, partly mystical and therefore somewhat obscure concept used by Aquinas to describe "divine authority" but a much more concrete idea of a multiplicity of authorities from which evidence can be accumulated. This was the notion of authority explored by Valencia and justified in his text. In the writings of Melchior Cano, Franciscus Toletus, Gergorius Valencia and many other late scholastic Catholic theologians, one may trace a transition from a notion of authority as one unified, spiritual concept of divine authority to a multiplication of human authorities containing evidence of divine revelation. Authority thus loses its autonomous structure, while its human and hence historical nature is recognized.

In conclusion, one may say that the broadening of the notion of faith to include not only ultimate Truth grounded in divine authority, nor even the *Symbolum* defined by the Pope, but also those articles anchored in Church authority that was defined as human, but divinely inspired, had two parallel effects: On the one hand, as a result of such broadening, authority had to be anchored in the infallible act of judgment of the Pope—to which all Catholics were obliged, at least by the law of conscience—if not by state law. On the other hand the Pope's "fullness of power" was deliberately restricted by the recognition of a multiplicity of authorities containing evidence of revelation, a strategy that necessarily opened the way for the historization and even relativization of all knowledge relevant to faith.

The Concept of Ambivalence in the Relationship between Science and Society

Helga Nowotny

1. Sociological Ambivalence as Incompatible Expectations

In his 1963 essay "Sociological Ambivalence" (coauthored with Elinor Barber) Robert K. Merton addresses the problem of ambivalence *not* by referring to the psychological experience we all are familiar with. Being pulled in opposite direction through our emotions or wavering in decisions due to conflicting tendencies is better left to psychological theory. Seen from a sociological perspective, ambivalence is all about social structure. In its most extended sense, it refers to "incompatible normative expectations of attitudes, beliefs, and behavior assigned to a status (i.e., a social position) or to a set of statuses in society" (Merton 1963).

Sociological ambivalence therefore is closely linked to the social definition of roles and statuses, not to the feelings of different types of personality. Bureaucrats wavering between the expectations of citizens for personal treatment of their case and the obligation to act in an impersonal way or scientists being caught between "the value of originality, which leads them to want their priority to be recognized, and the value of humility, which leads them to insist on how little they have been able to accomplish"—are empirical examples for the widespread ambivalence inherent in different roles and functions.

Ambivalence arises through processes which induce social structures to generate the circumstances in which ambivalence then becomes embedded in particular statuses and the roles associated with them. In order to reconcile the conflict between contradictory norms and their counter parts, they can be accommodated by an oscillation of behavior, just as a physician is trained to practice both—a measure of affective detachment from, and a degree of compassionate concern about the patient.

The oscillation to which ambivalence gives rise requires a dynamic organization of the social structure since only through appropriate alternation can the various, incompatible functions of a role be effectively discharged. The resolution occurs in time. Dynamic accommodation requires that each of the normative expectation and their normative counterpart can run its—alternating—course.

In the following pages, I will use the insights offered by Merton's concept of ambivalence to clarify some of the reoccurring expectations and conflicts in the often tension-ridden relationship between "science" and "society." The social definition of the status of "scientist," "expert," "lay person," or "scientific citizen" is fraught with the risk of quickly becoming vacuous. The "normative expectation of attitudes, beliefs and behavior" assigned to these roles and status-sets vary widely. Expectations are bound to change in accordance with the specific configuration in which encounters take place and the situation in which they occur. Last but not least, given the heterogeneous arrangements in which the terms "science" and "society" are made to stand in for otherwise intangible social phenomena in constant flux, ambivalence is seemingly omnipresent. Yet, as I will show, it remains highly structured.

2. The Passions and the Interests

One of the recurring incompatibilities between norms pertains to the expectations and attitudes linked to passions and interests. In his authoritative reconstruction of the intellectual debates of the 17th and 18th century that helped to transform a once morally shameful enterprise, long condemned as the sin of avarice, into the triumphant force for future industrial and economic development, Albert Hirschman shows how nascent capitalism was assigned the role of containing the destructive passions that were driven by ideology and religion at the time. The ironic twist—or is it the unintended outcome of sociological ambivalence?—culminates in the finding that capitalism was originally supposed to accomplish the repression of the passions in favor of the superior interests of commerce and commercial life (Hirschman 1977).

History has since revealed that the interests were far from harmless and that the passions, if repressed, would surface elsewhere. It is no coincidence that the passions embody the expressive, emotional function of many norms, while interests often correspond to the instrumental, calculating function of counter-norms. Just as the passions regained control over economic life, as the latest financial crisis demonstrates so vividly, the interests beg the question in whose interest they are. In particular, individual interests collide with collective interests, leading to passionate debates about their compatibility.

The entanglement of passions and interests is not restricted to capitalism, or, rather, its triumph has invaded also the scientific life and the social structure of science. Contemporary science and technology, the technosciences as they are often called, have significantly expanded the interplay of passions and interests. Not only do commercial interests abound and are researchers constantly reminded to direct their work towards profitable applications, but the growing density of interactions between "science" and "society" has led to numerous value conflicts. Legal and regulatory requirements have become paramount for every new emergent technology and efforts are under way to strengthen deliberative democracy by giving citizens a voice. Society 'speaking back to science' has become an irreversible feature of the relationship between them, and science is well advised to listen. Meanwhile, the passions and the interests can be found on every side of the artificial divide.

As I have shown in *Insatiable curiosity* the most paramount passion on the side of the science and scientists is their curiosity (Nowotny 2008). It is the main driving force that pushes them into the territory of the yet unknown. "I am not especially talented" is one of Albert Einstein's typical understatements, "only passionately curious." Scientific curiosity celebrates the unpredictability in the quest for new knowledge and is ever ready to embrace failure. Curiosity refuses the acceptance of pre-set goals and assigned objectives. It insists on following its own intuitions and sense of direction. It is deeply subversive, a fact that was recognized by the religious and secular authorities who were distrustful of the *libido sciendi* which they sought to repress as concomitantly presumptuous, arrogant and dangerous.

As a passion, scientific curiosity contains a tacit plea. It wants to be recognized as a powerful emotion that overwhelms those whom it seizes. They cannot but follow their instincts and drive. The tacit plea for official recognition culminates in the explicit argument for leaving a free space in society in which science is recognized as autonomous, bound to its own rules and ethos. Realistically, the institution of science has always known and accepted that its space of autonomy is limited in practice and that scientific autonomy is always relative. Nevertheless, the negotiations with society, regulatory authorities and the state continue. Every major, new scientific break-through that harbors the potential for far-reaching changes and a big societal impact, introduces a new round in staking the respective claims.

The hard chip of the bargaining process on the side of science is its impressive historical record: barely four hundred years after the institutionalization of modern science, the world has changed beyond recognition. The current scientific-technological civilization has reached a global scale and continues to accelerate in its advances. This has been enabled, so the core argument goes, by granting an autonomous space to scientific curiosity. Without allowing this passion to run its free course, the goose will soon cease to lay its golden eggs.

Yet no society, past or present, can permit such a powerful passionate drive to thrive without limits and control, all the more when scientific curiosity asserts that it does not know where it will end up and what it will find. Hence, scientific curiosity is amoral, not immoral. It does not want and cannot take responsibility for the unforeseeable consequences of its unpredictable course. The claim that science benefits humanity and that, by proxy, scientific curiosity explores the paths into the future that will lead to further human betterment, is not sufficient to grant it exemption. Therefore, society undertakes every effort to tame scientific curiosity. It has to tread a subtle line and seek for a balance in doing so. Too much freedom brings loss of societal control and potentially excessive, uncontrollable results from curiosity. Too many constraints carry the risk of stifling it. History has sufficient examples in store when a period of flowering artistic and scientific curiosity was followed by long decline and ultimate stagnation.

In liberal democracies the taming of scientific curiosity takes the form of channeling it into contexts of economically promising directions and commercial usefulness. But liberal democracies must also attend to the demand of their citizens for a democratically legitimate participation in the decision-making processes regarding their scientific-technological future. The discourse about potential or real risks that has dominated the public arena in the past decades has been yet another attempt at taming. The current transformation from government to governance, expressed in various deliberative procedures and other participatory arrangements that seek to include stakeholders, users and concerned citizens, reinforces this trend.

Yet the greatest challenge and the most overt effort of taming come with the public discourse on values. The clash with the plea of scientific curiosity for exemption and the moral impulse to restrict it could not be greater. While the interests in the first strand of taming under the banner of innovation are unapologetic about their economic nature, and while the risk discourse prompted a more cautious approach culminating in the recognition of the precautionary principle, the attempt to tame scientific curiosity on ethical and moral grounds reveals the most profound contradictions and incompatibilities of values. The call to pose restrictions on scientific inquiry on moral grounds often appeals to presumed immutable values. But values change under different historical circumstances and scientific activity itself is grounded in a publicly recognized value—the value of free inquiry. Moreover, in pluralistic societies no minority can be permitted to impose its values on the majority.

Taming has been successful, to a certain degree, in the governance of bio-ethics that has succeeded to establish itself as the 'effective currency of a global moral economy' (Salter and Salter 2007). Bioethical guidelines refer to a limited number of principles that need to be observed. As the lead currency for an economy that operates in a transnational context, it also allows to be transferred into different cultural contexts. Scientific curiosity has accepted bioethic governance as taming instrument, since it offers predictability and a measure of (ethical) standardization, both indispensible as framing conditions for emergent technologies. Yet, the compromises reached are inherently unstable, since they offer no guarantee as reliable precedents for future developments. Nor has there been a harmonization in regulation in different countries, nor within the European Union. As the heated debate about embryonic stem cell research both in the US and in Europe shows passions and interests continue to run high.

3. Show, Tell—and Sell the Wonders of Science: The Case of Fossil Ida

One of the oldest forms of dialogue between "science" and society" is the outcome of scientific efforts to assure continued support by demonstrating the fruits of its curiosity. Displaying the wonders of science was the first successful attempt by early modern science to gain favour from wealthy and powerful patrons. As Richard Holmes shows in *The Age of Wonder*, commercial interest was never far away at a time when the Romantic generation discovered the beauty and terror of science (Holmes 2009). But sociological ambivalence between the passion for science and the interest in exploiting its results for commercial gains was then held at bay, at least for some of the leading figures of that generation. Wealthy young men with aristocratic privileges, like Joseph Banks and others, were exempt from experiencing a conflict, since the role of scientist was still widely open to amateurs. It was self-evident that for men of lesser means, aristocratic or wealthy patronage had to be found to enable them to live the life of an independent "gentleman" devoted to the pursuit of knowledge and new discoveries.

After the end of the Age of Wonder scientists became accustomed to live the life of a professional academic/researcher. Eventually, patronage assumed the form of a regular salary that was paid either by the state or by industry. Even during the climax of industrial research reconstructed by Steven Shapin, who (wrongly) faults Robert K. Merton as having proclaimed that academic "pure" science was the only real science and those working in an industrial context were morally inferior, the sociological ambivalence experienced by scientists working in industry was presumably less or on a par with the kinds of ambivalence prevailing in academic life (Shapin 2008). It seemed however, that the "oscillation of behavior" in observing incompatible norms had been internalized sufficiently well as a conflict-reducing strategy by both academic and industrial scientists. Structurally, they were separated and worked in different, institutionally distinct spheres. This institutional separation assured that certain kinds of ambivalence would remain at a low level.

Today, the boundaries between public and private research have become blurred and so-called public-private partnerships are the coveted new organizational forms. Public science at universities is no longer exempt from making every effort to gain support from wider society as well as from government, industry, and business. The widely shared expectation for science is to "show and tell" and, if possible, also to "sell." It has led to a booming surge of intermediary agencies, media consultancies, and public relations firms with the aim to further "engagement" with the public. Many research organizations have set up special units devoted to this task and to provide coaching for their researchers on how to communicate with the media.

While show-casing the most exciting latest research findings or discoveries has been a constant feature, new emphasis has been added more recently under the pressure for researchers to communicate with the public. The wish to share part of the passion that has inspired the scientific work is understandable. Where passion runs high, it wants others to join in. Besides, it adds a welcome, emotional tone to the style of communication, that is highly welcome as the scientist appears more like an ordinary fellow. The inherent ambivalence between the expectation to devote oneself to the production of new knowledge, regardless of its commercial pay-off, and to be seen as eagerly pursuing its potential commercial benefits, has thus been mitigated by setting up platforms and public out-reach activities. Ambivalence becomes mediated through new organizational forms and the oscillation of behavior can be outsourced to these activities. Norm and counter-norm are being transformed into a new normative expectation: that of engagement with society.

Nevertheless, ambivalence persists. It is brought to the fore through the reaction that follows when the behavior is perceived as transgressing the norm. This is the story of *Ida,* a fossil that was discovered in 1982 at the Messel pit in Germany, but presented under a huge publicity and media coverage only in May 2009. Its age is approximately 47 million years and its scientific name is *Darwinius masillae*. It belongs to a group of basal or stem group primates from the Eocene epoch.

After its original unearthing, the extremely well preserved fossil slab ended up in the hands of a private collector who eventually decided to sell it. Paleoanthropologist Jørn Hurum was so convinced of having stumbled upon one of 'the holy grails of science' that he persuaded the museum in his native Oslo to make part of the exorbitant funding available. After working in secret for two years, a small research team published their results in the open-access journal *PloS One*. The authors classified *Ida* as a significant transitional form between the prosimian and simian (anthropoid) primate lineages, which led the media to tout the fossil as being the famous 'missing link' in the ancestry chain between anthropoids and humans.

Hurum orchestrated the launch of the fossil, charmingly named after his young daughter, as a spectacular scientific and media event. Immediately, *Ida* reached iconic status and news about her discovery circulated on major TV stations and hit the front pages of many newspapers. Also the location of the presentation was well chosen. Staged in the American Museum of Natural History in New York, the fossil became known through a publicity campaign which was highly unusual for a scientific discovery. Despite unanimous public interest and acclaim, the scientific community reacted with less than enthusiasm. It questioned the scientific significance of the fossil. As it turned out, it was certainly not the "missing link" and critics maintained that it would not match the significance of *homo floriensis* or of feathered dinosaurs either. Harsher criticism was in store for the publicity that Hurum had been able to generate. He was accused for seeking celebrity status. Hurum defended himself with a simple and straightforward argument: what is wrong in sharing his passion for science with the public, he asked. Besides, the wide publicity gained by *Ida* would help to cover part of the large sum the Oslo museum had to pay. In a later interview, he also conceded that the publicity got out of control and that some of the claims that were made were not justified.

Is this yet another—banal—story of how science, when playing with the media, might catch fire, or another—cautionary—tale about the jealousy of the scientific community when one of its members gains undue celebrity status? It is also a story about the persistence of a situation of classical ambivalence in science, that pits monetary interests and publicity on one hand against serious research and humble, not publicity seeking behavior on the other. Hurum was expected to follow the norms of conventional research and publication behavior. Yet how could he have done so, if he had not persuaded the museum leadership in Oslo to pay a huge sum for a rare and, moreover, a beautifully preserved fossil that lent itself well for visual presentation and popularization? Having succeeded in getting the museum to pay for it, he felt under obligation to help raise at least part of the funding that went into the deal.

The instrumental interest in fundraising collides with the expectation to keep a low public profile and to publish in a conventional manner. In the case of *Ida* oscillation of behavior was simply not an option. Without fund-raising, no fossil; without a fossil, no interesting research and publication, whatever its ultimate significance may be. Oscillation between competing expectations simply collapsed in time. *Ida* and Hurum's passion for her carried the day and brought the news about a new fossil in the chain of beings to a world-wide audience. The reaction by his colleagues shows that this should not become the norm. The old order of incompatibility had therefore to be restored, at least for now.

4. The Rise of the Scientist-entrepreneur: The Displacement of Ambivalence

So far, examples have been presented in which ambivalence, perhaps paradoxically, becomes stabilized through oscillation of behaviour. But stabilization can also occur when organisations set up new mediating units, thus creating new roles whose function it is, at least partly, to transcend older kinds of ambivalence. These are processes affecting the social structure of science, but can also lead to the emergence of new kinds of expectations which may attempt to reconcile previous incompatibilities. In the remaining pages I want to show that ambivalence can also become structurally displaced.

Recent developments in the life sciences provide a fertile ground for a preview of current transformation in the making (Nowotny and Testa 2009). The spectacular rise of biotechnologies as the new instruments that have turned the cell into a laboratory have also vastly expanded the opportunities for commercial, gainful exploitation of "life itself" (Rose 2006). The term "biocapital" has made a career in the literature (Helmreich 2008) and can be interpreted as one of many attempts to make sense of natural processes that have become open to artificial manipulation and operational control. Rapidly expanding research fields, like postgenomics and synthetic biology in particular, have helped to create a research environment in which scientists are no longer the only actors, even if they remain major ones. They are now interacting on a daily basis not only with new entities that they create in their labs, but also with venture capitalists, business executives, corporate lawyers, non-governmental organizations and others that now populate the new research fields. Hybrid organizations emerge, like private companies acting as non-profit organizations, alongside the traditional public and private-for-profit ones. Funding has become a major preoccupation in its own right, as have IPRs and other legal and regulatory issues.

In short, the role of the scientist as researcher is undergoing a major transformation. The life sciences in particular have become the breeding ground of a new species, the scientist-entrepreneur. Gone are the days when a scientist had to choose between working within academia or industry and equally gone seem the days when conflicting norms demanded allegiance either to scientific curiosity without contamination by profit motives or to admit one was working for industry, under suspicion to be potentially at the edge of morally dubious practices of science. Now, it seems, the winning formula has been found for how "to have fun" and "making a lot of money" at the same time. One of the foremost representatives of this new breed, who has become, or has turned himself into an icon of the molecular age in the life sciences, is Craig Venter.

He acquired his place in history as the scientist-entrepreneur who taught the world how to read genomes faster than anybody else and who narrowly won (officially, the outcome was declared a draw in June 2000) the race between the publicly funded Human Genome Project and Celera, a private company founded towards the end of demonstrating that its shot-gun sequencing technology would outperform its rival. Venter has moved on since then. Among his latest exploits was the launch of a project to sequence the microorganisms of the open oceans in order to identify hitherto unknown microbes. On board of his research sloop, *Sorcerer II,* he circumvented the globe, studying the genetic material from uncultured marine microorganisms which were sampled in the open oceans. He is now off to the Baltic, the Mediterranean and the Black Sea, expecting further insights into the rich diversity of marine life which will help him and his company to make fuller use in understanding and therefore being able to build life as part of the rapidly developing research field of synthetic biology.

As scientist-entrepreneur, Venter combines the passion for science with a self-professed love of the oceans (he is also a passionate sailor) and an indomitable scientific curiosity. But the passionate driving forces extend beyond the lab and the conventional institutions of science. In his autobiography, he depicts himself, in his race against narrow-minded peers and bureaucratic constraints that stifle any risk-taking research initiatives, as being equally passionate in creating his own research environment and the necessary infrastructure. His ability to raise private capital allows him an unrivalled freedom for his passion for science. Business plans and business models are as familiar to this research environment as are the right kind of strategies regarding intellectual property rights, data access and the invention of new sequencing methods and tools. Ambivalence in the traditional sense of feeling exposed to conflicting pressures and expectations seems to have vanished.

As a role model Venter is so convincing that he, alongside with a handful of others from the computer and IT world, is greeted as one of the pioneers of a "new Romantic Age" by Freeman Dyson. According to Dyson, there is a possibility that we are now entering such a new era (although he admits that it is too early to tell) (Dyson 2009). It will be centered on biology and computers, just as the old Romantic Age (Dyson refers to Richard Holmes) was centered on chemistry and poetry. Other candidates for leadership in this pioneering bunch are Kary Mullis, who invented the polymerase chain reaction, and computer wizards like Larry Page, Sergey Brin and Charles Simonyi. This list can be expanded to include the likes of Dean Kamen, a medical engineer who builds linkages between living human brains and artificial limbs. In Dyson's vision, the evidence for the new Romantic Age would entail a shift backward in the culture of science, upgrading the individual over organizations, and permitting the return of "amateurs" like Venter.

The scientist-entrepreneur may have left ambivalence behind, since the new role explicitly defines and enables the co-existence of passion for science and interest in its monetary rewards. A new compatibility has been achieved, and it is used to further expand the range of the previously impossible, as when money gained is reinvested into the next research project fuelled by curiosity. But has all ambivalence really disappeared or has it simply been displaced and relocated in different, and new hybrid structural assemblies and arrangements?

Ambivalence is very much in evidence in newly arisen, incompatible demands and expectations. One such case is the ongoing discussion about open access that targets one of the most ambivalent concepts in contemporary science: intellectual property rights. While patents are nothing new for science and certainly not for technology, IPRs have achieved unprecedented salience alongside the possibilities offered by powerful biotechnologies that allow to manufacture new living entities and have transformed cells to work as technologies. A field like regenerative medicine, which encompasses tissue engineering as well as stem cell technology, faces a range of practical issues and problems involving regulatory systems and huge logistical questions of transporting living human cell-based products (Franklin and Kaftanzi 2008). Ever since the US Court of Customs and Patents granted Ananda Chrabarty a patent on a living microorganism in 1980, a taboo was broken and has opened endless debates about what is, should, and never would be patentable.

The ambivalence is further enhanced by questions that pertain to issues of what is to be regarded in the private interest of a company, a university, an individual and what needs to be preserved as a public good, with open access to all. *The Public Nature of Science under Assault* calls out for new institutional arrangements, in which science, politics, markets and the law are challenged to find a stabilizing framework in which oscillations between those demands are made possible that cannot be reconciled otherwise and hence will remain incompatible (Nowotny et al., 2005).

The debate about open access is merely one, the latest and public expression for the uneasiness which casts its shadow over ever-widening ranges of the contemporary scientific landscape. The more data become available, the more pressing the issues become that surround them. The nature of data varies widely, but their growth in numbers and volume is spectacular. The European Bioinformatics Institute, located at EMBO in Heidelberg, provides updated lists of the number of complete sequences of phages, viruses, archaebacteria, bacteria and eucaryotes that have been registered. The BioBricks Foundation is a non-profit organization that encourages the development of BioBrick standard parts, which are DNA sequences of defined structure and function, designed to be incorporated into cells to construct new biological systems. Currently, the MIT's Registry of Standard Biological Parts, where BioBrick standard parts are available free of charge, has accumulated around 3500 genetic parts since its foundation in 2003.

Contrary to Freeman Dyson's belief that individuals are replacing organizations, the organization of scientific work points in the direction of ever greater networked inter-connectedness. Enabled and driven by the data requirements of distributed research infrastructures, research organizations will differ from the ones we now have, but their importance will be enhanced, probably in ways that mimic organic superorganisms. The place of individuals will not be eclipsed by them, but the level of ambivalence that links them to organizations, as well as to what makes up an organization, is likely to grow as well.

As Steven Shapin's *The Scientific Life: A Moral History of a Late Modern Vocation* demonstrates, perhaps against the author's intention, there has never been a unidirectional flow in the evolution of the scientist as a moral agent. Shapin's moral history could be re-written as a history of—shifting—sociological ambivalence. It is not the personal virtue of the individual scientist that shapes the research carried out in a large pharmaceutical company, nor do scientists hold superior moral standards. Instead, with the boundaries between academic and commercial research becoming blurred and the advent of entrepreneurial science, new kinds of sociological ambivalence emerge. Rather than seeing in every manifestation of ambivalence immediately the 'dark side' of science or interpreting it as an inherent ethical deficit that calls for new ethical guidelines on how to translate expertise in the natural order into virtue in the moral order, we should first analyze ambivalence as what it still is: incompatible expectations and demands that arise from contemporary changes in the social structure of science.

5. Citizens' Science: What Follows

The rise of the scientist-entrepreneur is accompanied by its usually neglected correlate, the rise of citizens' science. This is a set of science-, technology-, and innovation-related practices that millions of people around the globe engage in. Some of it is clearly market-driven, as when commercial firms push consumers into using their products under the flag of preventive medicine or as a way to self-enhancement. Other practices arise around patients' groups and other civic manifestations of citizens to take greater responsibility over their life and how they want to live it. Citizens' science generates its own biosocial groups, when patients and their families mobilize around a shared disease, impairment or belonging to a specific group at risk.

In the ICT field, users of these technologies have become recognized as active producers of new knowledge and of available skills. Commercial firms that eagerly seek to enlist them recognize their contributions. New kinds of associations and networks arise that bind together openly acknowledged commercial interests—to obtain information through and about actual and potential customers, clients and patients that can otherwise not be assembled—with real or potential benefits to those who supply this information or simply their time and efforts. While we are made to believe that such arrangements are beneficial for both sides, the structural ambivalence persists.

The link to the scientist-entrepreneur is constituted through the market that binds them together. The medical diagnostics industry is a case in point. It is literally dependent on obtaining vital information and data, but also tissues, genes or other living material in order to build up its own reference base and to be able to standardize it. The increasingly popular DNA ancestry hunting is another case where a huge market of apparently "identity-hungry" consumers are voluntarily contributing their DNA in addition to the pay-check they have to send in. Ambivalence comes in when we are asked how to assess this phenomenon: we may dismiss it lightly as providing harmless entertainment and even project an educational value on it. But we may also worry about "Making up People" (Hacking 2006).

So, there is no lack of sociological ambivalence, even in citizens' science. While one set of norms calls for regulation of the new technologies and practices through anticipatory governance, especially when, as in the case of personal-genome tests and genetic diagnoses, the line between experts and lay people is further blurred. But any call for more regulation is likely to be countered by the demand for greater emancipatory freedom for citizens, as well as for greater respect for the choices they wish to make. Should there be more publicly licensed, collective spaces that would allow people to experiment with the experiences they make, be they voluntary or not? Or do we need above all new kinds of institutions that could compassionately accompany individuals after they have opted for one of the available treatments or none? Instead of rushing, too rapidly it seems, onto the quicksand terrain of an unproven ethics that must be so general as to fit all, should we not deepen the sociological analysis that would allow us to take a closer, empirical look at the root of the new ambivalences that surround us, carving out a space for individuals to define their own incompatibilities and looking for ways of reconciling them?

By observing the shift of the location of ambivalence from socially defined roles and their relocation into the midst of yet-to-be-defined, new structural assemblages and blurred institutional arrangements, some of the major challenges that science and democracy face have surfaced. Their future relationship embodies perhaps one of the most intractable ambivalences.

Re-evaluating the Place of Science in Evaluating Modernity

Gabriel Motzkin

Imagine that a spaceship were to land on earth sent by a superior civilization. The emissaries would descend, speaking of course perfect English, and would inform us that everything we thought we knew about science was wrong. We would immediately want to know whether this meant that the science we have is wrong, or whether science itself is a wrong way to go about interpreting reality.

