Shifting Paradigms

Thomas S. Kuhn and the History of Science

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Thomas S. Kuhn and the History of Science

A. Blum, K. Gavroglu, C. Joas, J. Renn (eds.)

Proceedings 8

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Proceedings of the conference "50 Years Since Structure: Towards a History of the History of Science," held in Berlin in October 2012

Communicated by Rivka Feldhay

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Cover Image: Thomas S. Kuhn being interviewed November 1989 in his office at MIT.
Photo: Skúli Sigurdsson

ISBN 978-3-945561-11-9
Published 2017 by Edition Open Access
http://www.edition-open-access.de
Max Planck Institute for the History of Science
Reprint of the 2016 edition
Printed and distributed by
ProBusiness digital printing Deutschland GmbH, Berlin
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The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie; detailed bibliographic data are available in the Internet at http://dnb.d-nb.de.

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Introduction

Alexander Blum, Kostas Gavroglu,
Christian Joas and Jürgen Renn

Thomas S. Kuhn's *The Structure of Scientific Revolutions* (*"Structure"* in the following) was first published in 1962 and became the most widely read book on the history of science. Since then, philosophers, historians, sociologists, educationalists, anthropologists, psychologists, economists, cultural commentators, journalists and readers belonging to many more academic and non-academic areas have been discussing this book. In scholarly journals, seminars, popular writings, monographs, public lectures and conferences, the book has been analyzed, commented upon, (often) criticized, (sometimes) praised, its impact assessed, and, in various instances, dismissed as trivial.

The appearance of *Structure* was perhaps the second major milestone, after the first publication of the journal *Isis* in 1912, to mark the rise of the history of science to a field enjoying broad recognition beyond the narrow community of its practitioners. On the occasion of the 50th anniversary of the publication of *Structure*, the Max Planck Institute for the History of Science organized an international conference, inviting scholars from various disciplines not only to reflect on Kuhn's impact and legacy but also on the history and the current state of the history of science. The present volume is an outcome of the conference *50 Years Since Structure: Towards a History of the History of Science*, held in Berlin in October 2012.

The primary intention of the organizers of this event was not to celebrate Kuhn's book, but rather to offer an occasion to discuss the remarkable developments that have led the community of historians, philosophers and sociologists of science to its present state. To this end, scholars were invited who themselves have shaped these developments in the past decades. For some, *Structure* was a decisive factor in these developments, for others it did not play much of a role; yet most would acknowledge that it is a book that was always "there," accompanying most of us in our collective or personal undertakings to further establish history of science. Indeed, the book has had a dominating presence for roughly half of the lifetime of the history of science as an institutionalized endeavor.

The present book sets Kuhn's *Structure* in context and makes it the subject of historical reflection and analysis. The first part of the volume is dedicated to personal recollections, including an interview with Kuhn himself conducted in 1990. The second part aims at historicizing Kuhn and his work. One important context that is discussed is that of the Cold War and its impact on the role and understanding of science. Another context relevant to situating Kuhn's work is that of the philosophical discussions of science in the twentieth century. The contributions to this part not only deal with the overarching theoretical argument of *Structure*, but also with the context of Kuhn's choice and interpretation of his major case studies: the birth of Copernican astronomy and the quantum revolution.

The contributions to the third part trace Kuhn's legacy in traditions of research and teaching in the history of science, which is remarkably substantial given that he never created a Kuhnian school in the history and philosophy of science in the traditional sense of the term. The essays in this part show in particular that the impact of *Structure* and other works not only consisted in discussions of Kuhn's challenging claims, but also in the models they set for productive investigations in the history of various scientific fields, some of them far from Kuhn's original concerns.

The openness of Kuhn's work is also reflected in the reinterpretations that it made possible. The fourth part is dedicated to such reinterpretations, in particular in the sociology of science, where his concepts and terminology have fallen on fertile ground. The fifth part deals with issues in the history and philosophy of science that were either neglected by Kuhn or where his position was challenged by alternative approaches.

The broad spectrum of papers and perspectives assembled in this volume will hopefully convince readers interested in the history of science that this field itself has a dramatic and contested history that is paradigmatically embedded in the fate of Kuhn's *Structure* and merits further exploration.

In closing, we would like to honor the memory of the British historian of science, John Pickstone, who sadly passed away in February 2014 before this book was published. His "big picture" approach to the history of modern science, technology and medicine greatly influenced the field. He will be missed by all those who had the pleasure of knowing him or working with him.