We are quite comfortable with the idea that the science we have may be wrong, although this notion of the relativity of science has not been popular at all times since the scientific revolution. The shock of discovering that Newton's physics were not the ultimate paradigm for understanding the physical world, and the current unease stemming from the perception that we know nothing about most of the universe, the notorious dark energy and dark matter, have made us accustomed to the idea that a science may seem absolutely true at one time and invalid at another. We are comfortable with this notion of historical relativity because we also know that a revolution in scientific paradigm does not really invalidate the science of the previous epoch. Otherwise there would be no point to doing science, to using science as a way of trying to understand reality. We have to believe that something about what we learn from science is atemporally true, even if we are not always quite sure what it is. The question is what is meant by atemporal; whether a truth that is only true within one domain can be considered to be atemporally true. We will return to this question.

The broader question is whether or not science is a wrong way to go about interpreting reality. Suppose that the universe is really a container for gum balls, a set of toys for a very big kid who is outside our known universe. Our findings would still be correct for everything inside the container, but would they be correct for what is outside the container? And more to the point, if there are quite other laws outside the container, would they invalidate the laws that are inside the container? To the first question: we have no way of knowing what is outside the container, but we can distinguish what must be true in all possible worlds, and what must not. Generally, the laws of logic are assumed to be true in all possible worlds, but are there empirical laws that must be true in all possible worlds? If for example a universe such as ours can exist only under very severe constraints, would that be true of all possible universes? Or can one imagine a universe that is similar to ours but exists without these constraints?

I do not know the answer to that question, but I do have an answer to the second question, i.e., whether knowing what is outside the container would invalidate our findings for what is inside the container. The answer is: yes and no. Our findings would still be correct for the observations and the predictions that we have made, but we would understand them quite differently. We would understand them differently in two different senses: first, we would understand our physics differently, but we would also obtain a different picture of the world, and we would perforce have a different ontology and a different idea of how science can or does contribute to our civilization.

There is a paradox inherent in the scientific enterprise, which is one of the key drives animating the modern age: On the one hand, we assume, together with Kant, that there must be laws that once discovered would explain everything. On the other hand, the nature of our drive for knowledge is such that we will assume that there is another framework than the one we have. Thus we assume both the possible completeness of science as a system and we assume that it is impossible that our knowledge enterprise can discover that completeness.

I would like to argue that this paradox is a consequence of what, for want of a better term, could be called post-modernity. I do not refer by this obscure and over-used term to Derrida, Lyotard (remember him?), and their associates. I rather mean our idea of the significance of science since Darwin and Einstein. Previous to the changes that their theories signify, the basic paradox that confronted a philosophy of science was the Kantian paradox of having an idea of what a complete science would look like even while not having all the science in place (Kant 1979, 40–42 and 150–151). The question for such a philosopher of science was not one of the eventual completeness of science, but rather the question of how we can know something we do not know, i.e., our idea of a complete science. If, however, any science is permanently incomplete, then what do we make of the idea of completeness? That idea of completeness is then stripped of its ontological status, and the question is how and why should that idea of completeness serve as a regulative idea for the science in question? But if the idea of a complete science cannot serve as a regulative ideal for the science in question, then the status of the principles of that science becomes questionable, since the principles are no longer part of a coherent system in the same way.

I will not delve into incompleteness theories in logic or in physics, although I will return to physics, but I would like to tarry for a moment with the theory of evolution. Anyone who reads current work in biology will realize that the theory of evolution is a bad theory, but it is also true. Its truth is not some idea of the cosmos that will be invalidated by a different intuition of motion. Its truth is of the nature of the statement that Count Berchtold and his Ballhausplatz Hungarian coterie sought to humiliate Serbia in 1914, thus bringing about World War I (Tunstall 2003, 117–118). Its truth is the truth of a science that, like history, is contingent going forward, but is absolutely necessary seen backward. Moreover, we cannot predict, just as in history, the future course of evolution, but we know very well that evolution through natural selection and adaptation will continue to govern life just as we know that future human beings will have a history, although we have no idea what that history will be.

Like dark energy and dark matter, most of the facts in evolution will never be known. They have disappeared completely, and are now inaccessible. We triangulate to what we think might be an intermediate step in evolution that we then interpolate into the sequence. But because we do not have these intermediate steps, the actual sequence in evolution is notional.

However, there is another problem in the theory of evolution: right now no one is sure about the mechanism: is selection more important than adaptation? Does genetic drift play any role? Should one be considering epigenetic processes? Does it make sense to speak of levels of selection with different principles? And the biggest question of all: is biology like physics, or are there other principles at work in biology than in physics? All of these questions are subjects of hot dispute to the degree that one sometimes thinks, what is the science? Massimo Pigliucci and Jonathan Kaplan, after long statistical analyses that tear apart most of the specific theories, concludes that evolution is just like history, where we know that something happened, but we know very little about what happened (Pigliucci and Kaplan 2006, 153). Even with the historical record we possess a minute fraction of the data. The same is true for physics, but at least in physics we can do laboratory replication. In biology we also can do some laboratory replication, but not enough. For that reason, we cannot determine how things really happened. We can find out something in the lab about evolutionary constraints, but the historical contingencies at work in evolution are largely sealed off from us and can only be notionally reconstructed, much like attributions of historical causality.

This point is more important than it may appear. It is not just a question of empirical insufficiency. Until Darwin, people thought that they basically understood where they came from. It was a cardinal point for early modern theories of the autonomous subject that people did not think that our origins were unknown. Thus the Cartesio-Kantian self-constitution of the subject was in one sense a replication of the subjective level of the theological figure of the creation of the world. If there was anything that was created *ex nihilo* in the eternal Kantian cosmos, it was the spontaneity of the subject.

As Heidegger understood, Darwin made this idea of subjective spontaneity, of subjective self-constitution, ludicrous. The moment of subjective constitution turns out to be irrecoverable. Life is unique, anamnesis does not allow us to imagine our own constitution, and we must accept that we are here as a consequence of a historical process that we do not and cannot understand in the sense that we cannot replicate our own creation, which is a basic drive of the Idealist program. That means that we have no way of recapitulating the creation of the world in our own life. Perhaps the attempt to create artificial life should also be evaluated in this context. Thus we reach the conclusion that our autonomy is limited unless we somehow can reach beyond the conditions of our own life.

Yet when we consider the ways of doing that, then we reach the conundrum that we once again are looking for certain knowledge that we cannot test. The development of modern mathematics appeared to promise this result: that we could know truths for certain that have no relation to the world in which we live. Hence the idea of truth as meaning adequation to reality was abandoned. But if truth no longer is adequated to reality, then does reality need to observe the laws of truth? The mathematical conception of the world is one in which reality is a subset of truth. Some such instinct motivates the development of string theory, which posits universes that we not only cannot now observe, but which we probably will never be able to observe, since they require higher dimensionalities than the ones in which we can measure. We console ourselves by opining that we could measure the effects of actions in higher dimensionalities in our three-dimensional or four-dimensional world, but we really have no evidence for that conclusion at all. String theory is once again the attempt to posit a higher non-empirical reality that is mathematically structured and that makes our empirical world a subset of a mathematical world, one moreover in which quite other physical principles will be operative in other universes. The implication is then that even a complete knowledge of our physics will not be a complete knowledge of physics. We are left with the sense that the universe is sublime, sublime because it is, as Kant predicted, just like our minds, i.e., it withstands the link between mind and world because of its very large number of possibilities. But is that so different from the sense that we do not know where we came from, i.e., that most of the world is unknown? My point is that this was not at all the sense people had of the world from the seventeenth to the twentieth centuries, as the dominant belief was that while everything was not yet known, it could potentially be known.

Recently the Gulbenkian Foundation held a colloquium on whether science is nearing its limits (Steiner 2008). The answer of course is that science is not nearing its limits because there is so much that is potentially knowable that is not yet known. But the decisive question is whether there are laws of nature that affect us that are not knowable—and that can never be known. It is that limit which we have reached. That limit implies that the project of Enlightenment, according to which an infinite reason can comprehend a lawful reality that is complete, has failed. We have been arguing that this result has implications for science, since a science without potential completeness is not really a science; it is more like a technology, i.e., a useful tool for dealing with the world, and not a key to nature, except in the local universe in which we are located. This implication of physics is one of the reasons why so many are unhappy with untestable theories in science. However, the point about a mathematical conception of the universe is that it is both true and certain, and yet untestable and unfalsifiable.

If both physics, as the theory of nature, and biology, as the specification of what we can know about our origins, are shrouded in veils of absolute unknowability, what does that do to our evaluation of the place of science in evaluating modernity? I wish to be clear that I am not saying that the results of science or its theories are phantoms, but rather that it is an illusion to think that the place of science in culture or in the way we view the world is immutable, or even stable in the long run. We may regret this outcome, but it is historically certain that science is as fragile as is religion or ideology as a way of understanding truth. Looking to the future, what we now need is a better justification for science if we wish to retain it.

Yet the sense that our paradigm has shifted should also be affecting the way in which we view the past: if science is a relative phenomenon, we will evaluate that modernity we associate with science quite differently. How? Clearly, Kuhn's notion of paradigm shift was only possible after there had been a paradigm shift. But there have only been two master narratives in the history of science, the one that held from the seventeenth century to the early twentieth century, and the one since then. One can argue that the shift in the seventeenth century was also a paradigm shift, but it was so in a different way. Namely, in both the modern and the postmodern periods, the centrality of science to our civilizational enterprise has not been disputed, whereas people in previous eras would have been surprised to discover how centrally we emplace science in our culture. The early twentieth century paradigm shift has not altered this phenomenon: science is as central as it ever was. However, it may be that a change in the nature of this centrality has occurred. If science can no longer be relied upon to provide an ultimate explanation of reality, then it is no longer science in the same sense. Perhaps science is so central in our culture because of its exclusive claim to be the paramount technology rather than to provide an ultimate explanation of reality. No one today seriously thinks that technology can be anything but scientific, but there are many who doubt that science can provide an ultimate explanation of reality. In other words, science is so central in our culture because it makes an exclusive claim to tool-making. Since prehistoric times, tool-making has been the central and essential activity of human beings. Martin Heidegger understood this very well when he claimed that the industrial revolution occurred prior to the scientific revolution (Heidegger 1954, 22). In his own way, he was arguing that the essence of science is technology. My point is that such an argument was entirely appropriate for a view of science after Darwin and Einstein, but not for a pre-

Darwinian perspective, when science could still claim to provide ultimate explanations.

I do not wish to claim that this paradigm shift has invalidated science in any way. Some historians of science seem to think that science is inherently relativistic. From our point of view, science is as relativistic as the praxis of sharpening a stone to make it into an arrowhead. There are many technologies for performing this operation, but some are better than others. Moreover, while the advantage of some technologies may well be contextual, e.g., what kind of material you have available in your environment, it is not relativistic in the sense that it depends on what people feel about it at a given time. Within a frame of reference, some technologies are absolutely better than others. So it is with science.

However, the phenomenon of science becoming a technology has both metaphysical and sociological implications. It could imply that rationality is bounded, that outside of a closed, rational world, anything goes. I think it implies something much stronger, namely that the empirical world is a closed frame of reference, and that rationality far exceeds the limits of the empirical world. That is the hidden implication of the sublime quality of mathematics. Such a conclusion has consequences for our view of the empirical world and of rationality as well. With respect to the empirical world, it implies that it has no metaphysical significance whatsoever, since it is not infinite. It has no such metaphysical significance in a strong sense, i.e., it is not even anti-metaphysical in a positivist sense. The nature of the empirical world is such that no metaphysical or anti-metaphysical judgments can be inferred on the basis of theorems that predict what will happen in it.

But the point about our knowledge of that empirical world is that we apply rational principles in order to understand it sufficiently for our technological purposes, and those rational principles turn out to have cosmological significance when applied outside the limits of that empirical world. In other words, in order to make technological sense of our world, we have to assume that rational principles are at work in other universes. Thus, instead of scientific relativism as proposed in the wake of Kuhn, the real implication of our paradigm shift is non-empirical rational cosmological absolutism. The metaphysics that Kant attacked have thus been vindicated. If some statements are true in all possible worlds, but no mathematical system can ever be such as not to allow for an additional theorem, then while there can be no ultimate explanation for reality, any explanation that we provide can only be rational. The consequence of repositioning science as a technology is then the rebirth of rational metaphysics as an enframing activity.

This situation has both epistemological and sociological implications. From the point of view of a social epistemology, it implies that the activity of truth seeking is esoteric, and will never be available to most people—again, a stake in the heart of Enlightenment. The reason is that if the truth is not empirical, then it will also not be accessible to most people, neither as a technology, nor as something that they could penetrate with sufficient effort. The Enlightenment project was founded on the potential accessibility of truth for all, given enough effort. Even Gadamer founds the authority of the physicist on our potential understanding of his activity (Gadamer 1960, 427–428). It is that assumption that has become increasingly untenable, as some physics has come to seem increasingly like mathematics. However, this conclusion also implies that the link between some science and technology is an increasingly tense link.

Sociologically, this postmodern shift to a science that cannot provide ultimate explanations has had a telling effect on the activity of the knowledge elites. When we look back at the history of universities, the situation seems to be the following: universities grew to be central institutions in the middle and late nineteenth centuries as institutions engaged in the pursuit of knowledge for its own sake, with some technological spillovers. In the twentieth century, universities have increasingly become engines of technological innovation, most with their own profit-seeking subsidiaries. Despite their apparent ideologies, their social function has been to provide centers for technological innovation. The function of pursuing knowledge for its own sake has come to be a secondary function.

We all know this, but we have barely imagined what it means for the future. Let me take the example that I know well: the contemporary fate of the humanities, which are increasingly perceived as dysfunctional disciplines in contemporary university life, especially as universities have expanded their role to provide technological education for the masses. The answer is that they are dysfunctional; the humanities are dysfunctional disciplines within the context of technologically focused scientific universities. Nothing we do will make them more functional so long as we view the function of science as providing the technological essence of our cultural activity.

That does not mean that all future knowledge will only be technological. On the contrary. What I am arguing is that the humanities, and especially philosophy, have been replaced in our culture by esoteric mathematical explanations of possible universes. Inside the heart of the science faculties, a new philosophy is beginning to emerge, one that responds to the scientists' own quest for the sublime. Hence a university without the humanities will not be a university without the quest for knowledge for its own sake. Inside that university, the humanities will increasingly come to resemble paleontology, i.e., the scientific attempt to reconstruct evolution as the understanding of a remote, unknown, and dead past.

However, there is a still more curious possibility: if science itself will be divided between those who pursue technological explanations for empirical reality and those who pursue sublime explanations for whatever is outside that empirical reality, how will that affect the status of science? Can science survive without its claim to proffer ultimate explanations? Can an increasingly technologically oriented science enjoy sufficient long-term legitimation in our cultural context?

The Merton thesis brought together religion, social analysis, and the pursuit of knowledge. It assumed that both the social place and the larger world-view underpinnings of a science are what either stimulate or impede scientific development. It also emphasized institutional analysis, since late seventeenth century science took place both outside and inside universities. This institutional aspect is becoming ever more relevant in our time. After the twentieth century concentration of knowledge production inside universities, we are witnessing the exportation of part of knowledge production to institutions that are outside the universities, in part because of the expense and in part because of the decentralizing proliferation of knowledge. In turn, this phenomenon, together with the steady growth of knowledge elites as a proportion of the population, necessarily will imply downward status mobility for the knowledge elites, since their activities are becoming less rare. Thus the pursuit of technology will have the cost of being a relatively less rewarded activity as that knowledge becomes common. Think of computer programmers fifty years ago and now.

The final question is what the ideology or religion will be of a world with scientific universities in which some people will be engaged in pursuing the sublime, some will be technological innovators, and others, with low status, will excavate the past or teach. The normal situation for such a world should be one in which the empirically oriented knowledge elites should be rationalists and atheists, while those questing for the sublime should be mystics. I am less sure about the future religious beliefs of the technologically oriented elite, mainly because of human beings' unlimited capacity to hold mutually incompatible beliefs in different areas of life. However, the mysticism of non-empirical scientists, which was already known in the nineteenth century, is a subject worthy of reflection, since it is diametrically opposed to the spirit of Calvin, which was exoteric and anti-metaphysical. It was then all the more remarkable that some physicists, even then, were mystics.

Democracy and the Normative Structure of Science after Modernity

Yaron Ezrahi

The New York Academy of Sciences published in 1980 a festschrift for Robert K. Merton entitled *Science and Social Structure* (Gieryn 1980). The essay I contributed to this volume was on "Science and the Problem of Authority in Democracy". In a part of this essay I compared the normative structure of science implicit in the first comprehensive historical account of the Royal Society of London written by Bishop Thomas Sprat and published in 1667 with Robert K. Mertons' (RKM) essay on the norms of science [1938, 1942] later republished under several titles like "Science and Democratic Social Structure" and "The Normative Structure of Science" (Merton 1973). My thesis was that considering the profoundly different contexts and the great distance between the respective conditions and organizations of 17th and 20th century science there is still a striking continuity between the ethos of modern science as described by Robert Merton and that of early western science enunciated by Thomas Sprat. According to Merton, the principal norms of science as a social institution are universalism, communism (by which he meant shared ownership and responsibility for the knowledge produced by scientists), disinterestedness (the suspension of personal and subjective orientations) and "organized skepticism" (towards unwarranted beliefs). Whereas universalism, communism, and disinterestedness were elements of what I regarded as the constructive social dimension of science as an inclusive participatory cooperative enterprise engaging peoples from different social classes, religious affiliations and ethnic attachments, the norm of skepticism appeared to guide both the methodological orientation that links persuasion to evidence and the criticisms of nonscientific descriptions of, and beliefs about, the world. Many historians of modern science and democracy have considered the social structure and the subculture of science as an example of the possibility of voluntary consensus among rational individuals regardless of their group affiliations and therefore as a major inspiration for the Enlightenment ethos of modern democracy.

The question I pose in this essay is whether Merton's 1942 analysis of the normative structure of science and its relation to the normative structure and practices of democracy are still valid in describing the relations between the norms of science and democracy today. My tentative response to this question is that, perhaps not surprisingly, I discern in some respects greater discontinuities between the ethos and practice of science and its relations to democratic norms between 1942 and the present than between 1942 and 1667. There seems to be also vast evidence that these discontinuities have been affected by profound changes in the ideological, technological, economic and structural ecology of science since the last decades of the 20th century. Thus despite instances for the partial resiliency of the norms of universalism, communism, disinterestedness, and organized skepticism in the internal life of science, their perceived affinities to the norms of contemporary democracy seem largely transformed.

Take the norm of universalism. Its grip on social perceptions of scientific knowledge has declined due to strong cultural and political tendencies to particularize the technological, normative, and meaning import of bodies of scientific knowledge in different societies (Jasanof 2005). These processes of local contextualization of the scientific enterprise have obviously been associated with the declining relevance of a unified voice of the scientific community and the norm Merton called "communism" signifying scientists' collective ownership and responsibility for the knowledge they produced. Instead of the distinct voice and responsibility of the scientific community, Michael Gibbons, Helga Nowotny, and their colleagues have pointed to a process of "co-production of science and the social order" by scientists as well as by other social groups and institutions (Gibbons et. al. 1994; for further development of the idea of co-production see Jasanoff 2004). There are several nonscientific normative, economic and political "gate-keepers" that influence which disciplines and bodies of knowledge are cultivated, and which of the scientific assertions on the world will be admissible as facts for social currency. The interaction between scientists and these other social groups facilitates a continual adjustment between the production and diffusion of scientific knowledge and the social order. In their landmark book *Causality in the Law* professors Hart and Honoré observed that in the law evidence is a matter of both fact and policy (1973). I am suggesting here that even more so in the political and institutional context of public policy—as Larry Summers, the demoted president of Harvard University learned the hard way—notions of causality and reality are matters of both facts and policy. In order to acquire their status as facts in the social context, scientists' assertions must get at least tacit lay approval, although lay assertions about the world may enjoy the status of facts even when they contradict scientific ones.

In 1994 I have developed the concept of "civic epistemology" to indicate the complex, often informal perceptual and normative selection mechanisms that edit out what are unacceptable assertions of fact in the given socio-political context of public affairs. In an illuminating study, Professor Sheila Jasanoff has further developed the concept of civic epistemology and applied it to show how scientific assertions in the field of biotechnology are subject to deeply different normative and instrumental contextualizations in the respective public policy domains of Britain, Germany, and the United States (Jasanoff 2005). It is much more widely recognized today that in light of the manifest participation of technologists, industrialists, legislatures, ethicalists, and NGOs along scientists, it is no longer possible to clearly isolate their knowledge production process and regard it as subordinate to the norms of universalism and communism Robert Merton wrote about in 1942. No doubt, aspects of these normative codes are still preserved in some areas of basic research where interaction with national local social values, national culture, and politics may be considerably weaker. But universalism has become a more esoteric norm of scientific research more confined as a part of science to a subculture.

The norm of disinterestedness was conventionally regarded as a bridge between the neutral attitude of scientists to their findings, their obligation to accept the results of their research even when they run against their wishes, and the claimed neutrality of the liberal state when it sought to base its actions and legitimacy on scientific advice and instrumental rationality. On this normative basis it was possible to distinguish science from religion and ideology and use scientific knowledge and authority to criticize various popular beliefs. But the capacity of the scientific community to socially project itself as a guardian of the values of objectivity, neutrality, and rational detachment could not be sustained when science and technology have become more visibly engaged in controversial public policy decisions and programs (Jasanoff

2005).[1] Inasmuch as the lay public judges the experts' advice and authority on the basis of their perceived consequences rather than their theoretical validity, the fact that any application of scientific knowledge in public affairs involves redistribution of costs and benefits among competing interest groups means that scientific disinterestedness and neutrality have inevitably become suspect. This process, which was augmented as the voice of science, appeared increasingly on both sides of the same issues often indistinguishable from the voices of politics and business.

With regard to the norm of "organized skepticism," I think Merton was referring first to skepticism as an integral internal element in the practice of science, a methodological doubt as a scientific virtue in the context of research and theory building. In the wider social context, organized scientific skepticism appeared to connote a liberal-democratic cultural norm underlying distrust in religion, ideology and other authorities that resist support by evidence admissible by empirical science. The Enlightenment vision of the partnership between science and democracy was based on the premise that guided democrats from Jefferson to Dewey that those government policies and programs that are based on scientific knowledge are more transparent and trustworthy. Whether governments actually enlisted both the knowledge and authority of science to shape their decisions or just the authority of science to legitimate them, the seal of science enjoyed the status of a valuable political resource in many democratic countries. I would like to suggest that this has changed significantly since the later decades of the twentieth century due to shifts of both democratic political culture and theoretical perspective of sociological theorists and historians of science. Following many decades during which the electronic *agora* created by the mass communications revolution has become the principle stage of political personalities and events, the lay public has lost much of its earlier expectations that democratic governments would actually become more transparent and that the layperson could more easily distinguish facts from fictions, substantive decisions from mere gestures. Corresponding to the transformation in democratic political culture induced by these developments has been a growing theoretical understanding that not only trust but also distrust is an integral compo-

[1] For an early treatment of the consequences of scientific controversies in the social sciences see Frankel 1976.

nent of modern democratic political culture countering excessive lay trust in government, trust often buttressed by political rituals and the theatrics rather than the substance of decisions and programs. If internally "organized skepticism" is still inculcated as a virtuous methodological norm, what is prevalent in the extra scientific sites of politics and mass communication is an undisciplined or "unorganized skepticism" rarely allayed by scientific evidence and authority.

I am arguing that added together, these discernible changes in the norms socially projected by science as specified by Merton, norms that have profoundly influenced science's relations to society and politics in modern advanced democracies, seem to have significantly devalued the uses of contemporary science as a political and policy resource.

Particularism and diversity are being associated with science (and technology) no less than universalism: socially inclusive and heterogeneous co-production of knowledge and the social order is more widely acknowledged than the more exclusive "ownership" and responsibility of the scientific community to the products of science; furthermore, honest partisanship is more expected of scientists involved in relating knowledge to society and public affairs than supposed disinterestedness and objectivity, while unorganized skepticism, which coexists with and balances excessive trust in government, seems to govern attitudes to both scientific and political authorities in many areas.

Shifting our perspective from the norms that govern scientific and political practices to the deeper socio-cultural ontological and epistemological frames of experience one can discern marked changes in orientations towards reality, agency and ethics. In advanced modern democracies the belief in the public and uniform nature of reality has been fundamental to the expectation that scientific knowledge can discern the areas and means of human freedom of action within external objective constraints imposed by nature. The world as an object that governs by objective regularities and unpredictable contingencies has been regarded as an unavoidable and rarely resistible constraint on human actions and political ambition. Hence, any break in the dichotomy between concepts of nature or reality and human agency could easily shake up long lasting modern presuppositions of democratic institutions. This dichotomy has supported the apolitical and impersonal authority of scientists as representatives of the necessary external possibilities and constraints faced by human agencies. It has been very much due to faith in the universalism of science; the disinterestedness of scientists and the power of empirical evidence and rational arguments to overcome myth, uncertainty, and skepticism that made science central to the democratic concept of instrumental and transparent public action (Ezrahi 1990). But this sharp separation between facts and opinions or, more fundamentally, between data and concepts, has become increasingly unsustainable after modernity.

Robert Merton was, of course, too sophisticated and knowledgeable a thinker to accept the dichotomy between given empirical data and theory as more than a part of the ethos of science in democracy. In his essay on "Sorokin's Formulations in the Sociology of Science" (1936, 165–6) he wrote: "any sharp separation of reason and empirical data in contemporary science must [...] distort much of the operative reality. Work in the scientific laboratory rests on both" (Merton 1979). This statement could be the motto of a growing trend over the last decades, which challenges the very foundations of the separation between empirical data and concepts, facts and theories. Following his discussion of the interconnections between sensual perceptions and concepts of the world, the Harvard philosopher Nelson Goodman insisted that facts are but "small theories" (Goodman 1978, 91–107). In his instructive study of the relations between photography and the history of theories of perception Jonathan Crary has noted the process by which mechanistic theories of camera-like eye centered theories of perception, were replaced by physiological optics: the idea that vision is much more actively shaped by brain and culture, learning and conditioning (Crary 2001). From a philosophical perspective John McDowell followed Wilfrid Sellars in debunking what he called "the myth of the given" by which he means that no empirical preconceptual experiences unembedded in concepts are possible since concepts are guides and organizers of our sensual experiences. "Experiences" says McDowell "already have conceptual content" (McDowell 1994). This unsettling of the myth of the given is not confined only to academic circles. There are clear signs of its penetration to popular culture as well. Samuel Weber has observed that in the age of rapidly expanding media "the world of sense perception is increasingly uncanny" (Rickels 2001, 95). In the same vein Jenny Slateman has suggested that television has obscured the apparently clear cut distinction between faith and seeing, a distinction that has thoroughly dominated our culture" (Ibid., 216). In the Jerusalem production of Othello some time ago Jago produces the false evidence of Desdemona's betrayal by means of a Camera whose capacity to produce deceptive yet persuasive reality effects has been repeatedly demonstrated in the courts and cinema theaters. The idea that seeing is the safest way to factual reality indeed has dominated our culture—mostly since the 17th century although it has never been really defensible. As Clifford Geertz puts it, “the real is just as imagined as the imaginary” (Geertz 1980, 136). Continually exposed to the perceptual illusions inherent in contemporary visual culture lay publics have no doubt lost much of their earlier confidence in their capacity to distinguish facts from fictions. In such a context, scientists’ authority to represent the regularities and structure of reality, of the external world becomes more fragile. Not surprisingly, against this background the sociologist of religion José Casanova discerns the return of genres of religious symbolism to the public sphere (Casanova 2007). This trend is not exactly the return of religion as it is the reemergence of beliefs about causality and reality that are more audaciously unaccountable to material standards of evidence and to reason as they were understood and cultivated by Enlightenment culture. It has shaken the materialization of political actions in the visible space of modern society as means-ends instrumental relations that enable public accountability and participation. Obviously the repenetration of genres of religious and metaphysical beliefs into the public sphere of politics is bound to shake the relevance of, and trust in, forms of transparency and accountability which have served for so long as building blocks of democratic authority and politics.

With respect to transformation of orientations towards social imaginaries and concepts of agency, I would only want to note the decline of the former hegemonic model of the rational man and the influence of its various versions in economics, game theory, politics, choice theories, ethics and the law. Whereas rational man models have remained valuable in professional discourses on normative theories of behavior and in training advanced students in the above fields, their value in predicting behavior and accounting for the structure and dynamics of large behavioral systems and networks of human interaction has been increasingly questioned. The fall of the financial markets in September 2008 constituted a further blow to naïve lay universal overconfidence in economic models and in the power of mathematical economics to warrant the confidence of more expert market players. This development has been accompanied by the boost to the sub-field of behavioral economics which integrate psychological, organizational, sociological and other factors in the analysis, prediction and explanation of human decisions, gambling and interaction (Tversky and Kahneman 1981; Ariely 2009; Gigerenzer 2008).

Another aspect of these developments is the upsurge of studies in the history and politics of emotions as a subfield in the humanities and the social sciences. The interest in factoring in the behavioral import of emotions in various spheres of society and politics represents the emergence of a wider and richer conception of human behavior less congenial for the application of rational models of behavior that have formerly appeared so conducive to the partnership of science and democracy (Walzer 2004).

No less significant have been the perceived changes in the social and philosophical perceptions of ethics. One way of describing it is the replacement of what can be called moral holism (the notion that values can be harmonized to form a whole that enables a clear hierarchical order) by moral or normative perspectivism, the notion that the multiplicity of particular moral perspectives in society does not enable a legitimate and unambiguous collective ordering of values. The idea of moral holism has underlaid for long concepts of the public good and public interest. They were formerly inspired by notions of cosmic harmony that were central in ancient Greek and Christian cultures (Spitzer 1963), and transferred into images of moral social order in modern time. This affinity between nature and society greatly facilitated the power of science, as the inquiry into the regularities of nature as a source of extra-political authority in guiding human action. As the representation of trancehuman order, scientific knowledge had at first the authority to guide behavior and justify action without entering the problematic territory of moral-political choices. Knowledge-based consensus, freedom, and democracy appeared compatible. It is precisely the faith in this normative harmony or moral holism that broke down with the end of the Enlightenment program and the rise of moral perspectivism after modernity. As contemporary theorists have been observing, since the last decades of the 20th century mass migrations and globalization are deeply transforming the sociopolitical context of knowledge and democracy diminishing both the reality and visions of homogenous societies. In his essay, "The world in pieces," Clifford Geertz noted that already at the end of the 20th century "a much more pluralistic pattern of relationships among the world's peoples seems to be emerging but its form remains vague and irregular, scrappy ominously indeterminate" (Geertz 2000, 218–263). Lacking the former impulse of integration, the current types of much more formless social configurations appear in Geertz's language as manifesting a kind of "disassembly" of parts and fragments resisting the totalizing concepts with which we were accustomed to organize our social and political worlds. Such central concepts as nation and culture, observes Geertz, which guided us to perceive as systemic wholes large human groups, have lost much of their grip.

These transformations of orientations towards reality, agency and the moral dimension of public affairs obviously indicate a turning point in the relations between science and the sociopolitical order. But it is far from clear what are the contours of the new configurations that are now emerging. In my opinion, the cracks in the ontological vision of knowledge and reason-based democracy do not necessarily lead to chaos. In relatively free societies human groups still can make, if not rational at least intuitive choices of the imaginaries of order, power, and authority that appear to them to approximate their preferences. Obviously, such choices are not usually manifest in dramatic moments of collective decisions in the *agora* of the democratic society but emerge from multiple decisions made over social and historical times by a multitude of individual and group actors including leaders, parties, popular writers and artists. In the West, at least, democracy, lacking the convenience of resting on the ontological moral holism and the certainties of rational knowledge envisioned by Enlightenment thinkers, can still settle on the less firm basis of a more reflexive-intuitive understanding of the historical role of moral choices that select metaphors and imaginaries of order and authority that, when reified in the political sphere, promote human cooperation rather than conflict.

The Matthew Effect Writ Large and Larger: A Study in Sociological Semantics

Harriet Zuckerman

Peer recognition matters in science—to those who receive it, to those who give it and to the system as a whole. Robert Merton's[1] early research persuaded him that scientists' pursuit of recognition from knowledgeable peers shaped their work and the larger scientific enterprise.[2] In all its complexities, peer recognition and its role in science remained a central focus of his research until the end. Not the product of mere vanity, the pursuit of recognition is built into the social organization of science. Even those who are modest by disposition are institutionally compelled to seek it. Receiving recognition assures scientists that their efforts have been worthwhile. It provides them with incentives to move forward, improves their research opportunities and enhances their prestige. All other rewards, as Merton crisply put it, "flow from it" (Merton R. K. 1995, 381). It is also consequential for the institution of science. The quest for recognition focuses collective attention on important problems. It quickens the pace of scientific activity and speeds the advancement of knowledge—or if one prefers—it promotes scientific change. It is not however an unalloyed good. It encourages scientists to make unwarranted claims for credit, to engage in unproductive priority conflicts, to exhibit unbridled competitiveness, and in some instances to commit fraud and plagiarism. Scientists believe that the extent of recognition meted out should accord with the extent of

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1 Readers will note that I refer to Robert Merton with proper academic distance. I do so despite having spent 43 years simultaneously and consecutively as his collaborator, partner and wife.

2 Merton earmarked priority in scientific discovery or the importance of "being first," and thus being credited for it, as sociologically significant in his doctoral dissertation published in 1938. At the time, he vowed to discuss it further at some later date. A full-scale discussion of priority did not appear until 1957 when he returned to it in his presidential address to the American Sociological Association (R. K. Merton 1957a).

contributions made. Recognition should go to those who deserve it and in the amount that is warranted. But scientists are not naïve; they do not think that the ideal and the real always coincide. "Too much" or "too little" recognition can be awarded or it can go entirely to those who do not merit it at all. This evokes indignation and envy; it increases contention and it undermines the system by which intellectual property is secured.

Although the pursuit of recognition is deeply embedded in science, Merton observed that many scientists are ambivalent about seeking it (Merton R. K. 1963). They want it but denigrate it. They are aggrieved when they think their work is unappreciated but trivialize its importance. For Merton, however, peer recognition was not at all trivial sociologically. How it is allocated and misallocated tell much about the organization of science and serve as strategic research sites for understanding the workings of its evaluation- and reward-systems and why their outcomes are so important (Merton R. K. 1987).

The "Matthew Effect," identifies conspicuous instances of the *mis*-allocation of recognition, that is, of giving recognition to those who have not earned it and failing to give it to those who have.[3] The Matthew Effect, as Merton put it in his canonical paper of 1968, "consist[s] of the accruing of greater amounts of recognition for particular scientific contributions to scientists of considerable repute and the withholding of such recognition from scientists who have not yet made their mark" (Merton R. K. 1968 in 1973, 446).[4] It was, he wrote, close to inevitable

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[3] Few scientists would reject the claim that recognition *should* accord with the extent of contributions to knowledge; this is so however skeptical they are about practice conforming to principle. It is a bit ironic that some insist that Merton believed that scientists resolutely conformed to the "norms of science" when so much of his work focused on violations of them.

[4] It may be that the Matthew Effect is itself a misattribution. "There is good reason to think that St. Matthew was not the author of the 'stately phrase' despite our having come to attribute it to him." Rather, Merton wrote that some scholars of religion have claimed that Matthew was in fact quoting Jesus and thus the Matthew Effect might be more correctly labeled the Jesus Effect (R. K. Merton 1988, fn. 7, 609). Since I have no competence to judge the validity of this claim I am not ready to re-label the Matthew Effect. The history of the naming of the Matthew Effect confirms Stephen Stigler's not un-serious proposal of "Stigler's Law of Eponymy" which holds, in its simplest form, that "No scientific discovery is named after its original discoverer" (Stigler 1993, 147–57).

that it would be called the Matthew Effect, following the Gospel of St Matthew: "For unto everyone that hath shall be given and he shall have abundance; but from he that hath not shall be taken away even that which he hath."

This paper is divided into three parts: the first reviews the principal conceptual elements of the Matthew Effect and of Merton's kindred idea, "cumulative advantage" (Merton R. K. 1987, 25). The second lays out the reasons why I chose to write about the Matthew Effect and assess its current use in response to the editor's call for papers that take up Mertonian concepts holding special interest for their authors. The last part takes up a quartet of questions about the status of the Matthew Effect as concept-and-term viewed from the vantage point of 2010 and the perspective of "sociological semantics." [5]

- The first question is foreordained. To what extent has the Matthew Effect been used by scientists and scholars since its publication? If it is still used, is it still seen as Merton's work or has his contribution been lost through the process he described as "Obliteration by Incorporation" or more precisely, "the Obliteration (of source of Ideas, Methods, or Findings) by Incorporation (in Canonical Knowledge)?" A common pattern in the sciences, OBI describes the incorporation of the substance of contributions into the body of knowledge coupled with the disappearance of the names of their contributors. The result in part of the exhaustion of potential of certain ideas once their implications are probed coupled with the contributors' of the most important work becoming "household words," and scientists' reluctance to cite household words, as Joshua Lederberg put it, the names of most contributors ultimately disappear and are never learned by later generations (Merton R. K. 1987, 25). [6]

[5] Concisely defined, sociological semantics "examines the ways in which [...] words acquire new meanings as [they] diffuse through different social collectivities" (Merton R. K. and Barber E. 2004, x., 9–10, 230–298).

[6] As he wrote in 1987, Merton intended a full-scale analysis of OBI (another case of his interest in social processes leading to the mis-allocation credit) but never got to it. He first treated the phenomenon in "On the History and Systematics of Sociological Theory" (R. K. Merton 1968a, 28, 35, 38).

- To what extent has the Matthew Effect, like many other Mertonian terms-and-concepts, found its way into popular discourse? How much is it used in the vast array of sources captured on the Internet? How does its diffusion compare to other Mertonian contributions?
- How pervasive in social life are Matthew Effects? What kinds of phenomena are claimed to exhibit them? What implications has the process of generalizing the Matthew Effect had for the meaning of the term-and-concept?
- And last, what do "the travels and adventures of the Matthew Effect" reveal about the fate of ideas and their accompanying terms as they spread over the course of time?

1. The Matthew Effect and Cumulative Advantage

This brief conceptual gloss reviews the fundamentals of the Matthew Effect and the kindred Mertonian term-and-concept, "cumulative advantage." The two are intimately linked. More precisely, the Matthew Effect is a special case of cumulative advantage; it is one of several processes that contribute to the "rich becoming richer" while the "poor" become poorer or fail to benefit at the same rates as the rich.[7] Cumulative advantage is also germane to our story because, in some quarters, it has become synonymous with the Matthew Effect and thus has played a central role in its evolving use. The two concepts though related are decidedly different. The central idea captured in the Matthew Effect, as Merton noted, is that scientists are inclined, both wittingly and unwittingly, to award more recognition to well-known scientists and less to others for contributions of the *same* or *equivalent* importance, while cumulative advantage refers to the repeated advantaging of those who are already advantaged and thus to increasing disparities in opportunities, performance, and rewards. It includes no assumptions about whether such advantages go disproportionately often to those who deserve them.

[7] As early as 1942, Merton was writing that the stratification of science involves "the accumulation of differential advantages for certain segments of the population" (Merton R. K. 1942 reprinted as "The Normative Structure of Science" in Merton R. K. 1973, 273).

Matthew Effects appear most clearly in the allocation of credit among collaborators and among those responsible for multiple independent discoveries. In both instances, they occur when greater credit is given to well-known scientists and withheld from those who are less well known for what are essentially equal contributions.[8] Both classes of events provide severe tests for the presence of Matthew Effects. But in practice, the presence of Matthew Effects is harder to identify than it might seem. Weighing collaborators' contributions is no easy matter, especially when each has worked on all aspects of the research or when their skills differ but each is necessary to getting the work done.[9] Gauging the equivalence of multiple independent discoveries presents its own difficulties. Such discoveries can be "equal" in a number of ways. Their implications for science can be functionally equivalent but not precisely the same. If their implications are the same, they might not have been executed in the same way, or the interpretations their contributors give them may differ. And of course, the requirement of simultaneity introduces its own complexities. Even when those involved did not know of the other's work, how close in time do discoveries have to have been made to declare them to have occurred simultaneously? And most important for determining whether the Matthew Effect has been at work, the reputations of the participants in multiple discoveries have to be different enough to claim that the more famous among them has gotten the lion's share of the credit. Thus identifying instances in which the Matthew Effect has occurred, that is, which satisfy the necessary criterion of equivalence of contribution but difference in status of the investigators, has required researchers who wish to study them to be unusually ingenious.[10]

[8] Multiple independent discoveries refer to discoveries that are the same or equivalents of one another but are made independently by two or more scientists at approximately the same time. These occur far more often than it might appear at first blush. Such discoveries have served as strategic research sites for Merton—not only for understanding the allocation of recognition, as they do in this instance, but also in his studies of the development of scientific knowledge (Merton R. K. 1961 and 1957a).

[9] The ordering of authors' names on scientific papers serves as a device for conveying their relative contributions in some but not all scientific disciplines (Zuckerman 1968).

[10] See Stephen Cole's study of the Matthew Effect, which demonstrates both the ingenuity required and the difficulties of identifying the Matthew Effect empirically (Cole 1970).

Merton published two papers on the Matthew Effect; the first, which appeared in 1968, identifies in detail the social and psychological mechanisms that produce Matthew Effects. In a communications system flooded with papers (and now also with electronic materials), scientists must find ways of deciding what to read and what to pass by. Papers by well-known authors are more visible. They are also more likely to be read because readers assume that papers by well-known authors are likely to be worth the investment of their reading time and thus they are read more intensively than others. Such papers also tend to be given more credence because of their authors' track records of credibility. Such decisions are more or less rational accommodations readers make in dealing with a communications system that prevents anyone from getting full command of the literature and having direct knowledge of actual contributions. However, such rational decisions have the inevitable outcome of producing some measure of error in allocating credit; it promotes over-crediting, relative to their contributions, of better known authors and the under-crediting, relative to their contributions, of less well-known ones.

The misallocations of credit bound up in the Matthew Effect are unintended consequences of impersonal social and psychological mechanisms that are part-and-parcel of an over-crowded communications system and a reward system that favors recognizing those who have already been recognized. Matthew Effects need not result from systematic efforts to deprive scientists of their due though this of course happens in some fraction of cases. Nor should the awarding of greater credit to well-known scientists necessarily signal the presence of a Matthew Effect. After all, already established scientists may well have been the prime movers in the given investigation and merit the greater recognition they receive.

Those familiar with Merton's work will recognize that the Matthew Effect and the mechanisms that produce them are a classic example of middle-range theorizing. As Peter Hedström and Lars Udéhn observe, middle-range theories, including the Matthew Effect, explain well-defined and repetitive social phenomena by reference to the operation of well-defined and repetitive mechanisms and these define their consequences with precision. In the case of the Matthew Effect, unintended and ironic consequences are produced by adopting reasonable procedures for dealing with large and growing scientific literatures in ways consistent with customary scientific practice.[11]

Mentioned as early as 1942, the idea of cumulative advantage remained an undeveloped "proto-concept" as Merton himself put it 46 years later (Merton R. K. 1988, 610).[12] As he then went on to explain, advantage accumulates when:

> Social processes through which various kinds of opportunities for scientific inquiry as well as the subsequent symbolic and material rewards for the results of that inquiry tend to accumulate for individual practitioners of science, as they do also for organizations engaged in scientific work. The concept of cumulative advantage directs our attention to the ways in which initial comparative advantages of trained capacity, structural location, and available resources make for successive increments of advantage such that the gaps between the haves and have-nots in science (as in other domains of social life) widen until dampened by countervailing process. (Ibid. 606)

Cumulative advantage and its obverse, cumulative disadvantage, are outcomes of repetitive processes that occur over the course of time, and produce increasing disparities among individuals and organizations in the opportunities they acquire, in their achievements (and failures) and in the rewards they receive or the penalties they suffer (Zuckerman 1998, 146). For individuals, the accruing of advantages (and disadvantages) often starts early in life. Some individuals are identified as being especially promising while others are not. In processes of cumulative advantage, precocity is immensely valuable, especially when competitions are confined to those in the same age cohort. Having more opportunities and thus better chances of achieve-

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[11] Peter Hedström and Lars Udéhn in "Analytical Sociology and Theories of the Middle Range" earmark the Matthew Effect as the model of middle-range theorizing since it seeks to explain phenomena which appear in a variety of social domains and does so by attempting to tell "not the whole causal story but the heart of it" (Bearman and Hedström 2009, 20).

[12] Merton goes on to note, in the first Matthew Effect paper, that as a "proto-concept," cumulative advantage was "inert, unnoticed and unexplicated until it was taken up almost a quarter century later […]" despite its much earlier appearance. Put another way, cumulative advantage did not become a "central message" in Merton's work until 1988. On the "central message," see Don Patinkin (1983).

ment, those who are advantaged often do better than others and thereby confirm the initial judgment that they were superior at the outset. This sets in motion successive rounds of their being granted more opportunity, their achieving more, which in turn brings them more rewards and more opportunity for achievement. Processes of cumulative advantage result in large disparities in achievement and rewards and a stratification system in which advantage is highly concentrated.[13]

Cumulative advantage is a powerful idea and has been the subject of considerable empirical research. In their review of the relevant literature Thomas DiPrete and Gregory Eirich show that these processes appear in many segments of social life and help shape systems of social stratification (DiPrete and Eirich 2006, 271). They lay out the two main forms cumulative advantage assumes and probe the implications of positive feedback loops and "path dependence," that is, the impact of prior states on current standing (ibid. and Rigney 2010, ch. 1). Their main interest is in the contribution cumulative advantage makes to the production of inequality across time "in which a favorable relative position becomes a resource that produces further relative gains" and how such processes work in "education, careers, and related life course processes." While they observe that the Matthew Effect involves the advantaging of well-regarded scientists and note that it is a consequence of scientists' inability to judge the great mass of work that is published, they do not give it sustained attention since their principal interest lies in more general processes of social stratification. Some years ago, I noted that:

> The Matthew Effect amplifies processes of accumulation of advantage. It heightens the extent of inequality in scientists' standing and the influence of their work. In the case of accumulative advantage [sic] and disadvantage, inequalities derive, in part, from actual differences in the extent of contribution; differences which make the reward system appear to be effective and just. In the special case of the Matthew Effect, however, such differences derive mainly from judgments scientists make which are shaped by prior experience and features of the stratification and communication systems of science, both being unrelated to reliable information about the extent and quality of contribu-

[13] This abstract account of the workings of cumulative advantage fails to capture the fact that obstacles arise even in the careers of those who have benefited markedly from it, as my research on the careers of Nobel laureates demonstrates (Zuckerman 1977).

> tions of the various scientists involved in these conjoint events. These constitute consequential departures from […] principle that rewards should accord with the extent of scientists' contributions. (Zuckerman 1998, 146.)

Today I would emphasize even more than I did then the difference in the nature of outcomes of the Matthew Effect and cumulative advantage. The former, by definition, results from the misallocation of rewards while in the latter, this needs not be so. Indeed, through time, those benefiting from processes of cumulative advantage may in fact merit the benefits they have received. The problem however, is that it is impossible to determine, based on subsequent disparities in achievement, whether the beneficiaries of cumulative advantage deserved to receive more opportunity and resources at the outset than others, or whether privilege or chance led them to benefit without justification.[14]

As I noted earlier and will note again, the Matthew Effect and cumulative advantage are becoming conceptually homogenized as time passes; both are increasingly taken as ways of conveying that the rich become ever richer while the poor become ever poorer or fail to keep pace with the rich in relative terms. In some instances, the terms have been used interchangeably and in others, the Matthew Effect has come to cover both. This appears to be the case in both academic usage and in the vernacular language.

2. Why the Matthew Effect?

Choosing the Matthew Effect as the centerpiece for this paper was, as the psychologists would say, the result of over-determination. Its influence on my work is all too obvious. First, its birth and its later devel-

[14] Cumulative advantage can benefit both those who merit it and those who do not. The advantages accruing to those with inherited wealth often fail to meet the test of merit. In such cases, as I have noted, differences in performance between the "haves" and "have nots" are minimized and so too is the accumulation of advantage in large measure because the "haves" are not as effective in using the advantages they acquire as they are when the criterion of merit affects the allocation of advantage. This difference in allocation produces differing rates of cumulation. When opportunities and advantages go to those who fail to merit them, cumulation tends to be "additive" in contrast to much more rapid or "multiplicative" cumulation which occurs when those in a position to use opportunity and resources benefit from receiving them. (Zuckerman 1977, 60–61)

opment were inspired by interviews I did with Nobel laureates as part of my dissertation research. Second, the Matthew Effect is a central theme of *Scientific Elite,* my first book. It is also a central theme of papers I have written since, on social stratification in science, men and women scientists, and cumulative advantage. But this is not all. The Matthew Effect was an inevitable choice for a quite personal reason. Those who have read Merton's first Matthew Effect paper, not its original version in the journal *Science*, but in later editions, including the one reprinted in the 1973 collection of Merton's papers, *The Sociology of Science*, or the follow-up paper, The Matthew Effect II, probably did not notice the fact that these later publications contain footnotes that the original did not. The wording of these footnotes differs slightly but the message concerning the authorship of the original paper is the same. The one in Matthew II reads:

> It is now belatedly evident to me [Merton] that I drew upon the interview and other materials of the Zuckerman study to such an extent that, clearly, the paper should have appeared under joint authorship. A sufficient sense of distributive [...] justice requires one to recognize, however belatedly, that to write a scientific or scholarly paper is not necessarily sufficient grounds for designating oneself as its sole author. (Merton R. K. 1988, fn. 2, 607 and Merton R. K. 1968 in 1973, fn. 1, 439)

Since then, his citations to the paper contained my name as co-author and I appear as such in the comprehensive bibliography of his work compiled by Maritza V. Poros and Elizabeth Needham (2004).

I do not know how many authorial confessions have appeared in print regarding the aptness of authorship decisions. I suspect they are rare, very rare. The truth is that I never claimed (either in private or in public) that Merton's sole authorship of the Matthew paper was unjust. I never asked that it be revised. I am as convinced now, as I was then, that the really fundamental ideas the paper presents are his.

Of course, I recognized that the laureates I interviewed noted time and again that there was a tendency in science for the "rich to get richer," not necessarily in material terms (though this of course can also be true) but with respect to peer recognition and influence in the scientific community. I did not miss the significance of the observations the laureates made about their receiving more credit than they perhaps deserved or when they were young being deprived of credit when they perhaps would have merited it. Nor did I fail to note their observations about the justices and injustices the reward-system in science can produce. I clearly remember one saying "The [scientific] world is peculiar in this matter of how it gives credit. It tends to give credit to [already] famous people." Indeed, in response to this comment and to other similar ones, I questioned them insistently about how credit was allocated in specific instances in their own work and in the work of others.

Had I been asked in 1968 when the Matthew Effect paper was published if it had depended on work I had done, I would have answered it had. The interview data I collected stimulated Merton's thinking and provided necessary evidence for his analysis. But I am sure that they were not sufficient. The phenomenon did not become the Matthew Effect until Merton made it so. All apart from the thought-provoking label he gave it (a reflection of his easy familiarity with biblical texts), his earmarking the pattern as a misallocation of credit, his identifying the prime cases in which the effect was most readily observed and his laying out the mechanisms the Matthew Effect entailed, made the paper his work. Owing to my research on scientific authorship and the norms that govern it (Zuckerman 1968), I know that no firm and universally accepted standards exist for assigning authorship. Some hold that authorship should go only to those who write papers. Others are persuaded that all those who make substantial contributions are entitled to authorship while still others are convinced that all individuals responsible for any and all aspects of the research, including securing the necessary funds, merit authorship. Furthermore, my research showed that authorship norms have changed through time. We are now in a period in which authorship is more often shared than it was four decades ago. Authorship now plays a more important role in the careers of the young than it did in a time when jobs and research support were meted out through "the old boy network." So too, many are skeptical about how finely contributions can be parsed. Perhaps the once common pattern of seniors claiming credit for joint work is less acceptable than it once was, and perhaps it is now more widely understood that well-known scientists get the lion's share of the credit anyway and that giving authorship to juniors is relatively cost-free. Whatever the reasons for these changes, at the time, I did not feel that I had been deprived by not being an author of the Matthew Effect, nor do I feel so now (Rossiter 1993).[15]

The text of the provocative footnote Merton added to later editions of "The Matthew Effect" did not go unnoticed. Margaret Rossiter, a pioneer historian of women in science, noted it and proposed a kindred phenomenon, "the Matilda Effect," or the pervasive minimizing or neglect of women's scientific contributions. She went on to indicate how costly the outcomes of the Matthew-Matilda Effect have been for women scientists.[16] Since the careers of American men and women scientists later became a focus of my research, the Matilda Effect, all apart from my personal involvement in its origins, holds more than a little personal interest (Zuckerman, Cole, and Bruer eds. 1991).[17]

Even if these reasons were insufficient, my revisiting the Matthew Effect became inevitable when I learned that Yehuda Elkana and Bjorn Wittröck had concluded that Robert Merton may be disappearing from sociological discourse, based on the relatively infrequent mention of his name but the frequent use of his ideas in papers presented at the 38th World Conference at the International Institute of Sociology in 2008. Quite naturally, they proposed that Merton himself had become the subject of what he described as Obliteration by Incorporation or OBI. OBI, as I noted earlier, consists in "the Obliteration of source of Ideas, Methods, or Findings by Incorporation (in Canonical Knowledge)" (Merton R. K. 1987, fn. 3, 25). In due course, I will examine instances of OBI and misattributions in the case of the Matthew Effect.