Where to Start?

John L. Heilbron

I want to thank the organizers for their generosity and their courage in asking me to open our useful and timely workshop. Not wanting to abuse the opportunity, I'll begin by asserting a proposition to which, as I suppose from your presence here, you all assent. Here it is: A better knowledge of the history of our discipline can help to resolve the identity crises that periodically afflict us and, perhaps, help us also to specify what, if anything, people who consider themselves historians of science have in common. Even a fuzzy specification can have its practical uses in suggesting curricula and defending territory within the institutions that support our work.[1]

History of Science and the Science of History

At first glance the task seems futile. Consider only the breadth of subjects slated for discussion at our roundtables and the proliferation of sub-fields reviewed in the *Isis* critical bibliographies. There are at least two signs, however, that point to a more hopeful prognosis. For one, the great expansion of our field, as measured by the number of entries in the *Isis Critical Bibliographies*, may have stabilized. After a big drop owing to changes in editors and editorial policy around 2000, they are tending towards, and perhaps will not exceed, their average in the 1990s.[2] The second hopeful sign is the selection of topics for the roundtables to begin tomorrow. Most of these topics are of the form "Science and X," where X equals science, philosophy, material culture, Eurocentrism, institutions and Thomas Kuhn. We do not have a provision for X = history. I take this omission as an indication that the organizers know that the history of science *is* history.

I believe that that was Kuhn's position too although his usual status as anguished outsider made him feel keenly the resistance of some general historians to our admission to their number. He attributed their resistance to the natural dislike

[1] The following text is a slight amplification of the opening talk at the Workshop, "Towards a History of the History of Science: 50 Years since *Structure*," organized by the Max Planck Institute for the History of Science, 17 October 2012. I am grateful to the editors for allowing me to retain the informal character of the original presentation.

[2] Entries remained flat at around 3000 between 1970 and 1985, and increased by 40%, to 4200 on average, in the 1990s.

of mathematics by people fond of history and to the persistence among them of a belief in a method that advanced science without any interesting intervention by human beings. Since he thought that the lessons of *Structure* had made this belief untenable, he regarded those who clung to it much as the old positivist historians had the Simplicios of earlier times. They were only a passing irritation, however, since eventually they would go the way of all Simplicios opposed to progressive paradigms. The two-culture problem, however, the antipathy of historians in general to science whatever its methods, was a far more serious problem. "In my depressed moments, I sometimes fear that the history of science may yet be that problem's victim." Kuhn meant that swelling our ranks with recruits who devoted themselves to external history would kill the true history of science while papering over the chasm between the cultures (Kuhn 1977, 160–161). This expression of foreboding dates from 1971. The history of our discipline that we are to construct will help us judge how far, if at all, Kuhn's bleak forecast has been realized.

Meanwhile, let us be content to know that history of science is history. It is not an inter-discipline, nor, I hope, an interim discipline. It has no special or preferred tie to philosophy, theology, sociology or political economy, although, as historians, some of us require some knowledge of one or more of them; as, indeed, we also do of art, literature, music, everyday life, in short, anything and everything that enables us to reconstruct the history of humankind's struggle to grasp, adapt to, and manipulate the natural world. We need not be overly concerned to draw boundaries among our sub-specialties or between history of science and general history. What should concern us is the scientific side of our business, by which I do not mean the sciences we study, but our standards of historical investigation and writing—the level of argument and evidence, and the control of technique, bibliography and languages, expected by and from professional historians.

If you grant this reasonable position, it follows that the historiography in which we should try to locate our own is the development of history as a science. The question whether or how far history can be considered a science is an old one. History itself gives the answer. Considered as a body of knowledge accumulated and upgraded by continually improving technique and ever-widening coverage, modern history is as much a science as modern physics. The two were begotten in the same scientific revolution and turned in parallel from reliance on ancient authorities to authentic documents. At the time that natural science learned to make instruments and experiments, history took up with charters, coins, medals, seals and inscriptions. Newton's *Principia* and Jean Mabillon's *De re diplomatica* were coeval—which does not mean equally bad. During the eighteenth and nineteenth centuries, the standards of evidence, reporting, testing and teaching rose rapidly in both the historical and natural sciences, and sometimes, as in the invention of the seminar and the institute, and in the study of meteorology, metrology, chronology and geography, they borrowed fruitfully from one another.

At the beginning of the twentieth century natural scientists and historians unselfconsciously referred to their endeavors in the same terms. As an example, I offer you two quotations, one from a physicist, the other from a historian, each a leader in his field. It is not easy to guess which is which:

1. "It seems probable that most of the grand underlying principles of [our science] have been firmly established and that further advances are to be sought chiefly in the rigorous application of those principles to all the phenomena which come under our notice."
2. "Ultimate [science] we cannot have in this generation, but […] all information is [now] within reach, and every problem has become capable of solution."

The first quotation comes from A. A. Michelson's speech at the dedication of the physical laboratories of the University of Chicago in 1894. The second comes from Lord Acton's report of 1896 on the status of *The Cambridge Modern History*, of which he was editor.[3] Acton's claim that history belongs among the sciences, with its echo of the practice of his master Leopold Ranke, was by no means unique in England (Lord Acton 1960, 26, 32–34). Everyone in Oxford remembers the conclusion of J. B. Bury's address at his inauguration as Regius Professor in 1904: "[history] is simply a science, no less and no more."[4]

Let us agree that history is some sort of science and history of science some sort of history. Then the question that brings us together, the question how our field has developed during the last half-century or so, should be related to the development of general history over the period. We should not be narcissistic or provincial in our efforts to define our field or faddist in our ideas about its core subjects and problems. It may be that we can learn something about answering our questions from the general historians and friendly philosophers who have been discussing and refining them for 400 or 500 years.

The *terminus a quo*

The subject of our meeting—the development of our field since *Structure*—does not make a perfect period for the historiographer. A better start date would be the years around 1900. We still depend on the work of the scientist-historians of that time and some of our major projects follow their lead. Consider only

[3] A. A. Michelson, quoted by Rescher (1978, 33), and Lord Acton, quoted by Carr (1961, 3).
[4] Quoted by Burrow (2007, 205).

the edition of Galileo's *Opere* by Antonio Favaro, published in 20 folio volumes between 1890 and 1910, which, together with the many special studies he spun from it, continues to support scholarship on the period of the Scientific Revolution. Favaro's approach remains alive in such major enterprises as the exemplary ongoing letter-press edition of Einstein's papers and correspondence. Although it has proceeded at a more deliberate pace than Favaro's, and with greater resources and a larger staff, it has not outdone him. Beginning our account of our field around 1900 would emphasize this essential strand of our heritage and allow us to appreciate its continuation into the new electronic environment. Other sorts of achievements of the old scientist-historians, like the preparation and annotation of Ostwald's *Klassiker der exakten Wissenschaften*, which came out at the rate of ten a year in the 1890s, and the decipherment of Babylonian mathematical texts, which gave the history of exact science a higher antiquity than the Greeks, suggest the range of their contributions to our historiography.

Commencement around 1900 would also allow us to evaluate better how much our conception of our field, its limits and problems, owed and owes to scientists. The division of our discipline into sub-specialties still follows too closely the organization of knowledge current in 1900. Pierre Duhem's explorations of scholastic thought about what looks like questions in classical physics remain influential in accounts of the process that created modern science. The positivist line, represented around 1900 by Ernst Mach's *Mechanics and its Development* and the award of the first chair in history of science at the Collège de France to Comte's followers, combined with Belgian internationalism to create the institutional father of modern history of science, George Sarton. Sarton's establishment of *Isis* just before World War I with the endorsement of several eminent scientist-historians would make a convenient end of the initial period of our historiography. The journal was to make possible the writing of a "truly complete and synthetic" manual of the history of science; to help in the creation of textbooks in science arranged historically; to "contribute to a knowledge of humanity […] and study the means of increasing its intellectual output;" and to "refound, on the deepest and finest historical and scientific bases, the work of Comte." Oh, and also to contribute to world peace and prosperity through the critical study of science, "the only [domain of thought] universally shared" (Sarton 1913, 43, 45).