The Elkana–Wittröck conjecture no doubt correctly describes the

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[15] Would joint authorship have made any difference in my own "credit rating"? Probably not in the short run if the Matthew Effect is a valid depiction of the allocation of credit among senior and junior authors. Robert Merton was famous at the time and would very likely have been credited with the paper any way. Perhaps later, when I did work on my own, some measure of credit might have come but there are no guarantees of retrospective recognition.

[16] The Matilda to whom Rossiter refers was not a scientist but an American suffragist and freethinker, Matilda J. Gage.

[17] In the lead essay of this volume, "The Careers of Men and Women Scientists: A Review of Current Research" (27–56 and 45–46), I review some available evidence on the Matilda Effect, the frequency with which women's publications are cited relative to those of men. More recent data indicate that on average women are cited more per publication than men (Holton and Sonnert 1995, 12).

state of affairs at the conference they attended. Nonetheless, it seemed at odds with my impression that Merton's work continued to be used in on-going research and that his name had not disappeared from the literature. This impression was based on the "Profiles" reports issued weekly by the ISI Thomson Personal Alert service, which lists all citations to the work of authors the Profile-user specifies that appear in the thousands of journals in the Institute of Scientific Information database. The conjecture also seemed at odds with data contained in the *HistCite Databases* reports Eugene Garfield periodically prepares on the historical and current uses made of the work of scientists whose work especially interests him, including that of Robert Merton.[18] However, not having followed the academic fate of the Matthew Effect in recent years, I had to wonder about how much it is being used and whether the identity of its originator was undergoing obliteration. Of course, by definition, OBI cannot be detected from data on citations or other indicators of use. If citations are absent, it is still possible that the underlying ideas, concepts or findings continue to be influential. If OBI is to be detected, analysis of texts is needed to determine whether the concepts, findings and terms of interest appear but do so with incorrect attribution or without any attribution at all.

3.1. Uses of the Matthew Effect in Science and Scholarship and in Popular Discourse

Counting citations scientific or scholarly contributions receive in academic journals remains the standard procedure for empirically assessing their use or "influence." Now, as it has been since it was introduced, citation counting is an imperfect means of assessing use or influence, much less of determining the "quality" of a given contribution. Having worked with citation data for decades, I am well aware of their shortcomings (Zuckerman 1988). Still, citation counts do serve as rough-and-ready measures of the notice scientific and scholarly papers receive and thus they are used here.

[18] This personal alert service, invented by the ingenious Eugene Garfield, is an extraordinarily useful means of keeping up with publications on topics of interest and with the work of authors one chooses to follow. Users can specify the authors whose work they wish to follow as well as key terms in which they are interested. See also http://garfield.library.upenn.edu/histcomp/index-merton.html.

More popular usage is harder to get at. The Lexis-Nexis database, which covers the business, medical, and legal disciplines can serve as a source of information but its coverage and organization limit its use for this purpose.[19] An alternative source on popular use is the number of appearances or "results" search engines such as Google uncover. If citation counts are crude measures, Google results are even more so. The number of results reported varies from day to day and even hour by hour. Moreover, Google's rules for inclusion and exclusion are bit eccentric and not entirely clear.[20] Still, if due caution is exercised in interpreting the data on results, some general sense of the extent of popular usage can be drawn from Google's reports.

3.2. Use in the Scientific and Scholarly Literature

These days, the most comprehensive source for citation counts is The Web of Science (WOS).[21] Its vast coverage of journals and other scholarly publications make it a better source than JSTOR with its

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[19] Susanne Pichler, the Mellon Foundation's highly skilled librarian has helped me understand the limitations of these databases. The Lexis-Nexis database is comprised of a number of sources. Lexis covers mostly legal sources while Nexis covers news sources, including magazines and newspapers. But they are organized so that it is impossible or next to impossible to search all sources at once. Moreover, coverage of magazines and newspapers does not begin at a uniform date. Nexis files begin in 1977 for the *Washington Post*, in 1980 for the *New York Times*, and 1985 for the *Los Angeles Times*, clearly none are covered from the date of its first publication.

[20] Susanne Pichler also introduced me to the complexities of the Google database. She reports that "(1) A Google result seems to equal one web page. If a term is found on more than one page on a site, Google will show only the first two pages and then provide a link to additional pages. (2) The results number is an "unreliable" estimate because the search engine does not actually tally all the results because that would take too long. Google also does not search entire documents. Searches are limited to the first 17,000 words of a web page. (3) A web page might show up as a result even if it does not contain the search term of interest. If another page makes the 'Matthew effect' for example a hyperlinked term, the page to which the link points will also show up as a result even if the term is nowhere on the page." Private communication, Pichler to the author, July 21, 2009. In short, there are reasons for Google results to under-represent and over-represent the use of the same term. Some confusion can be avoided by specifying the term of interest by putting it in quotation marks and by specifying names as completely as possible, e.g., by using Robert K. Merton not Robert Merton. This helps avoid the conflation of Robert K. Merton with

more limited collection of journals for estimates of use by the entire community of scientists and scholars.[22] It is also more reliable than Google Scholar that appears to be plagued both by repeated references and un-cited references and thus provides over- and under-counts of actual citations (Jacso 2009).[23] WOS covers 10,000 journals and 110,000 conference proceedings in 256 disciplines in the sciences, social sciences and humanities; its coverage goes back to 1900. In effect, it is an oversized and far more comprehensive successor to the familiar *Science, Social Science* and *Arts and Humanities Citation Indexes*. Citation data for of the 1968 Matthew Effect paper are shown in Table 1. They indicate that the Matthew Effect is definitely writ large—at least as gauged by citations in the journals WOS covers. From its publication in December 1968 through the end of 2008, it has been cited in 720 papers.[24]

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his son, the economist, Robert C. Merton, who is also mentioned in Google with great frequency (though fewer times than his much longer-lived father. However, if texts fail to discriminate between Robert K. and Robert C. and settle for Robert Merton, then searches may not turn up in "results," or produce correct ones. In light of all its shortcomings, Google mentions at best provide an approximation of use in popular communications.

[21] Produced by the publisher, ISI-Thomson Reuters, the Web of Science®, according to its website, provides researchers, administrators, faculty, and students with quick, powerful access to the world's leading citation databases. Authoritative, multidisciplinary content covers over 10,000 of the highest impact journals worldwide, including Open Access journals and over 110,000 conference proceedings.

[22] JSTOR, a useful source for fine-grained citation analysis, is far less comprehensive in the sciences than The Web of Science and thus is less useful for the task at hand. Between 1968 and 2008, JSTOR reports show the first Matthew Effect paper has been cited 426 times. As Table 1 indicates, the number of citations in WOS is almost twice as large and is smaller than in Google Scholar, which has its own shortcomings.

[23] Not only is no definitive description provided of Google Scholar's sources of data, the difficulties of using become evident when its citation results are closely examined. Inspection shows that the same paper can be listed a number of times if the style of citations to it differ even minutely from one another and, as the source noted above indicates, authors are not always credited with their papers.

[24] Merton's habit of publishing collected papers makes citation counting difficult since citers often fail to cite the particular paper to which they refer but give only the page numbers in the collected volume. Searching out citations to the reprinting of the Matthew Effect in *The Sociology of Science*. Chicago: University of Chicago Press, 1973 would be possible in

Table 1: *Number of Citations to "The Matthew Effect in Science" Science. 199; 3810, 1968: 55–63 in the Web of Science**

Decade of Citation	Number of Citations	Cumulative Citations
1969–77	88	88
1978–87	135	223
1988–97	126	349
1998–2007	304	653
2008	67	720

This paper was reprinted in R. K. Merton, The Sociology of Science. Chicago: University of Chicago Press, 1972, pp. 439–459. The data from the Web of Science underestimate the true number of citations the Matthew Effect received because citers often fail to designate the chapter title when citing the book.
Total Citations to this paper in JSTOR 1968–2008: 424
Total Citations to this paper in Google Scholar 1968–2008: 1613
* Thomson Reuters Web of Science

By way of comparison, most papers are rarely cited.[25] Although the frequency of citing older papers differs among the fields,[26] it is nonetheless highly unusual for a paper to be cited so often and for such a long period of time. Most papers sink into obscurity within several years after they first appear. Moreover, OBI tends to reduce the number of citation to papers over time, even of those that were once often cited. Because relatively few papers are frequently cited year after year over long periods of time, Eugene Garfield has earmarked these as “Citation Classics” (Garfield 1993b) or “landmark

principle but not readily feasible. In principle, it should be possible to track the use of the term in the journals of various disciplines to lean more about its diffusion and its inter-disciplinary use but I have not done so here.

[25] It is best not to compare numbers of citations to papers in different disciplines since there is considerable variation among them in this respect. For a compendium of wisdom on citation analysis and for data on average citations, see Eugene Garfield’s 15-volume set of his collected *Essays of an Information Scientist* (1961–1993).

[26] Older papers are more often cited in the humanities than in the sciences and empirical social sciences and for good reasons; the relevance of older work decays at a much slower rate in the humanities and older works also serve as the basis for humanistic study.

papers."[27] These papers, far more often than others, are of particular importance in the evolving history of their fields.

Not only has the Matthew Effect been heavily cited since its publication, as Table 1 shows, but its citations per decade have increased over its four-decade lifetime. Indeed, it has been cited more in its fourth decade than at any earlier one.[28] Whether this upward trend will continue is, of course, unknown. As we shall see, these increases in citation reflect the broad application of the Matthew Effect in a number of different fields. Judging from Garfield's Hist-Cite data on "the Matthew Effect," it is an intellectually cosmopolitan contribution, having been cited in 368 different journals between 1968 and 2009; furthermore, these cover the waterfront of disciplines and are definitely not confined to Merton's home discipline of sociology.[29]

But to some extent, the growth in citations the Matthew Effect exhibits could be an artifact of the growing size of the WOS database, including the inclusion of the Arts and Humanities Index and conference proceedings. According to Eugene Garfield, it would be very difficult to determine how much growth in the database has contributed to the observed increase in citation through time.[30] While this matter remains unresolved, for a quick but imperfect comparison I note that the second Matthew effect paper has been cited relatively often but its citations have not escalated at anything like the same rate since its publication two decades ago, thus suggesting that growth in the data-

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[27] The two terms are often used interchangeably. Citation Classics are identified not by the frequency of their citation alone but relative to the average citations to papers in the journal in which they appeared. Garfield has published accounts by the authors of about 4000 or 40 percent of papers that fit these criteria between 1977 and 1993. They are published in his Essays of an Information Scientist and available at http://garfield.library.upenn.edu/classics.html.

[28] Data from the Web of Science on citations to Matthew II from 1988 the first year of its publication to the present are available on the Web of Science. Yearly citations since 2003 have averaged about 10 per year, significantly higher than the average paper but lower than Matthew I.

[29] Taking journal titles as indicators, HistCite records show that the 1968 Matthew Effect paper was cited in disciplines as diverse as pharmacology, nuclear engineering, library science, zoology, physics, business law, demography, social work, education, political science, gerontology medicine, psychology, history of science and of course sociology.
See http://garfield .library.upenn.edu/histcomp/merton-matthew-I/index-tl.html

[30] Private communication.

base has not led to similar rates of escalation in citations to all papers.[31]

It is also possible that the citation history of the Matthew Effect is in a sense "self exemplifying." [32] It has likely benefited from being by a famous author and from being a "famous paper" and for these reasons may have been cited more often than it might have been had it not gotten as much attention as it initially did. Beyond this, Merton has been highly prolific and this in itself has made his papers highly visible. Such speculation is consistent with the findings of a study of economists' proclivity to cite the work of well-known economists (Tol 2008).

Finally, two qualitative indicators suggest the Matthew Effect is atypical of scholarly contributions. Unlike other terms-and-concepts, The Matthew Effect itself has been prolific, spawning its own terminological offspring in addition to the Matilda Effect. There is now a "Mark Effect", an "Ecclesiastes Proposal" and a set of "Ecclesiastes Effects," all of course serve as implicit citations and have their own biblical ring (Bothner, Podolny, and Smith 2010, forthcoming; Turner and Chubin 1979; Harsanyi and Harter 1993).[33] Another indicator of the Matthew Effect's influence and its diffusion is its citations in journals in languages other than English, including French, German, Italian

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[31] These two papers, are however, not comparable except in their having the same author. Matthew II was published in *ISIS*, a far less visible journal than *Science*, and it was published twenty years after Matthew I. Even if one were to examine the average citations to all papers published in *Science* in 1968 to establish a benchmark for judging the citation history of the Matthew Effect, doing so would provide an unsatisfactory comparison. *Science* then as it does now publishes a variety of kinds of papers including short research reports, longer research papers on the scale of the Matthew Effect, analytic inquiries about one or more scientific contributions, policy analysis and news about the sciences, each having a different propensity to be cited. Furthermore, given the differences among disciplines in the extent to which papers are cited and the relatively small number of papers published in *Science* which might be cited in journals of the social sciences and humanities, this exercise in comparison would be unlikely to lead to a conclusion worth drawing.

[32] This, of course, is still another phrase Merton coined. "Self-exemplification" was, he thought, an intriguing feature of social life, one needing more analysis than he had the opportunity to give it.

[33] There are also "Matthew Effects for Nations," "Matthew Citations," and "Matthew Core Journals," all bibliometric phenomena invented by Manfred Bonitz and his collaborators and described later in this paper.

and Chinese.[34] This said, the cautious conclusion to draw is that the Matthew Effect paper has been highly cited for a long period of time, that increasing citation as time has passed makes it different from run of scientific and scholarly publications, and that the paper has been and is still being used.

3.3. The Matthew Effect in Popular Discourse

Tracking popular use of the Matthew Effect in the Google database calls for an unusual degree of skepticism about the meaning and reliability of data. As I have indicated, the calculation of the number of "results" Google publishes is not straightforward; numbers vary daily and hourly and thus not too much weight should be put on absolute numbers or on differences among them.

Table 2 Number of Google Results: Selected Mertonian Terms-and-Concepts, December 27, 2009

Term-and-Concept	Number of Results/ No Authorial Specification	Number of Results Term + Robert K. Merton
Role Model	10,200,000	4,040
Focus Group	3,490,000	13,000
Unintended Consequences	1,850,000	19,900
Self-fulfilling Prophecy	498,000	23,300
Middle-Range Theories	425,000	1,250
Role Conflict	424,000	2,750
Status Set	403,000	522
Influentials	229,000	1,317
Role Set	146,000	2,050
Matthew Effect	113,000	26,700
Cumulative Advantage	47,000	3,920

[34] Merton himself noted in 1988, in Matthew II, that the term had diffused widely and was in common usage in the west and that there was even a term in Chinese for it (1988, fn. 7, 609). For one example, see Nicholas Carayol "Les propriétés incitatives de l'effet Saint Matthieu dans la compétition académique" (2006). If Merton were writing this paper, he would surely track down the variants of the Matthew Effect in different languages, their definitions in the relevant dictionaries, to determine when they were introduced and by whom—no small exercise in sociological semantics.

Term-and-Concept	Number of Results/ No Authorial Specification	Number of Results Term + Robert K. Merton
Manifest and Latent Functions Local and Cosmopolitan	12,200	2,230
Influentials	8,440	254
Bureaucratic personality	6,270	255
Anomie Theory	4,730	2,670
Multiple Independent Discovery	2,870	1,430

[1] Terms were entered into Google with quotation marks. The number of "results" Google lists varies from day-to-day. Daily changes are not substantial but small differences in entries can make large differences in the number of results. Thus "The Matthew Effect" yielded 113,000 mentions but Matthew Effect yielded 26,000 mentions.
[2] The Law of Unintended Consequences, a term Merton never used, produces 153,000 results; with his name, results number 757.
[3] The results Google identified for "status set" indicate that the term has many meanings: it is used in computing (IBM's System Director V6.1x contains a procedure for viewing the status set manager); it is used in communications technology (including Skype and Gmail) and in Facebook. There may be some connection between Mertonian status sets and Facebook's status sets but the remaining uses were probably invented independently of his work.
[4] Results for "Anomie Theory," without authorial specification, appropriately include Emile Durkheim, A. K. Cohen, Steven Messner, and Marshall Clinard. "Strain Theory," a term which has become nearly synonymous with Anomie theory, is used far more often than the original term, indeed, there are 94,000 Google mentions of "Strain Theory" and 14,000 with Merton's name attached. As far as I know, he never used the term either in print or in lectures he gave on Anomie Theory.

Table 2 reports the frequency of "results" in Google of an unsystematic sample of terms Merton coined and concepts he developed. The first column shows that a number of Mertonian coinages have become part of the language as the Web captures the way Americans speak and write. These coinages are not only numerous but they are intensely used. Note also that the Matthew Effect is by no means the most "popular" of all of his linguistic inventions; in this group, it is far exceeded by "role model,"[35] "focus group," and "unintended conse-

[35] Merton is generally credited with having developed and identified the term "role model." In fact the term first appeared in print in two sources in the same year. Merton first discussed role models in his expository essay,

quences."[36] The second column shows how often these coinages have been attributed to him specifically.

Evidently, attributed terms are far less frequent than those which have become uncoupled from their originator, as one might have predicted. Google results make clear that this is so for each and every coinage. Is this a feature of Merton's work but not others'? It is not. The same uncoupling or non-attribution showed up when I searched for terms closely associated with the work of two other distinguished and widely recognized sociological theorists, Talcott Parsons (for example, pattern variables) and Pierre Bourdieu (habitus). (See Appendix Table 1) Uncoupling is likely quite common.

Can this procedure of comparing attributed and unattributed "results" be used to measure the extent to which OBI has set in for a variety of contributions? I am hesitant to say it can although it can be a means of identifying instances in which OBI may have occurred. At the same time, the mechanisms producing OBI are distinctive to the scientific and scholarly world and fail to give sufficient weight to ignorance and lack of interest, both of which are likely to account in part to the observed uncoupling of authors from their contributions in popular discourse. And of course, coinages are only a gross indicator of the

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"Continuities in the Theory of Reference Groups and Social Structure" (1957b, 302–304), where he wrote "The *reference individual* has often been described as a *role-model"* and then explores at some length the differences between the two concepts and the processes of their selection. That same year, Wagner Thielens, wrote that "medical students are inclined to "choose a figure in the profession [of medicine] […] as a model to imitate… In short, they adopt a role model" (Thielens 1957, 137). As I observed later, neither text makes it clear who coined the term. "This suggests that the idea and its associated term had some currency at the time, surely in the Columbia context," since Thielens was one of those working on the Student-Physician project. Evidently, neither believed that staking claim on the term was of great importance (Zuckerman 1989, 233).

[36] Results the Google database turns up also suggests that interest in the Matthew Effect has heightened since its appearing in Malcolm Gladwell's 2008 bestselling book, *Outliers: The Story of Success*. Gladwell called his opening chapter "The Matthew Effect," made it the principal theme and cited the original source correctly. As far as one can tell, Gladwell's having mentioned the Matthew Effect, may have boosted the number of results mentioning the Matthew Effect. (Results for Matthew Effect+Merton+ Gladwell clock in at 754, small numbers in comparison to other terms reviewed here).

extent of an author's contributions. Nonetheless, textbook examples of OBI can be identified using this procedure; they are vividly evident where the term appears but the author cannot be found among the references.[37] Fine-grained studies of OBI require more than using a mechanical means of identifying their presence.

Table 2 also shows that despite Merton's name being attached more often to The Matthew Effect than any other term in the list, it has been decoupled from the term more than three-fourths of the times the search engine turns it up. Judging on the basis of the share of results for his coinages that are attributed to him, his connections with Anomie Theory and Multiple Independent Discovery appear to be the firmest but not much should be made of the numbers involved.

Table 2 provokes several additional observations. First, it is not altogether obvious why some of these terms are more often attributed to Merton than others. I had thought that ideas which had wide spread implications such as self-fulfilling prophecy, role conflict, role model, and influentials would make their way in the language without attribution. This does seem to be the case. However, these same terms vividly convey their own meanings; users need not know their origins in order to know what they mean and how to use them. James Shulman has proposed the appealing and entirely consistent hypothesis that intrinsic obscurity of a term should enhance the strength of its connection to its originator since its meaning cannot be determined without some sense of where it came from.[38] This too seems consistent with the data on the Matthew Effect and in the cases of anomie theory, multiple independent discovery, and manifest and latent functions. But

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[37] This is quite different from cases in which authors' names and ideas appear in a text but not in the bibliography. I could not help but note the following intriguing title, "Vertigo and the Global Merton." It refers to the widespread (read global) phenomenon of those at the "bottom of society" being "caught in a late-modern day Mertonian dilemma" of being inculcated in a culture emphasizing equality of opportunity but providing highly unequal access to opportunities required for success. No citation is given but both title and text clearly indicate that OBI has set in since the authors evidently know who was responsible for the work on anomie theory to which they refer and assume that he has become so well-known that there is no need to cite him or the original source (Young 2008, 503).

[38] This is the same James Shulman, who is the author of the Introduction to the Merton and Elinor Barber volume, *The Travels and Adventures of Serendipity* (2004).

this same finding could also be explained by the fact that these terms are very likely more often used by scholars, and that their appearance in Google registers that fact, rather than the terms having moved into the language more generally. On this reading, the reasons for certain contributions being attributed to Merton and others not (all apart from their explaining the same patterns for Parsons and Bourdieu) are far from settled.

Second, the data in Table 2, crude as they are, suggest that to a considerable degree, despite Merton's name having become uncoupled from each one of his contributions, his name continues to be mentioned in popular discourse and in the press and other publications are captured on the Web. While it is clear that OBI cannot be avoided, since Merton was so prolific and since his work has had so long a shelf life, nothing like total obliteration has occurred.

Third, the data raise the question as to whether some number of results produced for the Matthew Effect can be attributed to Malcolm Gladwell's having made it the centerpiece of the first chapter of his best-selling book. That is, Gladwell's adoption of the Matthew Effect has probably boosted the number of times the term now appears in general usage, leading to a kind of second-order Matthew Effect, with Gladwell's making the Matthew Effect and Merton more famous for having appeared in a famous book by a famous author. In another instance, Merton called this an instance of "the serial diffusion of ideas and terminology […] via mediated sources" (Merton R. K. 1955, 388).[39]

This much said, the data in Table 2, tell nothing specifically relevant to the Elkana–Wittröck observation that Merton's name is under-

[39] I know of no research on the frequency with which such "booster effects" occur, that is, how often terms-and-concepts are popularized well beyond their original audiences but it is not unusual. It is also not under the control of the originator who can be no more than a bystander to the event. Many years ago, Peter Messeri, one of Merton's students (and one I also like to claim), analyzed the extent to which fundamental contributions to plate tectonics in geology were OBI'd. He read, if memory holds, all the major journals in the field of geology and the relevant books from the early work on Continental Drift over a period of twenty years and noted when major terms and concepts appeared and whether they were attributed to their authors (Messeri 1978). Unfortunately this paper was never published although his excellent study of the reception of the ideas of plate tectonics in geology based on the same inquiry did appear (Messeri 1988).

going obliteration in sociology despite the continuing vitality of his ideas. They are silent on the sources that fail to cite Merton when his contributions are used. Without a search for each and every "result," there is no way of knowing from the raw numbers whether non-attribution is general or more frequent in sociologists' writings. An empirical inquiry into the question of uncitedness is far beyond the scope of this study of the Matthew Effect but there is no question that painstaking as it would be, it could be done. Further empirical inquiry into OBI and citations should reveal much about the disappearance of contributors' identities as the research fronts of fields of science and scholarship change with time (M. H. McRoberts and B. R. McRoberts 2010).[40]

More provocative are the instances in which Merton's name has vanished and the concept-and-term is attributed to others.[41] This is not a new phenomenon attributable to Gladwell's having introduced the Matthew Effect to a wide audience of readers in 2008, and despite the fact that he (sans Merton) is coupled with the Matthew Effect almost 5000 times in Google, and despite the further fact that he took care to cite Merton's work correctly. As early as 1977, the Matthew Effect was attributed to F. R. Jevons, the émigré British biochemist. Quite independently of Jevons, the Matthew Effect has been attributed to a

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[40] Based on a study of a small sample of papers in biogeography using a method similar to Messeri's, they conclude that most of the work used in these papers is not cited. Ironically, they fail to take note of the possible contribution of OBI although they do cite Merton's collection of essays, *The Sociology of Science*.

[41] An early example of the Matthew Effect being erroneously attributed to another author can be found in Hans Mohr, *Lectures on Structure and Significance of Science* (1977, 26). Mohr writes that deviations from the "ideal sequence" of extrinsic rewards following intrinsic ones "are explained in Jevons' book [(1973)] by the Matthew Effect" and then Mohr manages on the very next page (27) to coopt Merton's local and cosmopolitan influentials by noting that "Cosmopolitan refers to recognition by the world-wide scientific community; local refers to the recognition the particular scientist receives from the other competent members of his institution." Mohr, a German plant physiologist, was evidently misled by F. R. Jevons who fails to cite Merton's contribution where he mentions the Matthew Effect (80). He then manages to attribute the idea of Local and Cosmopolitan recognition to Steven Box and Stephen Cotgrove by citing their paper, "Scientific Identity, Occupational Selection, and Role Strain" (1966). This tangle of instances of OBI turned up in one of Merton's folders labeled "Accum ADV as OBI." It otherwise went un-annotated.

psychologist, Herbert Walberg, who used the term-and-concept to account for marked differences in achievement scores of young adults on a test of proficiency in science and generalizes the findings so that they are evidence for the "rich getting richer and poor getting poorer" because of their differentials in motivations and exposure to education (Walberg and Tsai 1983).[42] A year later, Walberg and three co-authors, introduced a further measure of ambiguity into the meaning of the Matthew Effect by writing "'Matthew' or 'cumulative advantage' [sic] effects are a notable example in education. Walberg and Tsai […] found that individuals with advantageous family and educational experiences […]" (Walberg et. al. 1984, 92).[43] Once the conflation of authorship appeared in print, the process of mis-citation was set in motion. Three years later, another educational psychologist, Keith Stanovich wrote "Walberg, following Merton, has dubbed those educational sequences where early achievement spawns faster rates of subsequent achievement [or] 'Matthew Effects.'" Stanovich goes on to observe that "The concept of Matthew effects springs from findings that individuals who have advantageous early educational experiences utilize new educational experiences more efficiently" and cites Walberg and Tsai for this claim (Stanovich 1986, 381). By 1993, in a set of reflections on his work, Stanovich claimed a part of the term and concept for himself and wrote "Even more popular has been my work on Matthew effects in reading development. The term Matthew effects derives from the Gospel according to Matthew […]. It is used to describe rich-get-richer and poor-get-poorer effects […]. Herb Walberg […] had focused attention on the processes […] and in a 1986 paper, I specifically explore the idea of Matthew effects in the domain of reading achievement" (Stanovich 1993–4). This turn of events was followed by two erroneous citations of the Matthew Effect "a term coined by Keith Stanovich,"[44] and "in 1983, Walberg and Tsai first coined the term 'Matthew Effect' to describe the fact that, without intervention, some students rapidly develop and build upon strong literacy foundations,

[42] Herbert J. Walberg and Shiow-Ling Tsai's paper contains the appropriate citation to Merton and the original source.

[43] Later in the paper, they observe that "Merton […] for example, quoted the Gospel of Matthew in the Bible on 'the rich-get-richer' phenomenon and argued that such Matthew effects operate in scientific productivity" (p. 108). The Matthew Effect is cited in the bibliography.

[44] See *www.wrightslaw.com/info/test.matthew.effect.htm–53k* Aug 1, 2008, 2–4.

and other students languish behind their more fortunate peers."[45] I will return to the pattern of changes in meaning and generalization of ideas in tracking the Matthew Effect well beyond the field of education.

Such details of mis-citation, unimportant in themselves, nonetheless illustrate how authors and their contributions can become disconnected; in this instance in stages and with the participation of some of those who benefited from the reallocation of credit. This example of OBI contains another twist, for it involves what Merton called the "Palimpsestic Syndrome," which refers to the tendency "endemic among scholars […] to attribute a striking idea of formulation to the author who first introduced us to it" (Merton R. K. 1965, fn. on 218–219).[46] The syndrome—still another instance of Merton's interest in the misallocation of credit and its sources—occurs even when appropriate citations appear initially but, as time passes, they become confused, degraded and eventually disappear.

4. How Pervasive is the Matthew Effect?

Searching in Google for appearances of the Matthew Effect shows that the term (and often the concept) has been applied to an extraordinary array of phenomena.[47] Sometimes these applications are on target, sometimes they are not. Indeed, Daniel Rigney's new monograph *The Matthew Effect: How Advantage Begets Further Advantage* (2010) seeks to be as comprehensive as possible in identifying these applications and perhaps goes farther than that in earmarking uses of the Matthew Effect and the variety of phenomena in social life which seem to exemplify it. Rigney's volume covers Matthew Effects in science and technology, the economy, public policy, education and culture and gives detailed attention to trends in economic inequality. This strategy of inclusivity has led him to assemble much of the relevant

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[45] Sebastian Wren "Matthew Effects in Reading," Developing Research-Based Resources for the Balanced Reading Teacher. Available at http://balancedreading.com/ matthew.html, 2.

[46] Merton would no doubt have been bemused by the appearance of such examples of the "palimpsestic syndrome" in the history of use of the Matthew Effect.

[47] Apart from being used in an array of empirical inquiries, The Matthew Effect has been the subject of a small number of analytical papers addressing its assumptions and implications. The relevant references are listed in an appendix at the end of this paper.

literature needed to understand how the Matthew Effect has spread. Rather than taking up each and every instance of research on putative Matthew Effects, I confine this analysis to those publications that not only use the term but also make it central by placing it in their titles. This selection criterion is narrower than Rigney's, broader than Merton's analysis might permit but has the virtue of capturing how the Matthew Effect, as term and concept, has not only been used but more so has assumed a major role in these publications.

Domains of social life claimed to exhibit Matthew Effects are as different as public health and health care, nominations and awards of Oscars, the effects of education and acquisition of reading skills, career attainment in science, the clergy and prostitution, sports, sports and organized competitions, bibliometrics including the distribution of citations among nations and in citations, the origins of pay differentials, the effects of taxation and the distribution of wealth, and as an explanatory variable in the reception of ideas—that is, why some ideas are taken up and others fail to be so. Based on the outcomes of an informal "qualitative content analysis" of these papers, I suggest that the Matthew Effect has been so enthusiastically adopted as a covering term for a variety of disparate phenomena because the term in-and-of-itself is both appealing and attention grabbing. This is not all, it appears especially in research publications because its use seems to demonstrate the often-limited phenomena under study have more general implications than might be immediately evident to innocent readers. Put another way, it is used to place the reported findings in an expanded conceptual context and thus it adds a certain aura of theoretical legitimacy to quite narrow investigations

4.1 Public Health, Health Care, and Aging

Differences among nations in comparative public health, in access to psychiatric care and in intra-cohort variations in aging have been attributed to Matthew Effects. Central in these analysis is the idea that the rich get richer (here, the rich are those who are healthier and have access to greater health care resources) while the poor get relatively poorer, sicker and die at a greater rate. Public health studies report growing disparities in infant mortality among nations and in ratios of expenditures on health relative to national defense over time; such disparities are taken as prime indicators of the presence of Matthew Effects. Indeed, the term has come to be shorthand for data showing outcomes which become increasingly different over time since when depicted graphically, they exhibit a "fanning out" on measured outcome variables (Joseph 1989).[48] More in line with the Matthew Effect as Merton understood it is research showing that access to psychiatric care tends to go to those who need it least, as Link and Milcarek put it: "[Y]ounger, more motivated, more communicative and more competent patients are more likely to receive attention. Less 'desirable' patients, those most in need, are likely to receive no therapy at all. This is seen to be a manifestation of Merton's 'Matthew Effect' and a demonstration that even when it comes to the treatment of psychiatric patients, the familiar dictum, 'advantage accumulates' appears to hold" (Link and Milcarek 1980, 279).

In studies of aging, the Matthew Effect serves as a theoretical device for explaining the findings of multiple studies which show growing "intra-cohort" differences over the life course in such outcomes as family income, occupational status (whether people are "stuck" or "moving,") and psychophysical aging. The Matthew Effect, according to Dale Dannefer, accounts more effectively for the increasing heterogeneity observed within age cohorts than does the competing view that such heterogeneity results from "accentuation of individual differences" which develop with aging. The Matthew Effect, Dannefer suggests, is conceptually superior because the differences it generates are caused by "structured mechanisms of social allocation producing similar differentiating tendencies in successive cohorts […] producing advantage and disadvantage." This explanation avoids the logical pitfalls of attributing data on groups to changes in individual biology or psychology. Matthew Effects, seen as "cumulative effects" which individuals experience through time and which produce growing differentiation within groups, come together here as parts the same causal processes (Dannefer 1987, 216).

[48] Joseph reports that Matthew Effects arise because the populations of nations with lower defense to health expenditures are more literate and have better skills in making use of expenditures on health care. A later paper in the same series compares the international data with inter-province data for Canada and reports that disparities among provinces have been decreasing due to intensive efforts to equalize health care services (Dzakpasu et. al. 2000, 1–8).

4.2 Oscars: Nominations and Awards

A far cry from research on public health and aging, Oscar nominations and awards for film and correlations between them are also said to exhibit the Matthew Effect. The underlying idea is that the number of nominations a given film receives in various categories (such as best actor or director or cinematographer) is a sign of approval by the nominating panels and this approval in turn plays a key role in determining which films receive the award.[49] Over the 80 years of the Oscars, analysis of the Oscar database shows that nominations for Oscars follow the familiar Gini coefficient—a very small number of films get many nominations, a larger number receive several nominations while most that are nominated at all receive just one or two. This distribution becomes the basis for considering films with many nominations as "rich" and those with few nominations as "poor." Put this way, the question immediately becomes germane to the Matthew Effect: do rich films get richer by receiving Oscars while the poor films lose out by being denied them?[50] They

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[49] The so-called "Best Seller Effect" like the Oscar awards involves the influence of visible collective approval on the outcomes of a competition. In his *Mind of the Market*, Michael Shermer equates the "Best Seller" effect with the "Matthew Effect" and adds that "Marketers know it as cumulative advantage," a trifecta of conceptual homogenization. He vividly portrays the processes set in motion when a head start in sales leads people to want a product, a book for example, on the assumption that what others want must be desirable and they too must have it. Being on the best seller list "sends a signal to potential book buyers [...] that this must be a good read, triggering an increase in sales that gets reported to *New York Times* book review editors, who bump the title up the list [and so] [...] round and round the feedback loop as the richest authors get even richer" (Shermer 2009, xiii–xvi).

[50] This study draws on the Oscar Database that contains information on the 8616 films nominated for the award and the 840 winners chosen in every category between 1927 and 2007 (Rajaraman 2009). Nominations for music and special effects for less visible and profitable awards are far more likely to go to films with few nominations, as Rajaraman's observes, "talent gets its due without help from Matthew" (ibid. 3rd paragraph). Quite correctly, one reader of this posting noted that the same clustering of findings could result from the tendency of talented individuals to work together. Although the title also calls attention to the possible intrusion of "halo effects," that concept-and-term is not really applicable since it refers to cognitive errors in which positive or negative assessment of one or more attributes leads to the incorrect assumption that other attributes of the same subject have same value.

do. “Rich” films receive approximately half of all the Oscars but have received only about a third of the nominations, and furthermore, the really significant (and literally “rich”) awards, such as the Oscar for “the best picture” and “the best director” almost always go to multiple-nominated films. Are such results the outcomes of Matthew Effects? Is prior approval a determinant of current recognition? Possibly. The numbers are consistent with this reading and they emphasize that aspect of the Matthew Effect in which prior high standing influences current standing but a skeptic might note that massive Oscar campaigns by the large film companies could also explain these findings in that both nominations and awards are the product of substantial financial investments leading both literally and figuratively to the “rich” making the rich richer and the poor poorer, if getting poorer means not reaping awards which might otherwise have come their way were the campaigns less dependent on money. Whatever the causal connection between being nominated for Oscars and winning them, it does appear that a critical element of the Matthew Effect, that is, that current success is affected by prior success is satisfied in the case of Oscar awards and further that competitions may be important sites for uncovering Matthew Effects.

4.3 Education and the Acquisition of Reading Skills

Judging from the frequency of citations to the Matthew Effect in journals of education and educational psychology, the term-and-concept has become thoroughly embedded in research in these fields. With its first use by Walberg and Tsai more than a quarter century ago to describe the predictive power early educational experiences on young adults’ motivations and current achievements, gauged here by their success on a test of scientific knowledge (Walberg and Tsai 1983, 373), the Matthew Effect caught on as term-and-concept in education. As I noted, several years later, it was adopted by Keith Stanovich, a reading researcher, to describe the powerful influence of children’s early success in reading on their later success in reading thus “demonstrating” the presence of the Matthew Effect (Stanovich 1986). His work showed the effect of early reading success on measured ability which was explained by a combination of psychological and social processes; children who read well are inclined to read more, to have better vocabularies, choose friends who are good at reading and ultimately apply these skills to learning in other areas. Similarly early failure in learning to read is said to produce problems in later reading and children falling farther and farther behind their peers who were better readers. As the term spread, its Mertonian origins have been unevenly retained, disappearing in some instances and transferred in others to early users of the term. Research continues on how the Matthew Effect works in education,[51] in producing increases in measured IQ (Shaywitz et. al. 1995) and on the practical lessons that might be drawn for reading pedagogy (Schumm 2006, 33).

This body of work has acquired many of the features of a "research programme" (Lakatos 1978)[52] in which the Matthew Effect serves a variety of purposes: as a conceptual framework for explaining increasingly disparate performance in groups; as a set of testable hypotheses; a method of organizing longitudinal data on achievement; an empirical generalization based on the way data array themselves; a description of a graphical display of data; and a theory of increasing disparities.[53] Thus the multiple purposes of the Matthew Effect in educational research provides evidence not only of the broad implications of the idea but also of its over-generalization, its growing imprecision and blurring as the term has diffused over time.

4.4 The Matthew Effect in Careers

Sociological research on the careers of individuals and organizations abounds with analyses of the role that cumulative advantage plays in producing large disparities in attainment between groups over the course of time (DiPrete and Eirich 2006). Yet the Matthew Effect, which after all, is a special case of cumulative advantage, turns up far less often in

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[51] For example, to what extent does reading interact with other cognitive skills and motivation to produce increasing differences between readers? (Bast and Reitsma 1998).

[52] I make no judgment as to whether this is a "progressive" or a "degenerating" research program in Lakatosian terms.

[53] Making the Matthew Effect a description of data, an empirical generalization and a source of prediction, Butler et al. write, "According to the well-established fan-spread of Matthew effect, rates of gain in academic achievement are proportional to early levels of achievement […]. The fan-spread effect is important in its own right but also has implications for the ability to predict reading achievement at different age levels" (Butler et. al. 1985, 350).

the literature on careers—despite its implications for scientists' careers being the context in which it was originally identified. This said, its use continues in a variety of studies worthy of consideration.

Consider only that Matthew Effects have been observed in studies of the careers of prostitutes (Mänsson and Hedin 2002), scientists[54] and disparities in standing of protestant congregations.[55] In research on prostitutes, the Matthew Effect is used to describe the downward social spiral of women's lives once they enter into the sex trade and their difficulties "exiting" from "the oldest profession." Here the Matthew Effect is made synonymous with cumulative disadvantage. The use of the Matthew Effect as an analytic tool in understanding the development of scientific careers is limited almost entirely to episodes in which junior collaborators are deprived of due credit; much more rarely, it refers to unequal credit going to co-discoverers of multiple independent discoveries. These conform quite closely to the Mertonian account of the Matthew Effect.[56] A near-perfect instance of the Matthew Effect is described by Richard Lewontin and John L. Hubby, the former, an internationally known and highly productive population geneticist, and the latter, his postdoctoral fellow at the time. In 1966,

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[54] Clearly Merton's basic papers belong here (R. K. Merton 1968 and 1988). Since the Matthew Effect as a concept was drawn from my work on Nobel laureates, I cannot but include *Scientific Elite* ([1977] 1997), among relevant citations despite the fact that the Matthew Effect does not appear in its title. The same can be said for Stephen Cole's research on the Matthew Effect (1970).

[55] W. Broughton and E. W. Mills, Jr., "Accumulative Advantage in the Ministry: the Matthew Effect Brought Home," Paper Presented at the meeting of the American Sociological Association (1976). This paper was subsequently published under the title of "Resource Inequality and Accumulative Advantage: Stratification in the Ministry" (1980) and thereby the Matthew Effect disappeared from the title somewhere between the two versions. Since Merton was one of those Broughton and Mills thank for his comments, it is likely that he—and probably the other commentators—noted that the Matthew Effect was less apt an conceptual frame than cumulative advantage since the objective of the research was to assess the role of increasing resource inequality through time on the prestige of congregations.

[56] A conspicuous example still evoking controversy among astronomers and astronomical physicists many years after the discovery of pulsars is the exclusion of Jocelyn Bell Burnell from the award of the Nobel Prize to Anthony Hewish Longair 2009).

they published "two breakthrough papers" which called into question accepted views about genetic variation and described a procedure by which it could be assessed in natural populations. Analyzing the citations their papers received over the next thirty years, they observe that despite their having rotated first- and second-authorship on the two papers, and despite the fact that the papers were published back-to-back in the same journal, and despite the fact that the work was "genuinely collaborative," the paper listing Lewontin first was cited 50 percent more often than the one on which Hubby was listed first. Many of the citations to findings reported in the paper Hubby first-authored were attributed to the one Lewontin first-authored and when only one of the two papers is cited, it is usually the one on which Lewontin is first. They conclude that this is "a clear-cut case of Merton's 'Matthew Effect'" (Lewontin and Hubby 1985).[57] Lewontin has since gone on to further scientific distinction with research on population genetics, genetic diversity, evolutionary theory, and one in which he has influenced the ideas about evolution of major philosophers such as William Wimsatt, Robert Brandom and Phillip Kitcher. Hubby spent the remainder of his career on the faculty at the University of Chicago but did not have the kind of intellectual impact his co-author and collaborator has had. There is no way of knowing whether the early Matthew Effect these "breakthrough" papers engendered had a lasting influence on Hubby's career.

4.5 The Matthew Effect in Sports and Organized Competitions

The Matthew Effect also appears in studies of sports and other competitions. As I noted earlier, Malcolm Gladwell, effective popularizer of social science and best-selling author, introduced his book, *Outliers: The Story of Success* with an account of the Matthew Effect in championship hockey (Gladwell 2008, ch. 1). The Matthew Effect, according to Gladwell, is exhibited in the much greater probability of mem-

[57] The papers to which this account refers were subsequently cited in 715 publications over the 30 years between their publication in 1966 and the writing of the citation classic. They found large amounts of genetic variation in the genome of species requiring questions to be raised about the validity of predictions of evolutionary theorists.

bers of junior championship hockey teams being born in the first quarter of the year than would be expected if talent were equally distributed across birth months. Clearly not an astrological effect, the birth month distribution turns up in junior champion teams in soccer and rugby (Tucker and Dugas 2009, 1–4)[58] and even in chess (Shehade 2008).

These studies focus on competitions children enter when they are young, where the participants are limited to a particular age cohort (for example, in Canadian ice hockey, selection for teams occurs at the end of the year candidates become ten years of age).[59] The overrepresentation of those with first-quarter birthdays comes about in these sports because players are more apt to be chosen if they are better developed physically and cognitively than those born later in the year. At the age of ten, differences between children of almost a year can make quite a difference in maturity. Once chosen, team members play more often and against better players, get better coaching, probably have better equipment and win far more often. Are these findings results of the Matthew Effect? They are, only if advantages accrue to those who do not "deserve" them. These data on success could just as well be attributed to then-and-there stronger players benefiting over time from cumulative advantage rather than from the "misallocation"

[58] They go on to observe that the South African rugby team data were least marked by birth-month correlations because of the weaker relationship between selection processes and relative age group performance, a kind of confirmation of the importance of selection, streaming and differential experience over age. They also comment that a self-fulfilling prophecy is at work in these outcomes, while not indicating any knowledge of where the idea of the self-fulfilling prophecy originates. This adds a measure of conceptual complication to their analysis and will be touched on later when I inventory a set of concepts that have been used interchangeably with the Matthew Effect.

[59] The analytical underpinnings of the processes that advantage older players were earmarked by Roger Barnsley, a Canadian sports psychologist who in 1985 described the concentration of high level hockey playing in those born early in the year as outcome of three processes: selection, streaming, and differentiated experience. Barnsley et al. (1985) did not link their work to the Matthew Effect. Barnsley has gone on to examine the birth-month distributions of National Hockey League players and those who play in the minor leagues (Barnsley, Roger and Thompson 1988). Selection, streaming and differentiated experience are familiar processes to those who study the effects of "tracking" in schools.

of rewards to children who happen to be born in the first quarter of the year that can, therefore, be taken to result from the Matthew Effects (Gladwell 2008).[60] Gladwell clearly opts for the Matthew Effect alternative and writes that such advantages are "neither deserved nor earned […]. The small advantage that the child born in the early part of the year has over the child born at the end of the year persists and grows […] children are locked into patterns of achievement and underachievement, encouragement and discouragement, that stretch on and on for years" (Ibid. 28).

4.6 The Matthew Effect and Bibliometrics

Bibliometrics, is both a field of study and a set of measurements of literatures and communications usually in science and scholarship but in principle, applicable to publications of all kinds. Originating in 1923, and having grown enormously since the 1950s, it treats "the careers" of publications, texts and information and the findings derived from their use (Hérubel 1999). It is still another domain in which the phenomenon of the Matthew Effect has been observed and deployed as an analytic tool. Bibliometric studies, in which the Matthew Effect figures divide into two sorts—those that focus on the distribution of citations in the scholarly and scientific literature, and those using citation analyses to study the development of ideas and their reception. Most striking "bibliometrically," are 30 or so papers by Manfred Bonitz and his collaborators, Andrea Scharnhorst and Eberhard Bruckner, who report the "discovery" of the "Matthew Effect for countries." This work was followed by the development of the "Matthew Index" (a ranking of nations according to their over- or under-citation relative to statistically expected rates); the identification of "Matthew citations" (or the excess number of citations papers receive above what would be

[60] Gladwell adopts a congenial sociological perspective on why individuals are successful. Rather than relying alone on innate talent or luck, "they are invariably the beneficiaries of hidden advantages and extraordinary opportunities and cultural legacies that allow them to learn and work hard and make sense of the world in ways others cannot." Being mentioned in *Outliers* is an apt example of what Merton, himself, termed "derivative or serial diffusion of ideas," that occur through "mediated references," that is citing work one encounters not in the original but in another source which cites it (R. K. Merton 1995b, 388).

expected on the basis of the impact factor of the journal in which they are published);[61] and the hierarchy of "Matthew core journals," which owing to their receiving more than their share of "redistributed citations" are the most competitive in the world. Bonitz and his collaborators propose that data on the "Matthew Effect for countries" and "Matthew core journal" provide measures useful for science policy—for determining which countries make the most effective use of their scientific talents and for decisions scientists "should" make about where they seek to publish. The Matthew Effect in these studies has come to refer to greater skewness or concentration in citations earned by papers, journals and nations than would be expected. Further, such concentrations or Matthew Effects are taken as indicators of the extent of competition in science and success in producing contributions of high quality. That aspect of the Matthew Effect which describes the misallocation of credit relative to their scientific merits has disappeared and been replaced by the idea that over- and under-citation relative to certain quantitative standards is its major distinguishing attribute.[62]

[61] Invented by Eugene Garfield, the doyen of citation analyst, founder of the Institute for Scientific Information and the creator of the various Citation Indexes, the "journal impact factor" is a measure reflecting the average number of citation to articles published in science and social science journals. It is frequently used as a proxy for the relative importance of a journal within its field, with journals with higher impact factors being deemed to be more important than those with lower ones.

[62] Referencing all the papers emerging from Bonitz, Scharnhorst and Brucker even if we confine them to those with the Matthew Effect in their titles would be excessive if the point is to illustrate the influence of the term-and-concept on their work. For a set of examples, see: M. Bonitz and E. Scharnhorst, "Characteristics and Impact of the Matthew Effect for Countries," *Scientometrics* 40 (1997): 407–22; M. Bonitz, "The Scientific Talents of Tations or Science and the Kingdom of Heaven. Bibliometric Matthew Effect for Countries Versus Biblical Gospel Parable of the Entrusted Talents," *Libri* 47 (1997): 206–123; M. Bonitz, E. Bruckner and A. Scharnhorst, "Characteristics and Impact of the Matthew Effect for Countries," *Scientometrics* 40 (1997): 361–78; M. Bonitz, E. Bruckner and A. Scharnhorst, "The Matthew Index—Concentration Patterns and Matthew Core Journals." *Scientometrics*, 44 (1999): 361–78; M. Bonitz and A. Scharnhorst, "Competition in Science and the Matthew Core Journals," *Scientometrics* 51 (2001): 37–51 and M. Bonitz, "Ranking of Nations and Heightened Competition in Matthew Core Journals: Two Faces of the Matthew Effect for Countries," *Library Trends* 50 (2002):

Thus, the psycho-social mechanisms producing the Matthew Effect have been put aside as the Matthew Effect has been transformed into a set of bibliometric laws and regularities.

Other bibliometric studies come closer to the original intent of the Matthew effect, seeking as they do to use bibliometric techniques to assess the extent to which credit has been misallocated. Put another way, such studies ask if the number of times works are cited is an indicator of their authors' fame. Or put another way, are already famous authors more often cited than those who are less famous? Can the independent effects of being published in "famous" journals be detected, that is, those with high "impact factors?" The answer to these questions is yes according to Richard S. J. Tol—at least among the most prolific economists; earlier fame, as gauged by numerous citations, does indeed have an independent effect on later citations, a finding entirely in line with the premise of the Matthew Effect if one is to accept Tol's using prior citations as a reasonable indicator of fame. He not only reports that the Matthew Effect can be observed in the citation histories of papers (famous papers attract more than the expected number of citations) but he also notes that it explains the distributions of citations among authors:

> New citations are partly due to differences in fame […]. Oft-cited papers are cited more often and oft-cited authors are cited more often. The two effects strengthen one another. This implies that there are 'increasing returns to scale' in influence and that Merton's [1968] Matthew effect is real and can be found in data […].
>
> The fact that fame breeds fame implies that number of citations alone is not a good criterion for quality. […] Highly cited authors are high quality researchers. However, the Matthew effect as defined and measured here implies that the relationship between citation numbers and quality is different at the top end of the distribution than at the bottom end.[63]

440–460 and M. Bonitz, "Ten Years [of the] Matthew Effect for Countries," *Scientometrics* 64 (2005): 35–79.

63 "It is a hard journey from being an unknown upstart to a famous economist. Famous elders hog the limelight, and their share of the attention is only partly due to superior quality; some are rather famous for being famous. However, it is not uphill all of the way; it is uphill only for most of the way. At a certain point, one crosses a threshold and is then propelled to fame" (Tol 2008, 423). Tol rules out the effects of self-citation and notes that one cannot alone make oneself highly cited.

Another test of the validity of the Matthew Effect draws on an ingenious if not altogether believable research design. Aware that such a test ideally calls for comparing responses to the same or equal contributions, Kieran Healy hit upon a natural experiment for determining whether papers with the *same* content would earn the same number of citations if they were published in journals of varying influence (that is, having different impact factors). Locating 4532 pairs of duplicate papers published in different journals, not simply papers with the same content but papers with the same authors, titles, and number of references in the Web of Science database, (itself a surprising and counter-intuitive finding), Healy reports that the journal's impact factor "strongly" affects the number of citations identical papers receive and thus demonstrates the operation of the Matthew Effect. "Papers published in high impact journals obtain, on average, twice as much [sic] citations as their identical counterparts published in journals with lower impact factors. The intrinsic value of a paper is thus not the only reason a given paper gets cited or not" (Healy 2008, 1). In this instance, the Matthew Effect is measured by and equated with publication in journals with high impact factors since the venue of publication is extraneous to the "value" of the paper. It would be necessary of course to rule out the independent effects of visibility of papers published in high impact journals in order to claim that this is a definitive test of the Matthew Effect. If the papers claimed to be equivalent are truly so, Healy's research design comes close to being a test of whether unequal credit (or citation) is accorded for qualitatively equal contributions. Along similar analytical lines, a study examining citations to papers published in "the well-regarded *Annual Reviews* series" notes that publishing in serials that are themselves considered authoritative confers authority both on the papers that appear there and on their authors, both of which "bask […] in the halo of R. K. Merton's Matthew Effect" (Brown 2004). [64]

[64] *Annual Review* publications without URLs are more often cited than those containing them. Even so, Brown finds that the number of citations to web publications in *Annual Reviews* is worrisome given the authority of papers appearing in the series owing to the often-fleeting accessibility of web materials.