Another eligible *terminus a quo* is 1930. In contrast with the fin de siècle, when an Acton and a Michelson, a historian and a physicist, could describe their fields in much the same terms and scientists could turn historian without changing their positivist underwear, historians of science of the later period responded to the wider historiographic trends of the depression-ridden 1930s. The decade began with Herbert Butterfield's contribution to general historical methodological in his *Whig Interpretation of History* and with Boris Hessen's disclosure of a Soviet approach to history of science, which inspired less crude versions by leftist British historian-scientists. At the same time, in quite a different direction, Otto Neurath and other logical positivists championed the idea of a unified science in which history would have a place—when it learned to express itself in the language of physics.[5]

Two new journals with distinctive programs in history of science made their appearance in the decade. *Annals of Science*, which aimed to "illuminate new aspects of political and social history" and to demonstrate that "all Science, all Natural Philosophy, is as purely human a production as Art or Literature, and is equally precious," began life in 1936 under the effective editorship of one of the world's few full-time lecturers in history of science, Douglas McKie of University College, London.[6] *Annals* specialized in the period since the Renaissance and carried the best of the production of the scientist-historians. *The Journal of the History of Ideas* first appeared on New Year's Day 1940. Its editor, Arthur Lovejoy, opened it by decrying departmentalization in the study of the history of ideas, the fad of the social construction of knowledge, faddism in general, and the lowering of standards of research and reasoning incurred by attributing irrational motives too freely to historical actors. This was the Lovejoy whose *Great Chain of Being* (1936) set a pattern for histories of unitary scientific ideas like Max Jammer's *Concepts of Space* (1954), *Concepts of Force* (1957) and *Concepts of Matter* (1961). Among much else of central interest to our field, Lovejoy's journal of ideas carried the entirely opposed but equally brilliant treatments of the Scientific Revolution by Edgar Zilsel and Alexandre Koyré. Finally, Koyré's peculiarly influential *Etudes galiléennes* dates from 1939.[7]

To stay with my theme of the relationship between general history and the history of science, I'll say a few more words on Butterfield and whiggism. He condemned it utterly. It is anathema, "the source of all sins and sophistries in history, starting with […] anachronism." We must not impose present notions on the past and we must not judge historical actors on how closely their behavior and ideas resembled ours (Butterfield 1957, 31–32, 97–98). That is about all most of us know about whig history. But Butterfield was too good a historian to leave it there. He added that no matter how hard you try, you will not avoid whiggism, it is an occupational disease. It is the inevitable consequence of the abridgments that transform note cards into analytic history, and of any narrative that has a beginning and foreseeable end. Because of its progressive character, science lends itself particularly well to whig history.

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[5] Cf. Carnap (1959, 165–166).

[6] Knight (1998), p. 156 quoting Harcourt Brown, p. 158 quoting McKie.

[7] Lovejoy (1940, 4–6, 15–19, 21); Stoffel (2000, 39–40); Wiener and Noland (1957, 147–175, 219–280).

Butterfield later tried to show how to mitigate the problem in his account of what used to be the touchstone tableau of our discipline, the Scientific Revolution of the seventeenth, or maybe the sixteenth and seventeenth, centuries. He advised a longer period, 1300 to 1800, and called his book, which dates from 1949, *The Origins of Modern Science*. In it he emphasized the need to attend to the losers, to deal sympathetically with outmoded systems of thought, to keep constantly in mind that historical actors differed from us. But in specifying his task as the identification of "the particular intellectual knots that had to be untied at a given conjuncture," he in effect took his present as the measure of losers and winners, of those who tied knots and those who loosened them. After all the knots were cut or unraveled, there came a revolution that, in Butterfield's ringing words, "outshines everything since the rise of Christianity, reduces the Renaissance and the Reformation to the rank of mere episodes," and rises unique as "the real origin of the modern world and the modern mentality" (Butterfield 1957, vii–viii). Butterfield's performance was impressive. He knew his material, argued cogently, understood the risk of presentism, and yet, wiggle as he would, was whiggish.

The so-called "social turn" in the history of science has the merit of attacking the more obvious forms of whiggism in narrative but often at the expense of abridgments that admit the subtler sorts. The restriction famously intoned, by the authors of *Leviathan and the Air Pump*, that "solutions to the problem of knowledge are solutions to the problem of social order," seems a transparent translation of our concerns about the place of science in government, industry and the military into motives for the behavior of historical actors who had no desire or means to make their contributions to knowledge of any use beyond their own amusement. Perhaps a more gaping abridgment in the same work is the extravagant synecdoche of taking Hobbes as the leader and also the only member of a group who shared his paradigms of science and power.