4.7 The Matthew Effect and the Reception of Ideas

If the Matthew Effect was intended to account for the further success of already successful individuals, it may also seem applicable in accounting for the success of ideas propounded by successful advocates. Ideas have careers or biographies not unlike those of individuals. Thus the Matthew Effect has been put to use in explaining the adoption in psychiatry of "scientific classification," and the rejection of "Dorothy S. Thomas' contribution to the "Thomas Theorem." In the former, the successful revival of using medical or neurobiological criteria in diagnosing psychiatric illnesses and the rejection of other plausible diagnostic systems is attributed in part to the Matthew Effect (because the advocates of using such criteria were particularly distinguished physicians) (Blashfield 1982),[65] as well as to the power of an "invisible college"[66] and the prestige of the journal in which the relevant papers were published. None of these explanations are related to the substance or quality of the ideas proposed as several of those involved in the episode observed. Judging from the comments of one of them, being said to benefit from the Matthew Effect is seen as criticism rather than praise.

Being accused of perpetrating a Matthew Effect is as bad or worse than benefitting from one, especially if the accused is himself the originator of the Matthew Effect. The story is, as such stories are, complicated. It concerns the claim that Robert Merton, in crediting W. I. Thomas for the "Thomas Theorem,"[67] deprived Dorothy S. Thomas of

[65] This paper was scathingly criticized by Samuel Guze, one of the principals in the events, who declared that benefiting from the Matthew Effect and the other "social factors" undermines the validity of their ideas and the importance of their contribution. As Guze writes, "it is important to distinguish between the message and the messenger" (Guze 1982, 7).

[66] Thus following Derek J. de Solla, Price's adoption of the 17th-century term "the invisible college," originally used for those who would form the Royal Society of London. Price resuscitated this evocative term to describe loose groupings of contemporary scientists whose shared research interests lead them to communicate with one another more often than they do with local colleagues even though they are geographically dispersed (Price 1963).

[67] The Thomas Theorem, as Merton so named it, asserts that: "If men define situations as real, they are real in their consequences" (Thomas and Thomas 1928).

credit for her part in its development. In recapitulating the history of the Thomas Theorem, Robert S. Smith (1999) asserted that Merton could not have known which of the two Thomas co-authors of *The Child in America* had been responsible for the famous phrase[68] and thus should have credited them both. Not doing so, Smith said, denied the younger and not yet distinguished Dorothy Thomas of due credit and was sexist to boot. W. I. Thomas was of course very distinguished at the time of publication and 20 years his wife's senior. Merton clearly would not let the accusation stand. Using the occasion of his reply to elaborate on such emblematic scientific practices as "establishing the phenomenon," eponymy, the serial diffusion of ideas, institutionalized sexism, partial citation, and OBI, Merton countered with his own reconstruction of the history of his attributing the Thomas Theorem to W. I. Thomas, ultimately solidifying his case with what he called his "smoking gun": a letter from Dorothy Thomas written in 1973, confirming what she had told him years before that she had no part in the formulation of the Thomas theorem. In that letter, she wrote, "In regard to *The Child in America* [...]. The statistical portions were mine [...]. The concept of "defining the situation" was strictly W. I.'s."[69] So much for the misallocation of credit and sexism. On the assumptions underlying Matthew Effects in general and in this specific case, Merton wrote:

> Absent [...] detailed information, fellow scientists and scholars are evidently inclined to think it 'reasonable' that the more accomplished collaborator with a history of major contributions [...] has probably originated a joint work or contributed more to it—unless there is compelling evidence to the contrary. This, even though such a probabilistic inference of course tells us next to nothing about the particular case with certainty. However, in the case of the Thomas theorem, the compelling evidence is there and this is not to the contrary. (Merton R. K. 1995b, 418)

Scientists and scholars place great weight on getting credit equitably allocated—this holds both for those who accuse others of failing to do

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[68] It was in Merton's paper, "The Self-Fulfilling Prophecy" (1948), that he termed the now well-known phrase, the "Thomas Theorem," drawing on W. I. Thomas and Dorothy S. Thomas (1928).

[69] That this statement came to be called the Thomas Theorem was Merton's doing and so was its use in subsequent citations, quasi-citations and miscitations to the Theorem (R. K. Merton 1995b, 404).

so and those accused of benefiting from it. The Matthew Effect describes an important if often unwitting departure from the norm that credit should go to those who are due it and the heat that claims of its occurrence generates suggests that it is a violation of a principle many believe is important.

4.8 Are there Matthew Effects in Economics?

This is both a question and a puzzle. If there are Matthew Effects in economics, there is little indication they are of interest.[70] This is so despite economists' abiding concern with disparities in earnings and competitive markets and the numerous instances in which the same level of performance is unequally compensated.[71] Several important contributions to economics have a passing resemblance to the Matthew Effect. These include the Heckman and Borjas (1980) analysis of the reasons for prior spells of unemployment and their duration being correlated with current unemployment, Sherwin Rosen's (1981) still influential work on "the economics of superstars," and Robert Frank's and Philip Cook's (1996) analysis of "winner take all markets,"[72] in

[70] The few papers that use the term-and-concept are limited to bibliometrics studies of economics as a field and economists' publications. It seems unlikely that the concept the Matthew Effect describes is used frequently by economists but that they avoid using the term in their publications. To check this assumption, a search was done for the Matthew Effect in the cluster of 92 economics journals in JSTOR. This search turned up just 28 appearances of the term in the entire cluster; three of these are in a journal which is in the economics cluster but not of it, *Annals of the American Academy of Political and Science* and two of these papers are by sociologists. Two listings are duplicates. Where the Matthew Effect is used, as one would expect, is in management science and industrial organization (Hunt and Blair 1987; Tang 1996; Veugelers and Kesteloot 1996). In addition, a paper in the *European Physical Journal B* examines the distribution of accumulated wealth, clearly an economic subject but authored by three engineers, a physicist, and two mathematicians (Hu et. al. 2006).

[71] Searching for the term in the literature on the wages paid to men and women and to blacks and white failed to turn up a single mention. Perhaps the search should have been for the "Matilda Effect."

[72] Both Rosen and Frank and Cook seek to explain vast differences in compensation which are typical of markets in which technology makes it possible for many to consume the achievements of the "best" performers even when the differences in quality between the best and the rest are very

which top salaries in certain economic sectors in the United States have reached astounding levels and levels of economic inequality have grown along with them. Rather than being examples of Matthew Effects, however, these are more appropriately designated as outcomes of feedback effects and cumulative advantage and disadvantage. Seeking to account for the near-absence of the Matthew Effect in the economic literature analysis is an exercise in counter-factual analysis; it is treacherous to try to explain why something did not happen. This said, in the case of economics the reasons may lie in economists' preference for rational explanations of behavior; the Matthew Effect may involve rational behavior but its results are far from rational.

4.9 Lessons Learned

First, familiar as many of these examples may be, there are nonetheless real difficulties in demonstrating empirically that Matthew Effects have indeed occurred and produced definable results. Other variables are very likely to be associated with misallocation of rewards and contribute to them but resist direct observation. Demonstrating decisively that Matthew Effects cause the outcomes that interest us is not easy and calls for the use of sophisticated models that are rarely applied in the numerous inquiries I have reviewed. A second lesson to be drawn is that Matthew Effects are not limited to science and scholarship but their frequency and pervasiveness are more limited than the process of cumulative advantage and disadvantage. A third lesson is that the two concepts are often considered to be interchangeable (both result in the "rich becoming richer") and this has produced the blurring of their meanings and at the extreme, their being taken to be equivalent. Further, when terms-and-concepts describe general phenomena and appear to provide theoretical explanations for their occurrence, they become attractive to those seeking to increase the theoretical significance of their research whether or not such terms truly apply. The final lesson, quite different, from the rest, is germane to sociological

small. Rosen is persuaded that this pattern is benign, in that the public has access to the work of the best performers even if it leads to great wage inequalities while Frank and Cook are persuaded that such markets have large perverse effects, and lead to waste and overcrowding of limited markets. Frank and Cook cite Merton's 1968 Matthew Effect paper in their analysis of competition among universities.

semantics; namely that as a term-and-concept continues to be used and spreads beyond its place of origin, it undergoes changes in meaning and indeed acquires new ones not altogether connected with the one first intended. Such terms can at once be over-generalized and excessively limited as well as misapplied and misunderstood. Terms-and-concepts that have an intrinsic appeal acquire lives of their own, far different from those envisioned by their originators.

5. Conclusion

The Matthew Effect is a prototypical Mertonian contribution. It exemplifies his eye for a telling social phenomenon which despite its generality goes unnoticed by others, his skills at laying out its distinctive properties, his ability to recognize and to elucidate the mechanisms that bring it about and perpetuate it and then, not to be denigrated, his skill at inventing evocative terms that make the phenomenon visible, comprehensible, and usable. This constellation of attributes appears in his work on Self-Fulfilling Prophecies, Unanticipated Consequences, Manifest and Latent Functions, Local and Cosmopolitan Influentials, Opportunity Structures, Socially-Expected Durations, and Obliteration by Incorporation, to mention only a few instances of the general pattern. In each of them, Merton's analytic stamp is clear as is his faculty for neology.

Like these conceptual contributions, the Matthew Effect and its kin, Cumulative Advantage, have not yet lost their intellectual interest nor the opportunities for empirical research they offer been exhausted. The phenomena they describe are powerful forces in social life that are not to be disregarded. They exemplify Robert Merton's instinct for the theoretical and semantic jugular.

Appendix 1: Some Analytic Examinations of the Matthew Effect and its Implications

Matthew S. Bothner, Joel Podolny and Edward Bishop Smith, "Organizing Contexts for Status: The Matthew Effect Versus the Mark Effect," Summarized in the Best Papers in *The Academy of Management Proceedings*, Volume 2009. Annual Meeting Proceedings. Forthcoming in *Management Science* 2010.

Roberto Festa, "'For unto every one that hath shall be given,' Matthew Properties for incremental confirmation," *Synthese*. Published online, 25 November 2009.

Jack A. Goldstone, "A Deductive Explanation of the Matthew Effect in Science," *Social Studies of Science* 9; 1979:385–91.

Michael Strevens, "The Role of the Matthew Effect in Science," *Studies in the History and Philosophy of Science* 37; 2006: 159–170.

Appendix Table 1
Number of Google Results: Selected Terms-and-Concepts associated with Talcott Parsons and Pierre Bourdieu.
February 7, 2010

Term-and-Concept	**Number of Results/ No Authorial Specification**	**Number of Results Term + Robert K. Merton**
Talcott Parsons		
Social System	3,030,000	98,000
General Theory of Action	2,800,000	395,000
System Theory	922,000	7,700
Sick Role	141,000	55,700
Pattern Variables	102,000	16,100
Pierre Bourdieu		
Habitus	3,370,000	1,570,000
Cultural Production	1,490,000	26,000
Cultural Capital	844,000	39,300
Symbolic Violence	124,000	24,700
Language and Symbolic Power	29,400	58,400

Repetition with Variation: A Mertonian Inquiry into a Lost Mertonian Concept [1]

Charles Camic

Throughout the chapters of this volume, my fellow contributors examine a range of concepts that originate in the work of Robert Merton and continue to circulate in contemporary sociological theory and research, but which have long-since grown detached from Merton's own name, in a clear instance of the intellectual process that Merton's himself referred to as "obliteration by incorporation." In this chapter, I too am concerned with a valuable Mertonian concept, albeit *not* one that presently commands any currency in sociology or any place in the growing scholarship on Merton's work. This is the concept of "repetition with variation."

Mention of this concept is unlikely to strike a responsive chord among sociologists, even those closely familiar with the Mertonian oeuvre. A search of ten leading sociological journals—*Acta Sociologica,* the *American Journal of Sociology*, the *American Sociological Review,* the *Annual Review of Sociology*, the *British Journal of Sociology, Contemporary Sociology,* the *European Sociological Review, Social Forces, Sociological Theory,* and *Theory and Society*—reveals, for example, not a single reference *ever* to "repetition with variation." Likewise in the scholarly literature on Merton's writings, the concept is entirely absent from the monographs and edited volumes that deal with Mertonian ideas and their development (see Crothers 1987; Clark et al. 1990; Coser 1975; Mongardini and Tabboni 1998; Sztompka 1986, 1996).

In the spirit of Merton's remark that "the resurrection of a term fallen into disuse is an integral part of the development" of the social

[1] I want to thank my fellow participants in the Budapest seminar for their remarks on an earlier version of this chapter and especially to acknowledge Harriet Zuckerman for her very helpful advice (both at the seminar and subsequently by email). I also thank Carey Seal for answering a query about the field of classics.

sciences (Merton and Barber 2004, 67), one purpose of this chapter is to bring to light the place of the concept of "repetition with variation" in Robert Merton's work. In pursuing this purpose, however, I have a second objective in view as well. This is to apply to the example of "repetition with variation" the distinctively *Mertonian style* of conceptual analysis that Merton advocated in the ambitious program for "sociological semantics" that he pioneered in a series of little-discussed writings that began appearing in the mid-1960s with *On the Shoulders of Giants* (Merton 1965) and became more pronounced during the last decade of his life (Merton 1995, 1995b, 1997, 2004; see also Merton and Barber 2004; Merton, Sills, and Stigler 1984; Sills and Merton 1990, 1992a, 1992b).

As Zuckerman (2010) has outlined this novel program and has delineated its tenets, Merton's sociological semantics centers sociological inquiry on "words, phrases, aphorisms, slogans and other linguistic forms," and it then undertakes to study the linguistic terms in question from two sides: first, by examining the focal term's *social origins*, identifying which social groups used the term, along with when, where, and how they did so; and second, by charting the term's *paths of diffusion*, i.e., its reception and subsequent evolution, changing meanings, dissemination or disappearance (Zuckerman 2010, 1, 6; also Camic 2010). Merton's preferred manner of addressing these questions was by means of what he described as *historical case studies* of "culturally strategic words" and other expressions (Merton 1977, 77; 1982, 263, 1995, 4; 1997, 225).

Taking these late Merton writings as my template, this chapter is a small effort to advance the research program of sociological semantics by an abbreviated historical case study of Merton's own concept of "repetition with variation." I divide my analysis into four parts. In Part 1, I introduce the concept of "repetition with variation" as it appears in Merton's early work. In Part 2, I back up chronologically to discuss some of the earlier sources, or originating contexts, that informed Merton's use of the concept. In Part 3, I move forward in time briefly to examine the subsequent diffusion of "repetition with variation" and the place of the concept in the literature of sociology and the social sciences and humanities more broadly. In Part 4, I speculate on the reasons for this pattern of diffusion and, in so doing, return to the corpus of Merton's work to consider the traces therein of the idea of repetition with variation.[2]

In organizing the chapter in this way, I would acknowledge that I am departing from the Mertonian semantic program with respect to manner of presentation. When Merton reported the results of his own semantic case studies, his tendency was to adopt the chronologically fractured, "non-linear, advancing-by-doubling-back Shandean Method of composition" (Merton 1985, xix), which 18th-century British author Laurence Sterne famously used in *Tristram Shandy*, the nine-volume comic novel that Merton greatly admired. Entirely natural for writers in the league of Sterne and Merton, however, the Shandean Method was simply beyond the literary reach of the present author.

1

The concept of "repetition with variation" is one that Merton introduces, elucidates, and applies in his much-neglected 1946 book, *Mass Persuasion: The Social Psychology of a War Bond Drive*. This short monograph is an empirical study of a one-day event that occurred in the United States during the course of World War II. Merton describes the event as follows:

> September 21, 1943 was War Bond Day for the Columbia Broadcast System. During a span of eighteen hours—from eight o'clock that morning until two the next morning—a radio star named Kate Smith spoke for a minute or two at repeated intervals […]. On sixty-five distinct occasions in the course of the day, she begged, cajoled, demanded that her listeners buy [U.S.] war bonds. Within the narrow borders of her brief messages, Smith managed to touch a variety of themes enshrined in American culture. She talked of neighbor boys from American towns and villages, now facing danger and death in other lands […]. She told dramatic tales of generosity and sacrifice by soldier and civilian alike […]. She invoked themes of love and hate, of large hopes and desperate fears, or honor and shame. Apparently, there was nothing here of a cut-and-dry radio script. This was presented as a personal message, iterated and reiterated, [and] by the end of [the day Smith broke all] previous bond-selling records, [amassing] thirty-nine million dollars of bond pledges (1946, 2–3).

[2] In this paragraph, as throughout the chapter, I place quotation marks around "repetition with variation" when my principal focus is the concept, and I omit the quotations when referring primarily either to the idea or the process itself.

Most immediately, the purpose of Merton's book is to explain the success of Smith's bond drive, conceptualized as "an extraordinary instance of mass persuasion" (1946, 3); and in constructing his analysis Merton characteristically eschews single-cause explanations and takes account of a wide range of factors. These factors include not only "the manifest content of [Smith's] radio appeals," but "a larger configuration in which the audience's images of Smith, the class structure of our society, the cultural standards of distinct strata of the population, and socially induced expectations, feelings, tensions were all intricately involved" (1946, 9).

At the outset of his account, however, Merton pauses to draw particular attention to what he sees as the distinctive and highly consequential "temporal pattern" of Smith's marathon (1946, 21–43). "Hearing Smith ten times in an evening does not produce a [...] response which is merely ten distinct reactions to a single program. On the contrary, it constitutes a new and different type of experience [...]. The repeated pleas merge into a cumulative whole, [which differs] from an aggregate of separate stimuli" (1946, 21). Having set the matter in this light, Merton quickly perceives that he has broached a sociological problem of far more general significance than the success of Smith's bond drive. He phrases this larger problem incisively: "*why does a stimulus applied successive times have an effect the* n*th time which it did not have the first?*" (1946, 34 [emphasis added]). In posing this question, Merton comments on the tendency of social scientists of his time[3] either simply to overlook the problem or to suffice with tired "analogies [such] as that of the drip of water wearing away a stone," despite the failure of these notions to illuminate the complex social-psychological processes involved not only in episodes of mass audience persuasion (1946, 34), but, by extension, in the larger family of social situations where, after the "*n*th time," social actors uphold beliefs or engage in forms of conduct which they did not adopt after their first encounter with those same beliefs or recommended actions.

According to Merton, one line of thought would attribute this fundamental difference between the first and the *n*th times to the impact of repetition *per se*. Merton associates this viewpoint with some highly suspect sources: "Hitler is authority [...] for the observation that

[3] Merton's observation would apply to sociologists 60 years later as well.

even a great lie will be believed if it is asserted often enough; experienced advertisers do not expect results from a slogan until it has been frequently repeated; the child seems aware that his mother who says 'No' at first may relent if he teases long enough" (1946, 34). What dictators, advertisers, and persistent children do not grasp, however, is "that simple repetition is not always effective" (1946, 34). To the contrary, repetition can be counterproductive in terms of inducing new opinions and actions, as Merton elaborates:

> It is widely recognized that simple repetition is not always effective. Very often it induces boredom, surfeit or active irritation, or at least a kind of defensive isolation. Thus people who live with the constant ticking of a noisy clock, the crowing of roosters, the rumble of a noisy furnace, or even the roar of passing trains, become so accommodated to these sounds that they pay no attention to them. What develops in such instances is a repeated pattern of ignoring the stimulus, and this pattern can be observed in the reaction of some radio listeners to the flow of routine appeals to buy bonds. The multiplication of spot announcements, instead of stimulating purchases, may lead to satiety and "radio deafness" (1946, 34–35).

That Smith's bond drive avoided these negative outcomes was the result, Merton insists, of the particular circumstance that her "*marathon did not consist of simple repetitions*, [but rather] utilized the classic formula of diversity within unity," so that each of her 65 successive appeals "contained a new instance, a new perspective, or struck a new note" on the need for listeners to buy war bonds (1946, 36 [emphasis in original]).

Hastening from this empirical observation to an identification of the general process that it reveals, Merton immediately brings to the fore the concept "repetition with variation," which he sees the Smith marathon as illustrating. In his words: "*Repetition with variation* of appeals proved an important element in the process of persuasion. Smith's broadcasts aimed at one and the same goal, but each was unique. The effect, therefore, was not one of mere reiteration" (1946, 36 [emphasis in original]).

Expanding the point, Merton characterizes "flexible repetition" as the mechanism that "enhanced the likelihood of persuasion" in this historical instance (1946, 37), conceptualizing the process of repetition with variation as exactly specifying the condition under which—and thus answering his question as to why—social stimuli succeed in shaping actors' opinions and actions after the "*n*th time." In Merton's succinct formulation: it is the temporal "cumulation of diverse appeals [that proves] decisive" (1946, 37).

2

Among the lessons of Merton's late writings on sociological semantics is the finding that concepts rarely bolt from the blue. Almost invariably, they enfold complex intellectual histories that are simultaneously social histories—histories of their emergence and varied patterns of usage within different social groups. Unsurprisingly, the same proves to be the case in regard to "repetition with variation," with the proviso that the particular social groups involved in this concept's history consist mainly of specialized communities of intellectuals, inside and outside the academy. Long before Merton took the concept up sociologically in *Mass Persuasion*, "repetition with variation" had a rich history beyond sociology, and a full account of that history could easily expand to monograph length. For the specific purpose of understanding Merton's own distinctive use of the concept, however, a more selective analysis may serve to identify a few of the more immediate and salient socio-intellectual contexts in which "repetition with variation"—and kindred notions—circulated in the period prior to Merton's book.

It is beyond Merton's task in *Mass Persuasion* to furnish this contextualization himself, although he is careful, within the text, to disclaim that he himself is inventing the concept of "repetition with variation." This he does by referencing British experimental psychologist Sir Frederic Bartlett's respected 1940 treatise *Political Propaganda* and quoting Bartlett's statement that "it is not sheer repetition that is influential [among propaganda methods], but repetition with variations" (1940, 69, as cited by Merton 1946, 36, n. 10)—one of several observations to the same effect that Bartlett offered in the course of singling out "repetition with well-considered variations" as a hallmark of democratic (versus Nazi) propaganda (1940, 70). Typically with Merton, however, any one reference turns out to be only a very small sampler from wider discourses with which he was familiar, and so it is in this instance as well. Indeed, behind Bartlett's book, lay at least four pertinent, broader socio-intellectual contexts with which Merton had become well-acquainted by the time he used "repetition with variation" in *Mass Persuasion*.

The first of these contexts was local and closely connected to the backstory of how Merton came to be involved in a study of Kate Smith's war bond drive within a few years of joining the sociology faculty of Columbia University in the fall of 1941.[4] This backstory has been uncovered by Simonson (2004), who has called attention to Merton's association with Paul Lazarsfeld and participation in some of the research projects that Lazarsfeld and his associates were conducting at Columbia's Office of Radio Research (of which Merton became Associate Director in 1942). These projects included commercial research on the impact on audiences of different kinds of radio programming and marketing campaigns, as well as several War World II–related "propaganda studies" for the U.S. Office of War Information—the study of Smith's radio marathon falling among the later (Simonson 2004, xix).

During these years, a recurring theme of Lazarsfeld's studies was the question of why radio and other forms of mass communication produce the audience effects that they have, and in addressing this issue Lazarsfeld was wont to observe the role of repetition:

> A […] characteristic of radio is that it continues in time. This means that a series of programs may become part of the daily or weekly habit patterns of the listeners, that cumulative effects can be build up over long or short periods. But it also means that it is liable to surfeit. It may be true that "if you hear a thing often enough you will come to believe it," but probably it is equally true that if you hear a thing too much you may not pay any attention to it at all after a time. Just *where repetition ceases to be effective, just where saturation points are reached, is still a problem which has to be faced* anew for each kind of program or message. (Fiske and Lazarsfeld 1945, 57 [emphasis added])

The "problem" that Lazarsfeld flags here is precisely the one Merton takes up the following year in *Mass Persuasion* and, pushing much further, therein proceeds actually to solve—and to solve not by appeal to the incidental particulars of "each kind of program message," but in a more comprehensive manner with his general distinction between

[4] In starting with the local, I follow the example of Merton's writings on sociological semantics about the significance of "socio-cognitive *micro*-environments" for men and women of ideas (see Merton 1977, 1995, 2004).

"simple repetition," which results in "surfeit," and "repetition with variation," which produces message effectiveness.

This was a distinction that Lazarsfeld himself had evidently not grasped as of 1945. This was so although a more narrowly-couched and clumsy version of the distinction had appeared just a few years earlier in a book that Lazarsfeld edited (with Frank Stanton of CBS) on radio research. In one of the chapters of the book, one of Lazarsfeld's assistants at the Office of Radio Research—the future luminary T. W. Adorno, as it happened—explored the requirements for effective radio symphonies. As he did so, Adorno—tapping into long traditions of commentary on "theme and variations" in musical composition—wrote of the first movement of Beethoven's Fifth Symphony:

> Throughout the movement [there] remains clearly recognizable [...] the same motif; its rhythm is vigorously maintained. Yet there is no mere repetition, but development: the melodic content of the basic rhythm, that is to say the intervals which constitute it, change perpetually [...]. It is this unity within the manifold as well as this manifoldness within that unity which constitute the antiphonic work [...]. Only if the motif can develop from the restrained pianissimo to the striking yet affirming fortissimo, is it actually revealed as the "cell" which represents the whole, [and] only within the tension of such a gradation does its repetition become more than repetition. (Adorno 1941, 121–23)

Whether Merton was directly familiar with this particular thread in Adorno's analysis, he was certainly aware that repetition and its vicissitudes were subjects of active discussion at the Columbia's Office of Radio Research, and he proved equally alert to a second relevant intellectual context as well. This was the extensive contemporary social-psychological literature on the interrelated topics of persuasion, suggestion, propaganda, mass communication, public opinion, and advertising.[5] The writings of Lazarsfeld and his associates actually formed part of this fast-growing literature, as did the work of two other prominent authorities in the local intellectual environment, Columbia psychologist Albert Poffenberger and Barnard psychologist Harry Hollingworth. Poffenberger's major text, *Psychology of Advertising*, for example, discussed at length the "cumulative effect of repetition" and

[5] This literature overlapped with the contemporary literature on the psychology of aesthetics, where "repetition with variation" also featured prominently (see, esp., Chandler 1934).

the special effectiveness of "repetition with variation" (1932, 220–21), while Hollingworth's *Psychology of Audience* marshaled experimental evidence that "repetition of advertising appeals is twice as effective when the forms, style, and expression is varied" (1935, 144).

By no means, however, were these works—or this idea—unique to the Columbia milieu, and Merton's close familiarity with this current in the contemporary literature of social psychology actually antedated his arrival at Columbia by a decade. As Simonson (2010) has shown, Merton took an intensive course on the social psychology of public opinion and propaganda when he was an undergraduate at Temple College in 1931; continued this interest while in graduate school at Harvard during the 1930s and a member of the "Parsons Sociological Group," which dealt explicitly with the topics of persuasion and propaganda; and taught a social psychology course on these same subjects himself at Tulane in 1940. This encounter occurred in a period when major names in this social psychological literature included, on the American side, William James, Boris Sidis, Henry Foster Adams, and Charles Bird and, on the European side, Gustav LeBon, Gabriel Tarde, and Sigmund Freud; and throughout the writings of these figures, statements like the following were extremely commonplace:

> Repetition of suggestions facilitates their consideration and acceptance. The early work of Sidis yields experimental substantiation, while emphasizing the caution that *repetition with variation of form is most effective* if the suggestion does not appear obtrusively [...]. Speakers who repeat their suggestions frequently and with variety are merely utilizing a well-known principle of learning. It is essential to integrate suggestions into as many attitudinal molds as possible, particularly when individuals are being prepared for delayed response. (Bird 1940, 279 [emphasis added])

Tellingly, the source that Merton expressly acknowledges in *Mass Persuasion* when he introduces "repetition with variation"—Bartlett's *Political Propaganda* (1940)—was a late representative of this tradition in social psychology.[6]

If this tradition was a substantial one, the third context to which Merton was oriented was still more expansive. This was the literature of the Western humanist tradition, including ancient and early modern

[6] This is a tradition that Merton was still occupied with years later in his "Introduction" to LeBon's *The Crowd* (Merton 1960).

texts on rhetoric as well as works of English fiction. This is not a literature that sociologists generally associate with Merton. Yet, as Simonson has recently pointed out, not only were writings on rhetoric "a 'very important' resource for [Merton] over the years";[7] but prior to *Mass Persuasion* he was already deeply "knowledgeable of the terms and texts of the European rhetorical tradition," going so far to include in the book (as part of his analysis of modern propaganda) "long quotations from classic texts in the rhetorical tradition—Plato's *Gorgias* and *Phaedrus*, Aristotle's *Rhetoric*, and Thomas Hobbes' *Art of Rhetorick*" (2010, 1–3).

What bears notice is that, throughout this tradition, "repetition with variation" was a widely discussed rhetorical technique, as Merton would have known from the original sources as well as from later scholarly commentaries, among them the work of his friend, the literary theorist Kenneth Burke, who—at the same time Merton was composing *Mass Persuasion*—was writing *A Rhetoric of Motives*, which classified among the "traditional principles of rhetoric," the "several kinds of repetition with variation" that Cicero differentiated (1950, 68). Not only this, but repetition with variation was also a frequently used *compositional practice* among ancient authors of both prose and poetry (Homer, Sophocles, Herodotus, Lucretius, Ovid, Virgil),[8] and Merton was a skillful reader of Latin (if not of Greek) since his high school days.[9]

Looking beyond the ancients, repetition with variation was also a highly favored literary device in England during the late medieval and early modern period to which Merton was attracted during his dissertation research on science and religion in 17th-century England (Merton 1970 [1936]) and whose literature interested him greatly throughout his lifetime (see esp. Merton 1965). As he read literary works from this period, Merton would have observed the technique on clear display in the writings of any number of authors, including Chaucer, Shakespeare, and, by the 18th century, none other than Laurence Sterne in

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[7] With the words "very important," Simonson quotes Merton in a conversation between the two of them.

[8] I base this statement on many articles of classical scholarship that I have located and examined via Jstor, but which do not warrant individual citations for the purposes of the present volume.

[9] I owe this information to Harriet Zuckerman.

the many volumes of *Tristram Shandy* itself (Bamford and Knowles 2008; Cook 1960; Fisk 2000; Richetti 1998).[10]

Merton's engagement with the literature of Western humanism went hand in hand, moreover, with his immersion in a fourth context: that of modern science. The foregoing discussion of "repetition with variation" has perhaps reminded readers of a process well-established in the experimental natural sciences under the name of "replication." The association is a straightforward one for the reason that scientists ordinarily view replication as the activity of confirming the findings of a particular experiment by *repeating* that experiment under *varying* laboratory conditions and then obtaining comparable results (for discussion of this view of replication, see Collins 1985). If we follow the *Oxford English Dictionary*, however, "replication," although it is a term that goes back several centuries, only became the principal expression for the process of experimental confirmation in the course of the 20th century. Previously, "replication" shared the ground with other terms, one of which was the notion of repetition with variation.

For example, in the writings of the laboratory scientists with whom Merton was closest during his time at Harvard (and would afterwards hold in high esteem), the physiologist Walter B. Cannon and the biochemist Laurence J. Henderson, there appears to be no mention of "replication." Instead, both researchers describe the process of scientific verification explicitly as the "repetition" of an experiment with different materials or under conditions that vary in other respects, as when Henderson touts an experimental design that has the "advantage of easy repetition" with different kinds of equipment (Richards and Henderson 1905, 11; see also Henderson 1913, 250) or when Cannon writes: "Our first procedure was a repetition of the Bohm and Hoffman experiments, [but] freed from the factor of pain" which subjects in earlier experiments had undergone (1922, 70; see also Cannon *et al*. 1924, 48). Through his extensive reading of the scientific literature of the early 20th century, Merton could scarcely have avoided frequent

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[10] I exclude from this discussion of Western humanism all of the tradition's more exclusively philosophical writings, although repetition in its different varieties received attention here as well, most notably perhaps in the work of Kierkegaard and Nietzsche (Gendron 2008). Merton's relationship to this strand of philosophical work remains to be examined.

encounters with this conception of experiments as repetitions with variations.

Even so, his scientific reading ranged much wider and, in the course of his dissertation research, extended backward in time to the dawn of modern science in 17th-century England—and to the writings of figures like natural philosopher Robert Boyle and scientific propagandists Francis Bacon and Thomas Sprat. This is significant for it is in these very writings that one actually finds some of the earliest English-language references to repetition with variation. Describing his own scientific investigations, for instance, Boyle spoke pointedly of the importance of "repetition"—of repeating experiments under varying conditions—recounting (*inter alia*) how "one repetition of [an] experiment" would produce one result, while another repetition might produce a different result (Boyle 1725, vol. 2, 286, vol. 3, 133). For his part, Bacon set forth the principle that:

> An experiment is produced in two ways: viz., by repetition and extension, the experiment being either repeated or urged to a more subtitle things, [by means of] the variation, production, translation, inversion, compulsion, application, conjunction, or any other manner of diversifying, [as] when a known experiment, having rested in one substance, is tried in another of like kind. (1605, 140–141).

Sprat's *History of the Royal-Society of London*—a text foundational to Merton's dissertation—inscribed this understanding of experimentation into its depiction of the Royal Society as a public setting for the "repetition" of experiments and for the recording of "variation" in their results (1667, 99, 197, 222, 244, 245, 254)—a theme that Sprat summarized for his audience in his statement: "Of the exactness, variation, and [...] repetition of experiments, I have discoursed" (1667, 215).

I do not intend the four contexts just described as a complete listing of the sources in which Merton would have encountered the concept of "repetition with variation" before it surfaced in *Mass Persuasion*. Neither do I wish to claim that he drew self-consciously on these literatures when he wrote—except, that is, when citing Bartlett's book on political propaganda—nor that his use of the concept was intellectually derivative from the examples that these sources provided. To interpret the preceding account in any of these ways would be to misconceive the purpose of carrying out studies in sociological semantics of the kind that Merton proposed in his late work.

Although it is (as noted above) among the central tasks of sociological semantics to examine the "social origins" of the particular words, phrases, slogans, etc., that have become the foci of investigation, one must beware of conflating *origins* in this sense with *causes* in the more mechanistic sense in which sociologists sometime conceive of causes. To find that thinker A (here, Merton) made use of concept X ("repetition with variation") which had previously appeared in context B (the Columbia Office of Radio Research), context C (contemporary social psychology), context D (Western humanism), and context E (modern science) is not to reductively attribute A's invocation of X to sources B, C, D, and E and, by this attribution, to bring analysis of that invocation to a close, pending the discovery of other antecedent sources. The significance for thinker A of antecedents B, C, D, and E lies not in the casual efficacy of those antecedents but rather *in their interpretive implications*: not in the brute fact that concept X previously appeared in contexts B, C, D, and E, but *in how these sources had differentially used the concept*. This is so because, insofar as one takes account of the specific ways in which B, C, D, and E used the concept, one's ability to hear thinker A becomes historically more attuned and progressively better prepared to appreciate how—in light of how B, C, D, and E invoked X—A, in turn, used the concept and in what, if any, signature ways.

Listening to Merton in *Mass Persuasion* against the background of other contexts where "repetition with variation" circulated throws into relief what is so distinctive—and so important—about his own usage. In Adorno's work at the Columbia Office of Radio Research, repetition with variation was a technique of effective musical composition; in the literatures of Western humanism, it was a device that rhetoricians, poets, and novelists employed to produce certain responses from their auditors and readers; in the literature of modern science, it was a process scientists carried out to confirm experimental results; and in the literature of contemporary social psychology, it was a practice that commercial advertisers, political propagandists, and other retailers of mass suggestions engaged in to sway the opinions of a population within the reach of their marketing campaigns. Perhaps because Merton had encountered all of these literatures and thus understood that repetition with variation was not confined to the realm either of musical composition, or of rhetoric, of prose and poetry, of scientific experimentation, or of mass marketing campaigns, but was, instead, a concept descriptive of a foundational process that occurred in all of these fields, his usage in *Mass Persuasion* is broader than those found in any of his sources, even the work of social psychologists, to which he is closest. For, while it is his analysis of the social psychology of Kate Smith's dramatic success at mass persuasion which prompts Merton to introduce the concept of "repetition with variation," from the first he presents this historical case not only as an instance of mass persuasion (though it is that), but—to repeat—as an example of a more encompassing pattern whereby "a stimulus applied successive times has an effect the *n*th time which it did not have the first." With this parsing, Merton effectively generalizes well beyond the several literatures in which "repetition with variation" had previously appeared to open a window (as observed above) onto a much wider range of social situations where, following multiple iterations, human beings accept ideas or undertake actions which they did not adopt or pursue after their initial exposure to those ideas or actions. In this sense, Merton's was a new usage, a distinctive sociological reformulation of a concept that the literatures of science, humanism, and social psychology had each used in narrower and more limited ways.

3

In his studies of sociological semantics, Merton regularly complements his investigations of the social origins of the linguistic expressions that he examines with inquiries into the subsequent diffusion of these expressions, and his own concept of "repetition with variation" merits consideration from this second angle as well. Significantly, however, when one attempts to follow the concept's reception—whether inside or outside sociology—very few traces emerge.

Merton's semantic writings teach that the "fate" of any word, phrase, concept, or other term is "contingent," entirely dependent on later historical developments, i.e., on "what other [users] make" at future points in time of the particular expression that is under investigation (1977, 107; 1985, xx). According to Merton, these subsequent "responses [can] vary [...] from out-and-out rejection of the word, to passive recognition of its existence, to active interest in [...] its continuing usage" (Merton and Barber 2004, 61), among multiple other possibilities, including that of "obliteration by incorporation" (Merton 1968a, 35), which several of my co-contributors to this volume have discussed in reference to some of Merton's own ideas.

With regard to Merton's concept of "repetition by variation," yet another possibility appears to have occurred. It is a development nearest to the scenario that Merton considers when he considers examples of "out-and-out rejection" and "resistance" (1977, 107–08), though these descriptors fail to fit the present case because they connote an element of active opposition that "repetition with variation" did not encounter. Rather, in publications over the course of the 60+ years since *Mass Persuasion* first appeared,[11] sociologists seem simply to have *neglected to notice* the concept, save for a very small number of exceptions from the one sub-area of sociology where the book found its most natural audience, the field of public opinion and mass communication research.

Of these rare exceptions, the most substantial was Joseph Klapper's *The Effects of Mass Communication* (1960), a comprehensive work of synthesis produced at Lazarsfeld's Columbia Bureau of Applied Social Research (the descendant of the Office of Radio Research) as part of a series which Lazarsfeld was co-editing on communication research. In the volume, "repetition with variation"—expressly associated with Merton (and Bartlett)—not only featured prominently among the communications-factors "related to the effectiveness of persuasion," but was characterized as well as a sociological principle that commanded solid empirical support, including the research of Lazarsfeld, Berelson, and Gaudet (1948) and Cartwright (1949) (Klapper 1960, 98, 119–120; see also Klapper 1949). [12] A decade later, sociologist Alan Booth, citing Klapper (though not Merton), reaffirmed the role of "repetition with variation" in shaping public opinion (1970–71, 605). These meager references, however, capture the extent to which sociologists used the concept; and, while uniformly positive, both Klapper and Booth continued to present "repetition with variation" in the manner of the social psychological literature that Merton had drawn upon, overlooking his expanded sociological reformation of the concept.

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[11] This statement is based on results of searches using the standard online engines such as Jstor and Google Books.

[12] The texts of Lazarsfeld, Berelson, and Gaudet (1948) and Cartwright (1949) appear to be less explicit about "repetition with variation" than Klapper suggests.

Looking beyond the discipline of sociology, the diffusion story was much the same. During the two decades that immediately followed the publication of *Mass Persuasion,* scholars from the neighboring fields of mass communication and communication arts occasionally mentioned "repetition with variation"—approvingly, although exclusively in its social-psychological aspect—when they discussed Merton's book (see McBurney *et al*. 1951, 155; Brembeck and Howell 1952, 177–78; Schramm 1954/1961, 320). Beyond these fleeting citations, however, their engagement with the concept as developed in Merton's writings (writings that for some time remained part of the literature of these fields [see Schramm and Roberts 1971]) seems to have come to a permanent end.

Even so, the history of "repetition with variation" itself was by no means finished. To accurately capture this history, however, one must be careful not to conflate the pattern in sociology (and neighboring fields) with developments elsewhere in the social sciences and the humanities. In terms of sociology, "repetition with variation" was a concept that had lacked recognition in the period before *Mass Persuasion*, and —except for the short-lived efforts of researchers like Klapper and Booth—that situation simply did not change in the decades following the book's publication. In a number of other academic fields, however, "repetition with variation"—the concept, as well as the idea— was (as section 2 documented) an established presence long before *Mass Persuasion* appeared, and it subsequently continued to command this position.

Indeed, over the course of the past century, each successive decade appears to have called forth more references to and more varied applications of "repetition with variation" than every preceding decade.[13] This pattern has been part of the rising tide of interest by scholars outside of sociology in the phenomenon of repetition—and of different types of repetition—in a wide range of contexts. According to one recent summary, for example:

> Current theory in linguistic pragmatics, in rhetoric, in cultural anthropology, and in literary theory, stresses the situated, interactive, rhetorical nature of [human] understanding. Various approaches to the ways understanding are constructed in the process of interaction—such as interactional sociolinguistics, epistemic rhetoric, ethnography of com-

[13] This statement, too, rests on information gleaned using standard online search engines like Jstore and Google Books.

> munication, functionalist poetics, and reader response theory—make reference to the crucial role of repetition in this process. Linguists have examined repetition in conversation and in language acquisition. Anthropologists and folklorists have studied the role of parallelism as a feature of performance and as a recurring characteristic of ritual forms of talk. Students of poetics discuss repetition as a key feature of artistic language. Literary theorists and rhetoricians discuss [...] the ways in which the authors of new texts make use of old texts. Clearly, anyone interested in a comprehensive theory of understanding must pay close attention to the mechanisms and functions of repetition. (Johnstone 1994, xi)

And this extensive enumeration says nothing of the significant role of repetition with variation in psychology and applied psychology, human development, educational theory and practice, philosophy, and multiple streams of discourse analysis (see Tannen 2007). To take just three examples: "repetition with variation" enters frequently in the books of literary critic Harold Bloom (e.g., 1986a, 1986b, 1986c, 1987, 1988, 1998), the work of historian and cultural theorist Dominick LaCapra (1983, 1985, 1989), and various currents of Continental philosophy, including—beneath the mantle of "différence et répétition"—the writings of Gilles Deleuze and Jacques Derrida (see Gendron 2008).

In none of these lines of scholarship, however, does "repetition with variation" bear any association with the work of Robert Merton.[14] To the contrary: the text of *Mass Persuasion* and the figure of Robert Merton are both conspicuously absent throughout the vast literature on repetition with variation.[15] This said, it would be incorrect, I believe, to classify this absence as an instance of "obliteration by incorporation," or OBI. One speaks of OBI when an intellectual field or tradition has so fully absorbed a particular concept, idea, method, or finding that the original source or parentage of this formulation is no longer cited or even remembered (Merton 1968a, 25–38). In the bodies of scholarship where "repetition with variation" is presently on active duty, however, there are no indications that Merton's use of the concept was *ever* recognized, let alone subsequently absorbed so fully

[14] After his return to Germany, Adorno's writings (especially those on musical composition) continued to speak of repetition and variations, and these writings do inform the Continental philosophical tradition on the subject. Whether or not Adorno read *Mass Persuasion*, however, his appropriation of the concept predated its use by Merton.

[15] Another result of research using standard online search engines.

that his book and his name ceased over time to be cited. Rather, as used by contemporary scholars, the concept seems to derive from *other sources*, among them some of the very strands of work that formed part of Merton's own encounter with "repetition with variation"—viz., the literatures of social psychology, Western humanism, and modern science.[16] What is more, the practice of contemporary scholars who invoke "repetition with variation" has not been to suppress and obliterate these other sources of the concept, but in many cases to acknowledge them openly (see e.g., Gendron 2008; Johnstone 1994; Tannen 2008).

That scholars in these fields have neglected to acknowledge Merton's use of "repetition with variation," in other words, is not because they have grown accustomed to taking it for granted, but because they have yet to notice and reckon with it. At stake here, moreover, is an issue larger than the vagaries of academic citation practices. If the account in section 2 is cogent, Merton did more than simply apply "repetition with variation" in his analysis of Kate Smith's war bond drive; he sociologically broadened the concept, imparting to it a more generalized meaning than it had in the various separate literatures where the expression had previously circulated. Given, however, that his treatment of "repetition with variation" has been overlooked in the six decades since *Mass Persuasion,* this sociological reformulation of the concept has had no opportunity to air and take hold even among scholars in the social sciences and humanities who deal with repetition with variation. In this sense, the fate of Merton's concept represents the trajectory of a "vanishing family branch"—of VFB, rather than OBI—for, although "repetition with variation" would remain an intellectual force, outside of sociology at least, during the period subsequent to Merton's contribution, his own innovation with regard to the concept would disseminate no further.

4

Why Merton's concept of "repetition with variation" experienced this fate is a question that, for the present time, must be left to speculation. With a very few exceptions, as section 3 documents, Merton's concept did not diffuse, and accounting for historical *non-happenings* is gener-

[16] Unlike Merton writing in the 1940s, however, more contemporary scholars tend to draw on more recent entries to these literatures.

ally an ill-advised undertaking. What is more, a large part of the story, the fact that scholars outside of sociology overlooked Merton's concept almost universally, seems scarcely to require further explanation. That non-sociologists would neglect the contribution of a sociologist that barely registered on sociologists themselves is obviously an unremarkable occurrence, particularly in an historical era when scholarly publications were proliferating in such great numbers that most scholars were unable to keep abreast even of the literature of their own fields.

Less obvious is why sociologists failed to notice the concept, which came to them by the hand of one of the preeminent, most respected, and most widely-read figures in sociology during the second half of the twentieth century—Robert K. Merton? To this part of the question, two factors suggest themselves as elements of what was likely a more complex historical process.

The first of these factors was the institutional relocation of the relevant academic topics. As seen above, Merton introduced "repetition with variation" in a monograph principally concerned with mass communication—a subject that he and Lazarsfeld situated squarely within sociology but which would not remain there. To the contrary: in the years ahead, as Katz has described, "communication research and studies of public opinion" were increasingly "abandoned by sociology" and, in the U.S. at least, transferred to separate "schools, colleges, and departments of communications, building of mergers of traditions of rhetoric and speech, journalism and publizistik, critical traditions in film and literature, and sociopsychologically oriented media research" (1987, S40). Almost inevitably, this institutional relocation significantly reduced the potential sociological readership of *Mass Persuasion*, diminishing the chances that sociologists would encounter "repetition with variation" and put the concept into wider circulation inside their own discipline.

The second factor was the motility of Merton's own intellectual interests. In the preceding discussion of sociologists' use of "repetition with variation" in the decades following the publication of *Mass Persuasion*, one likely sociologist-user of the concept went unmentioned, and that is, of course, Merton himself in the years that lay before him. As it happened, however, Merton was among those who did not subsequently make use of the concept; inadvertently, he thus deprived "repetition with variation" of the broader stage it would have occupied if he had included the concept in, say, *Social Theory and Social Structure* or in other of his major works.

Why Merton did not return to the concept, though, is another non-event that scarcely lends itself to a conclusive account. Of relevance, presumably, is that Merton, too, soon moved away from the area of mass communication, publishing a handful of pieces on the subject during the next three years but thereafter ceasing active research on the subject.[17] In even the most wide-ranging of these additional pieces (Lazarsfeld and Merton 1948), moreover, the questions addressed differed from those raised in *Mass Persuasion* and were not such as to lead Merton to reintroduce or revisit "repetition with variation."

The same holds true throughout his later work on all the other topics that he went on to examine, whether they were matters of occasional interest to him (housing, medical education, friendship) or of more permanent concern, such as the sociology of science or sociological theory. Indeed, in some ways, his sociology of science would appear to be deeply at variance with his interest in *Mass Persuasion* in repetition with variation, inasmuch as his writings on science deal regularly with innovation, invention, and discovery, and with the social institutions and processes that foster the creation in the natural sciences of new knowledge, as distinguished from the reiteration of existing ideas (see esp. Merton 1957a, 1973b; Merton and Barber 2004).

A similar emphasis on the discovery of new knowledge enters prominently into Merton's writings on sociological theory. In his famous essay "On the History and Systematics of Sociological Theory," for example, Merton is at pains to warn against the practice of describing the history of sociology in the manner of the "adumbrationist" historian of ideas who magnifies intellectual "similarities between past and present" and, in this way, denies that "sociology grows through occasional new orientations and through increments of knowledge gained through inquiry guided by these orientations" (1968a, 22–24). Merton illustrates this objectionable practice by citing his former Harvard

[17] In all three editions of *Social Theory and Social Structure* (1949, 1957, and 1968), however, Merton continued to make "mass communications" a prominent topic of the volume's Part III and to reprint his 1943 article with Lazarsfeld, "Studies in Radio and Film Propaganda." In his 1960 "Introduction" to LeBon's *The Crowd*, he returned again to the subject (see n. 5 above).

mentor Pitirim Sorokin (1928), who frequently asserted that modern sociological ideas are the "'mere repetition'"—sometimes "with variations"—of ancient ideas (1968a, 24–25). From Merton's point of view, this kind of focus on repetition seriously jeopardizes recognition of "the possibility of significant new departures in the history of sociological thought" (1968a, 25).

On the basis of passages like these, one might plausibly propose that Merton's concern with the discovery of new knowledge—both in the natural sciences and in sociology—sidelined his earlier interest in processes of repetition with variation, with the consequence that the concept of "repetition with variation" disappeared from his work. A broader reading of Merton's *oeuvre*, however, suggests something more complicated: viz., that while Merton ceased to use the concept as such, he remained acutely—and, among sociologists, perhaps singularly—alert to instances of the phenomenon itself.

In support of this interpretation (which I here offer as no more than a hypothesis for future research on Merton's work), I conclude with three brief illustrations. All three differ from the prime example of "repetition with variation" in *Mass Persuasion* because they have to do not with the members of a mass audience, but with the types of social actors that concerned Merton in his later work—scientists, sociologists, and other men and women of ideas. Awareness of this difference may, at some level, have made Merton hesitant to apply the same concept by name, although the illustrations arguably involve (in different ways) an analogous temporal patterning of effects.

First, there is Merton's recurrent concern with "multiples" in science.[18] In Merton's view, scientific discoveries are rarely "singletons"; indeed, he offers the "hypotheses [...] that all scientific discoveries are in principle multiples" (1961, 356)—i.e., "substantively identical or functionally equivalent ideas and empirical findings set forth by two or more scientists, each unaware of the other's work" (1968a, 9). In developing this thesis, however, he is quick to add that multiples are not ordinarily complete replicas: for example, "no two of the twelve scientists who 'grasped [the] essential parts of the concept of energy and its conservation' had precisely the same conception"; and "for the

[18] The points made in the next two paragraphs were not in the original version of this paper. I owe their addition to Harriet Zuckerman, who impressed upon me the place on repetition in Merton's sociology of science.

typically less precise formulations in much of the social sciences, it becomes even more difficult to establish substantive identity or functional equivalence of independently evolved conceptions" (1968a, 10).

In this sense, "multiples" bear a distinct resemblance to repetitions *with variations*, though Merton's accent here generally falls more on the element of repetition than that of variation. In any event, he insists that multiples form a "strategic research site" for sociologists of science, revealing much about the processes of scientific discovery and scientific communication, including what he calls the "functions of redundancy" (1973a, 371; 1968, 450). Indeed, taking issue with scientific policy-makers alarmed about "wasteful duplication," Merton argues that "redundancy in independent efforts to solve a scientific problem" actually increases the "probability of solution"; and he quotes with approval an interview, conducted by Harriet Zuckerman, of a Nobel laureate who remarked: "In respect to research; duplication of effort is a good thing [...] if there are different groups in different laboratories working on the same thing, their approach is sufficiently different [to increase the probability of successful outcome]" (1968, 450–51 and fn. 30 [words in brackets added by Merton]). Not only this, but repetition (with variation) redounds positively to the communication channels of science, especially in the contemporary era of "exponential increase in the volume of scientific publications": "often a new idea or new empirical finding has been achieved and published, only to go unnoticed by others," whereas "multiples—that is, redundant discoveries—have a greater chance of being heard by others in the social system of science and so, then and there, to affect its further development" (1968, 449; 1973a, 380).

Second, repetition with variation seems to echo through Merton's frequent claims for the significance of "continuities" in the growth of sociological knowledge. The very titles of several of his writings—titles that themselves exemplify the literary practice of repetition with variation—convey this message, which was central to Merton's research and teaching: *Continuities in Social Research* (Merton and Lazarsfeld 1950); "Continuities in the Theory of Reference Groups and Social Structure" (Merton 1957b); "Continuities in the Theory of Social Structure and Anomie" (Merton 1957c); *Continuities in Structural Inquiry* (Blau and Merton 1981). For what are the "continuities" that Merton describes, in these publications and elsewhere, if not fruitful repetitions with variation upon earlier sociological conceptualizations and findings? Merton says as much himself when he sets as "the hallmark of systematic inquiry" by which sociological knowledge develops the identification of those "theoretical and empirical continuities in inquiry which extend, modify and correct earlier formulations" (1957d, 177)—his supposition being that robust later-day sociological formulations are neither creations *ex nihilo* nor "mere repetitions" of past sociological statements but carefully refined variations on existing ideas. Worth noting in this regard is Merton's familiarity with Lewis B. Namier's essay "Symmetry and Repetition," which likened intellectual "continuity" to "variations" on established themes and offered the memorable aphorism: "Continuity is a compromise between novelty and repetition" (1941, 72)[19] —a view implicit in the Mertonian notion of continuity.

Third, in Merton's late program for sociological semantics, a process much like repetition with variation reappears to assume another pivotal role.[20] As I have argued elsewhere, Merton's concern with tracing the social origins and paths of diffusion of the linguistic expressions that he examines is a concern that goes hand in hand with an interest in identifying the social processes and mechanisms that produce these outcomes (Camic 2010). In describing these mechanisms, Merton develops the concept of the "sociocognitive microenvironment," which he uses to designate the local "social and cognitive network" or "sociometric structure" in which the women and men involved in the origin and spread of the relevant words, concepts, or other ideational forms interact with one another (Merton 2004b, 260–60; 1977, 98–99). According to Merton, unpacking the dynamics of sociocognitive microenvironments is pivotal to the sociological explanation of the origins and diffusion of ideas and other linguistic expressions (1977, 2005a, 2004b). What is more, understanding these dynamics requires a grasp

[19] Sills and Merton include a passage from Namier's short essay in *Social Science Quotation* (1990, 319), albeit not the passage that I have just quoted.

[20] In this paragraph, I focus on an explanatory argument that Merton makes in his semantic writings. Looking beyond this particular explanatory argument, one might also raise the question of whether a concern with repetition with variation does not lie at the very core of Merton's semantic program. For, as he traces of the fate of expressions like "on the shoulders of giants," "serendipity," and so on, what Merton specifically appears to be doing is charting the varied repetitions of these expressions across time and space.

of their fundamental temporal character, since, in the empirical cases that Merton examines (1977, 1995), it is not sufficient that actors encounter a single conducive microenvironment, but that they experience "successive microenvironments" that exert cumulative effects by conveying similar cognitive materials in different social contexts (1977, 86)—a development that Merton might well have called "repetition with variation." For, in emphasizing the importance for the origin and spread of new ideas not of any one sociocognitive environment by itself, but of a mutually-reinforcing "succession" of microenvironments (1995, 34), Merton calls renewed attention to a social process that he had clearly recognized decades earlier in *Mass Persuasion*.

Indeed, without too much straining, what one can observe in the three examples—of multiples in science, continuities in sociological theory and research, and successive microenvironments in the evolution of ideational forms—are the ways in which a stimulus (a scientific discovery; a sociological formulation; a sociocognitive network) has effects (on scientific communities; on the growth of sociological knowledge; on the social actors who originate and spread a linguistic expression) after multiple varied iterations which the same stimulus did not produce initially. In all three cases, in other words, Merton arguably continues to illuminate the sociological problem raised by his 1946 reformulation of concept of "repetition with variation" as to "why a stimulus applied successive times has an effect the *n*th time which it does not have the first"—tacitly pointing, as he does so, to the enduring value of the concept.

We come at this point, however, to the outer bounds of the agenda for sociological semantics. Whether sociologists going forward make further and more explicit use of Merton's concept, either in their own research or in the interpretation of his work, is a question for a later and more comprehensive inquiry into the fate of "repetition with variation." As Merton develops the program for sociological semantics in his later work, it is a program that does not forecast future trends but rather devotes itself to historical research into the origins and diffusion of significant linguistic expressions. In this respect, the program nonetheless offers a model with wide applicability, including extension to the study of some of the concepts that inform Robert Merton's own writings.

Robert K. Merton and the Transformation of Sociology of Knowledge and Possible New Directions

Shmuel Noah Eisenstadt

This chapter discusses the place of Robert K. Merton in the transformation of Sociology of Knowledge (SoK) and some indications for possible future directions. The basic sources are the three articles included in Part III of *Social Theory and Social Structure*, "The Sociology of Knowledge and Mass Communications" (Merton 1957), and a second direction indicated in Merton's thesis on Puritanism in *Science, Technology and Society in Seventeenth Century England* (Merton 1970)—or as Harriet Zukermann (1989) has shown, two Merton theses. I shall briefly discuss whether these two directions of Merton's work on the Sociology of Knowledge are complementary or contradictory, and whether a meeting between them indicates possibilities for the future. Significant from this perspective is the great corpus of Merton's work on sociology of science that manifests one of the transformations of SoK. A less well known article co-authored by Merton with Sorokin on social time (Sorokin and Merton 1937) is seemingly—but only seemingly—out of kilt from both directions indicated above. Thus the paper also discusses Sorokin's influence on both Merton and SoK in general.

1

The first question to consider is whether SoK was really transformed from the period when Merton was writing the articles included in *Social Theory and Social Structure* as well as his dissertation, and, if so, what was the essence of this transformation. Charles Camic wrote an instructive article on SoK in the new international encyclopedia of the Social and Behavioral Sciences (Camic 2004), which, compared with the article written by Lewis Coser about 40 years before (Coser 1968), indicates that Sociology of Knowledge was greatly transformed from the middle or late 1920s and the 1930s onwards in Europe (Shils

1982 and Stehr 1982). During this period SoK in Germany and in France constituted a central component of the sociological discourse. It was presented as bearing on central problems of intellectual discourse of that period. The thinkers connected with SoK in the 1920s and 1930s were above all Max Scheler, and Karl Mannheim in Germany, and in France the Durkheim school, including Durkheim, Mauss, and others (Durkheim and Mauss 1963; Durkheim 1954; and Granet 1950).

Three further names are important to mention, albeit neither of them are generally connected with SoK discourse. The first is the Russian-American Sorokin, already mentioned, who is especially important when studying Merton (Sorokin 1962, 1964; and Tiryakin 1968). The second is Max Weber. Weber's brilliant student Alexander von Schelting wrote in 1927 in the Archives an important article on SoK (von Schelting 1927), parts of which were incorporated as an appendix in his book on Max Weber's "Wissenschaftlehre" (von Schelting 1934). Twenty years later von Schelting published a monograph that deals with SoK, in a book called "Russland und Europa in Russischen Geschichtlichen Daenken" (Russia and Europe in Russian Historiographical Thought) (von Schelting 1948). It was published in Switzerland just after the Second World War and never translated into English, thus it has not gained a wide audience. In this way, Weber's connection to SoK gains some recognition through von Schelting, despite the fact that it is almost obvious: that at least Weber's three volumes on *Religionsoziologie* are among the most important contributions to the SoK discourse. Finally, there is Ernst Cassirer. While seemingly he has no connection with SoK, Cassierer's work, especially on the different symbolic formations, though not sociological itself, is a very important potential contribution to SoK that, as far as I know, has not been taken up (Cassirer 1957).

2

For some, at the time, the First World War created a crisis in the forward march of modernity. Knowledge and the question of its relativity, fused into the general discourse of European intellectual life of the inter-war period. It was in this context that the above mentioned writers addressed the Sociology of Knowledge. While this did not include scientific knowledge and the question whether it is objective and universally valid or relative to the intellectual life in which it crystallizes, the issues that it raises were such as, for example, the kin question of whether knowledge is autonomous. In other words, does knowledge have its own momentum or is the way it evolves and develops a reflection of social forces? The difference in focus among German and French writers is a response in itself to the very questions they raised, although a European thread is also apparent. In Germany, where the crisis of modernity was probably most acute, SoK was first debated in relation to the Marxist tradition of economic and class relations, but other issues, such as race relations, was also a central concern. Thus, the German perspective spoke of *Realfaktoren*, the social components that affect the direction and formation of *knowledge*. In France the Durkheimians tried to find a new basis for the organic solidarity of the modern societies, of the French lay republic against the various attempts to attack it (Durkheim 1954). They did so by focusing on the roots of the system of classification of human thought, apparently showing the autonomy of the system. However, according to the standard interpretation of Durkheim's work, the classifications were seen rather as a reflection of the social structure. This was, for instance, the major message of Marcel Granet's great study of Chinese thought and society (Granet 1950), as was also the case with other Durkheimian anthropologists. Thus Durkheim can be read as 'simply' the expositor of how systems of thought reflect social structures, but a closer reading, especially of his later work, shows an ever greater awareness of the autonomy of symbolic universes. Of the Durkheimians this was especially apparent in the work of Robert Hertz (Hertz 1994) and more recently of Mary Douglas (Douglas 1966, 1973, and 1987).

While SoK was central in the discourse of continental Europe, it was of almost no interest in England. Morris Ginsberg (Ginsberg 1948, 104–122) referred to it in an article summarizing and analyzing what was happening on the continent, but it did not go beyond this, and although Mannheim's *Ideology and Utopia* was translated into English by Louis Wirth and Edward Shils and published in London, it had no great resonance in English or even in American sociology (Mannheim 1936).

In the United States there was no crisis of modernity after the First World War comparable to that found in Europe. However, in the 1920s and even before, there developed a line of pragmatism as can be seen in the work of Charles Cooley, George Herbert Mead, John Dewey and others (Mead 1934; Cooley 1930; Thomas 1957; Dewey 1926, 1931, 1939, 1958). They dealt with the constitution of knowledge, with the relation between knowledge and practical life and also the place of knowledge in democracy. While these issues resonated in European thinking, American pragmatism was seen as anti-metaphysical, anti-transcendental, and overly practical. One German scholar, Eric Voegelin, wrote an incisive book on America already in the 1920s showing that the American program was a different way of addressing the transcendental dimensions of modernity (1995). But because of their different starting points and historical experiences, the continental European and the American problematique were not confronted with each other until the exercise has been recently taken up by Hans Joas (Joas 1993 and 1997; also Wolff 1974).

The difference between the European and the American approaches to SoK is related to the transformation in the States of many of the major "European" concepts of sociology**.** To take up two: charisma and mass culture or society. The central place of charisma in the work of Max Weber is well known, as is his great interest in the discourse in mass society, the fear of the masses in Germany, especially in German sociology. Both these concepts were transferred and transformed, even domesticated in America by the work of Edward Shils (Shils 1975, ch. 7, 15 and 21 on 'charisma' and ch. 5 and 17 on 'mass society'). In Europe these were ambivalent concepts, connected with the feeling of the crisis of modern society. In the United States they became important analytical components of social structure, to be analyzed as such.

Here a short note on Sorokin may not be out of place. Sorokin indicated, often in great detail, the relation between basic premises of civilization and distinct patterns of communities of knowledge, showing some crucial elective affinities between them. Of special interest in this context was his analysis of the different conceptions of time and space (Sorokin 1964) of which the paper written by him and Merton certainly constitutes a very important illustration (Sorokin and Merton R. K. 1937; also Merton R. K. 1989). Since Sorokin's work is not very widely known, it might be worthwhile to present it—even if briefly—following E. Tiryakian's more detailed exposition:

> Sorokin's major sociological presupposition is that total reality is a manifold infinite which transcends any single perspective; it encom-

> passes the truth of the senses, of the rational intellect, and of suprarational, hyperconscious faith, intuition, or insight.
>
> These sociocultural phenomena are not randomly distributed but form coherent aggregates. Although there is no meaningful integration of all the sociocultural items that coexist in a particular setting, sociological analysis can reveal a hierarchy of levels of integration. The highest level of integration of sociocultural meanings and values is reflected in major social institutions. All such high-level sociocultural systems (those whose scope transcends particular societies) are existentially organized around fundamental premises concerning the nature of reality and the principal methods of apprehending it. The range of major alternatives is limited: reality is felt to be directly given by the senses ("sensate") or disclosed in a supersensory way ("ideational"), or else it is considered an organic and dialectic combination of the foregoing possibilities ("idealistic"). Correspondingly, there are three irreducible forms of truth: sensory, spiritual, and rational. At various periods of history the possible basic premises are in various phases of development, and in any well-defined period of history the five principal cultural systems (law, art, philosophy, science, and religion) of a complex society exhibit a demonstrable strain toward consistency in their expression of reality. (Tiryakian 1968, 61–63)

3

In the 1920s and 1930s, Sociology of Knowledge in continental Europe was taken up in several historical works, providing interesting, but in a way ad-hoc analyses—such as, among others by A. von Martin, Arnold Hirsch and L. Balet, E. Gerhard and others, or in the work of Sven Ranulf in Denmark, as well as the chapters collected by Scheler (von Martin 1944; Balet and Gerhard 1936; Ranulf 1938; Scheler 1960). Later on there appeared in France, more sophisticated works by Groethuysen (1927–1930) and H. Brunswick (1947), or Jacob Katz in Germany (1935). Towards the end of the 1930s there appeared several interesting works noted in Lewis Coser's survey, touching on central problems of SoK, such as for instance, F. Znaniecki's book (1940). But there was little comparative or systematic analytical work in this direction. Concomitantly, most of these works did not analyze systematically relations between various aspects or components of knowledge or the processes through which different realms of knowledge are crystallized in different social contexts.

Nor did the research undertaken discuss the dynamics and the internal structure of the symbolic systems they were analyzing. Scheler in contrast to the Marxists, in contrast even to Mannheim, emphasized that the different symbolic spheres are not a reflection of the *Realfaktoren,* that they constitute an objective autonomous realm different components of which are selected in different societies by the *Realfaktoren*. But while Scheler emphasized the autonomy of the realms of knowledge, he did not systematically analyze the processes through which they become selected and activated in different social settings. Mannheim, on the other hand, differentiated between different *Realfaktoren*. He discussed generations, political interests and status groups, showing how these different social positions determine or at least greatly influence the contents of symbolic themes. But he did not analyze the contents of these symbolic realms. Neither Scheler, nor the Durkheimians or Manheim for that matter, provided a systematic analysis of the social processes through which the selection is affected, or the affinities between different social position and the constitution of symbolic realms.

Whatever the contributions of these works are, they shared, because of their emphasis on the problematic of the relations between *Realfaktoren* and knowledge, an important analytical weakness. Norbert Elias, who was a student of Karl Mannheim, in a short statement has said that *Realfaktoren* are to a great extent shaped by symbolic components; that *Realfaktoren*—class, race and the like—are not just external objective givens and that symbolic components are constitutive of them (Elias 1981). This criticism, however, was not systematically addressed. Elias made further contributions on this point in his book on the *Process of Civilization* (1939) and in his later works, which were not, however, brought systematically at this stage under the canopy of SoK (Elias 1939 and 1982; and Kilminster and Mennell eds. 2008).

In this context it is worth noting the fact that the above authors did not refer to the work of Cassirer (1957), who devoted so much to the analysis of different types of symbolic realms that constitute the foci of human consciousness, and the neglect of Max Weber whose work, especially the *Gesammelte Aufsätze zur Religionssoziologie* (1920–21), looked at how different societies and social strata create different types of knowledge and the changes in the nature of the carriers of different patterns of knowledge in different social sectors, with a strong emphasis on the crucial role of heterodoxies.

As we have observed, the transformation of SoK, as it crystallized after the 1930s, entailed the tendency to bracket out the contents and the constitution of different domains or components of knowledge from their broader societal context, on one hand, and the growing emphasis on the process and mechanisms of production, distribution and reception of knowledge, on the other hand, as well as a complete dissociation from the other areas of sociology and the intellectual discourse in general. As Charles Camic indicated, the whole interest in SoK as a unified field of central importance both in sociology and in the general intellectual climate has abated (Camic 2004).

Many of these deficiencies were pointed out by Robert Merton, especially in his paper on SoK, and provided important recommendations for possible directions of research—in particular asking which aspects of knowledge are being studied and what is the importance of identifying the processes through which different realms of knowledge crystallize in different contexts. This list however was not on the whole followed up in any systematic way. Merton himself moved more and more into Sociology of Science, even though he contributed the occasional articles on the social structural aspects of knowledge (Merton 1972; also Curtis and Petras 1970). Indeed, Merton was influential in the transformation of SoK in these directions—in the bracketing out of contents, emphasizing the different social processes of production and organization of knowledge. However, we should also not forget that there is another Merton, the Merton of the Merton Puritan thesis, which is also significant in terms of possible future directions of research (Merton 1970).

4

After the Second World War, the continual development of Sociology of Knowledge and its place in the general intellectual discourse have greatly changed (Kuklick 1983). Two things happened. On the one hand several overviews of SoK were published in various forms, such as annual reviews (Remmling 1973; Stark 1958) and a number of readers (Schütz 1962, 1967) as well as several *Handbücher* of SoK and of *Wissensocoziologie* in Germany. On the other hand, several lines of enquiries were followed that were a continuation of pre-War developments, such as that stemming from Schutz's phenomenology (Garfinkel

1984), research dealing with every-day knowledge and ethnomethodology as well as an extensive program in the Sociology of Science (Barnes 1974 and 1977; Barnes and Chopin eds. 1979; Kapport 1981; Ben-David and Sullivan 1974, 203–222; Barber 1952). A parallel development in the 1940s and 1950s was a series of works published in France devoted to the constitution of words of knowledge, following but transcending Durkheim (Vernant 1980; Vidal-Naquet 1981; Detienne 1972; Gernet 1976). However, these were not unified nor brought under the general canopy of SoK. Sociology of Knowledge lost its central place in the general discourse and within sociology as well.

Most of these new developments, cutting across many research areas, are summarized in an *Annual Review of Sociology* article by Anne Swidler and J. Arditi on the "New Sociology of Knowledge" (1994). A major thrust of their analysis is the shifting of the emphasis from external to internal *Realfaktoren*, or to a combination of external and internal ones, as they operate in the process of creation and distribution of knowledge. There is a continued emphasis, by Foucault and others, of the connection between knowledge and power, but the article goes beyond their work and it might be worthwhile to quote from their introductory and concluding remarks:

> [...] The new sociology of knowledge examines how kinds of social organization make whole orderings of knowledge possible, rather than focusing in the first instance on the differing social locations and interests of individuals or groups. It examines political and religious ideologies as well as science and everyday life, cultural and organizational discourses along with formal and informal types of knowledge. It also expands the field of study from an examination of the contents of knowledge to the investigation of forms and practices of knowing.
>
> This review begins with a fundamental factor that shapes the ways knowledge can be structured—the media through which knowledge is preserved, organized, and transmitted. It then turns to the analysis of collective memory, examining social conditions that shape how knowledge is transmitted through time. The review then examines how patterns of authority located in organizations shape both the content and structure of knowledge. We bring together work on how forms of authority affect the scope, generality, and authoritativeness of knowledge. We then review recent work on how social power, particularly that embodied in institutional practices, shapes knowledge. In the next section, we examine how knowledge reinforces social hierarchies and how the boundaries and categories that define the basic terms of systems of knowledge are constituted. Looking at the recent literature

on power, gender, and knowledge, we discuss revitalized versions of the standpoint theories that characterized the traditional sociology of knowledge, exploring how new approaches deepen the understanding of what a social standpoint involves. Finally, we turn briefly to recent work on informal knowledge, that knowledge ordinary people develop to deal with their everyday lives.

[…] Little of the work reviewed here explicitly locates itself in the sociology of knowledge. Despite diverse disciplines, perspectives, and substantive foci. However, these literatures allow at least some preliminary conclusions. First, social authority shapes the authoritativeness of knowledge, affecting both the authority knowledge can effectively claim and the forms that knowledge claims take. Second, distinctions, social and intellectual, are made along lines of social differentiation, particularly hierarchical ones. Third, shifts in the media through which knowledge is transmitted, especially the transition to print, have dramatic effects on the entire organization of knowledge systems. Fourth, to explain why new knowledge emerges and to account for the social effects of ideas, scholars need to pay careful attention to factors that directly affect the institutions and actors that produce and distribute knowledge. Fifth, analysis of how the social location of actors affects their knowledge must account for the constitution of actors themselves. Sixth, knowledge and power are intimately related because power allows people to enact realities that make their knowledge plausible. […]

[…] The new sociology of knowledge, not yet a unified field, does not have a single problematic around which debates revolve. Nonetheless, there are opportunities for fruitful research along the many lines where the literatures brought together here converge and diverse. (A. Swidler and J. Arditi 1994)

5

The above extracts from the Swidler-Arditi paper suggests two areas in the 'not-yet-unified-field' where deficiencies exist. First, as the direction of research turned from micro- to macro-societal processes, there is now a need for a comparative analysis of how the combination of these processes impact on the production and distribution of knowledge. Second, while in the paper there is a passing reference to Merton's Puritan ethic theories, developments related to this theme and, in particular, connecting it with macro-societal processes and other central problems of sociological theory, provide indications for possible new directions in the sociology of knowledge.

A number of conferences that took place in Jerusalem over twenty years ago begun to take on some of these shortfalls in the field. The symposium in honor of Robert Merton, later published as a special issue of *Science in Context* (Feldhay and Elkana eds. 1989; see also Eisenstadt and Silber 1988), spent little time on the mechanisms of knowledge production and instead highlighted a shift in the analysis of the content of symbolic realms and the cultural forces shaping it. Rivka Feldhay presented a paper comparing Protestant and Catholic science as the carrier of the great Jesuit intifada. While the comparison may seem like a natural approach to a relevant problem, one that has previously been applied to Protestant and Catholic economic and business enterprises, it has nevertheless not previously been addressed within the framework of SoK. The ensuing discussions centered on comparative scientific developments in Protestant and Catholic societies. What has become evident is that while there was significant scientific work in Catholic societies too, the same body of knowledge as was present in Protestant societies was interpreted differently based on their different cosmological assumptions. Another paper, dealing with the autonomy of science, pointed out exactly this relationship, showing that the construction of knowledge—and its impact on the general intellectual discourse—depend not so much on *Realfaktoren*, but on basic cosmological-theological premises (Heyd 1988).

6

Having surveyed the development of the Sociology of Knowledge from its beginnings, we will now turn to some possible future directions. While the examples given below may appear at first disconnected, it is hoped that, taken together, they may point to a more differentiated approach to SoK. Thus, there should be a reconnection of macro-societal analysis with the comparative study of civilizations as well as central themes in current sociological theory, bringing it closer to Merton's scientific semantics discussed elsewhere in this volume. Even if this were to be successful, there could not, of course, be a new synthesized sociology of knowledge of the type that was present in Central Europe in the inter-War period. This would be technically difficult as science and scholarship has been minutely specialized with ever-greater differentiation and sophistication. More importantly, the central assumption that knowledge, objective scientific knowledge, is an autonomous component of our collective cultural self-consciousness has been shattered by postmodernism, deconstructionism, and poststructuralism (Cohen 1988; Ezrahi 1988). These developments do not necessarily entail a complete denial of the objectivity of knowledge, but constitute a more sophisticated and critical analysis derived from post-quantum theory, making us aware of the influence of the observer on the nature of the observation. The observation does not become completely subjective, however, but our understanding of 'objective' has been altered.

Examples of two trans-cultural comparisons may help set the scope of possible directions of future research on SoK. First, the reception of Aristotelianism in the great monotheistic civilizations of Judaism, Christianity, and Islam (Macy 1988) may provide fruitful ground for analysis. While Aristotle was not concerned with God, and certainly not with revelation, Maimonides, St. Thomas, and El Ghazali had to confront the missing link between Aristotelian philosophy and revelation within the confines of their own theological realms. Second, an intriguing but overlooked phenomena in the field of linguistics—an obviously important aspect of knowledge (Shulman 1988)—is that its study in India has generally been far more sophisticated than comparable research in the West. Research into why this should be and what cultural premises have given rise to such difference would be instructive.

Another area where science, religion and knowledge production cross paths, is the participation of Orthodox Jews in different fields of science. In the late nineteenth and early twentieth century mathematics and engineering were the foremost areas of science where Orthodox Jews, including the great Lubavitch Rabbi, participated. Physics on the other hand, for example, entailed cosmological presuppositions with which Orthodox Jewry was not ready to engage. Today, physics and chemistry are no longer taboo, their place has been taken by microbiology and especially evolutionary microbiology. Research is needed to understand the process the Orthodox community went through to allow this change and to connect it with the changes in the scientific fields where philosophical issues of epistemology and cosmology have been bracketed out, allowing significant scholarly work without having to confront those issues. In a similar vein, the success of Japanese scientific scholarship requires some analysis (Eisenstadt 1996, especially 318–345). By looking at why Japanese science was able to sprint forward so successfully, we may be able to further our understanding of the relationship between scientific knowledge, religion and society.

Japanese scholars did not have principled problems with science, as Chinese, Jewish or Islamic intellectuals apparently did. Japan is not an Axial civilization where cosmological tension between God the creator and modern reality is central. In the West, for exactly this reason, questions of metaphysics always hampered the development of science, an issue we have already pointed to in relation to Protestant and Catholic societies.

Understanding the success of Japanese science is further complicated by the fact that the few Japanese Nobel Prize winners were honored for work they accomplished while in Europe or the United States. The reason for this is more likely to lie in the structure of Japanese scientific society and the emphasis on seniority and patron-mentor relationships among scholars, than in basic metaphysical assumptions. One further issue that could bare significant research in comparing Japanese and other scientific societies is their respective conception of life. An anthropologist once observed how after crude and fatal experiments were conducted in a Japanese laboratory on monkeys, they held special memorial services for the monkeys had they just killed (Asquith 1986, 29–33 and editor's introduction). Similarly, while abortion is legal in Japan, in the Kamakura Temple near Tokyo there are memorials for the aborted children that are regularly visited and worshipped by the parents. These conceptions of life and death differ greatly from those predominant in monotheistic societies as well as in classical Chinese Confucianism. It would be interesting to study the impact of this on Japanese science, for example in stem cell research.

The issue of the structure of Japanese scientific society raised above bears on the debate on whether Merton's emphasis on the ethos of science or power within the scientific community has a greater determinant factor on scientific knowledge production. The debate, however, misunderstands Merton's work, as the ethos of science he describes is closely connected to basic cultural assumptions and relate to the dynamics of the organization of the scientific world, including conflicts and patterns of incentives, punishments and rewards within it. The role of organizational ethos is well observed in the research of the Swiss historian Herbert Lüthy on *Le Banque Protestante*, the Protestant Bank (Lüthy 2000; Eisenstadt 1968). Lüthy looked at the bank in Geneva belonging to M. Necker who travelled to France in order to help Louis XVI save the French financial system. While Necker's Protestant Bank was very successful and the techniques of banking were the same in Switzerland and France, Necker was unable to help turn France's fortunes around. The difference lay in the organization of banking. France had financiers instead of bankers, who were employed by the state. Thus in France they worked to entirely different patterns of expectations than in the small private bank of Necker and were motivated by a very different system of preferences, incentives, rewards and sanctions. Turning the tanker around proved too difficult for Necker. However, Lüthy's study could provide the seeds for comparative analysis of organizational ethos and the results such differences lead to.

I hope that the above show that the sociology of knowledge, as a significant field within sociology, has a future with an evolving but ever deeper scope for research, for which Robert K. Merton has done much of the ground work.

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