Returning to the benchmark 1930s, I find in the history of historiography by the notorious Harry Elmer Barnes, Germanophile professor of European history at Columbia University in New York, an unexpectedly balanced view of the relationship of history of science to general history. Writing in 1937, he was eager to enroll our subject among the other new recruits – the histories of art, economics, literature, social institutions and general culture that, in his typically robust formulation, had made the previous fifty years "the most important [period of] historical writing of all time." Barnes reported regretfully that his colleagues had not yet given history of science "favorable or fruitful attention." They soon would have to do so, he warned, if they were to remain faithful to their commitment to tell the full story of modern life. "A generation hence, it may well occupy as much of their attention as the history of constitution-making" (Barnes 1962, x, 298, 300 (all quotes), 302–308, 331–342). This proved a good forecast if only because historians lost interest in constitution-making.

History of science was just readying itself for promotion to a historical science in 1937. A year earlier the first professor of the history of science at Harvard, who had been waiting in the wings for 20 years, made his appearance stage center. This was Sarton. He published his inaugural address in *Isis* to serve as a milestone against which the progress of the history of science could be measured at other inaugurations to which he confidently looked forward. The main ingredients in his milestone were the rocks he threw at scientists who wrote incompetently on the history of their disciplines. He insisted that scientist-historians must meet standards of accuracy and objectivity, and deploy research techniques, no less demanding than those in force in the natural sciences. Scientists who wrote history (this is Sarton's opinion, not necessarily mine) abandon their standards and relax their rigor from the very first word. The result is worse than useless, since it diminishes the history of science for everyone (Sarton 1936, 3, 11, 16–18).

Sarton's *bêtes-noires* were whig scientists who lacked the historical science, that is, the bibliographical and research techniques, to do more than wrench the most obvious nuggets from the vast mine whence diligent diggers have been quarrying positive knowledge for millennia. These unscientific scientist-historians worked under what I'll call the old historiography or paradigm—in perfect correspondence with Kuhn's usage in *Structure*. Since scientist-historians were in effect the only practitioners of the history of science in existence when Sarton founded *Isis* in 1912, he had asked the best of them, including Favaro and Ostwald, to stand as its godfathers. Now, 25 years later, from the heaven and haven of a Harvard professorship, he declared that they stood in the way of progress. This was primarily a caricature devised for turf wars; Kuhn too was to find it useful; but a historiographer of our field who begins in 1900 would not entertain it for a minute.

To drive out the amateurs, Sarton proposed the establishment of an Institute for the History of Science. Its immediate objective was to produce a few standard works that would raise the level of scholarship so high that dilettante scientists who wrote their histories "with a complete lack of scholarly integrity" would have no serious reader. Behind this barrier, the Institute's staff would take on the preparation of massive and authoritative accounts of "the whole of objective and verifiable knowledge." Arranged hierarchically like the fathers of Bacon's Solomon's House, the staff, all of them humanists, would devote themselves to the study of "the most precious common good of mankind."

This good was the positive systematized knowledge that constituted science. While cleansing his stables, Sarton by no means abandoned the underlying assumption of the old scientist historians who had made their home there: in his view, the history of science should be devoted to the origins of secure natural knowledge, of facts and the laws that connect them, with no admixture of metaphysics. The material requirements of this non-metaphysical operation were considerable. Sarton's Institute would need an endowment large enough to pursue its investigations in peace, productivity and prosperity in the manner, Sarton suggested, of the Bollandists, who had been writing their stories of the Saints, as free from hagiography as science is from metaphysics, for over 400 years (Sarton 1948, 170–171, 173, 1938, 7–8).

Enter *Structure*

The view of science as systematized positive knowledge was defended most vigorously around the middle of the last century by the logical positivists. One of their main projects was an *International Encyclopedia of Unified Science*. Its second volume, on social science, carried a long essay on the structure of scientific revolutions. The essay's main purpose was to bring what its author called the "new historiography of science" to bear on the philosophy of science, that is, to destroy the foundations of the logical positivism that had initiated the *Encyclopedia*. In so far as it undercut the epistemology of the old historiography, *Structure* made common cause with Sarton's project of expelling amateur scientist-historians from the fold, and freed historians still trapped by the old paradigm that regarded current science as the inevitable product of the dispassionate, logical, unprejudiced, objective human mind. When Kuhn wrote *Structure*, the old paradigm had not yet surrendered to the new; and, like Sarton soliciting endorsements for the infant *Isis*, Kuhn had to seek much of the historical information he needed for his work of destruction from people whose histories he hoped to render obsolete.

By the new history, or new paradigm in the history of science, Kuhn meant the intellectualized approach of Koyré, in which ideas beget ideas immaculately and the historian teases out the knotted evolution of intellectual pedigrees with sympathetic understanding of the intellectual world in which they developed. The new paradigm won the adherence or endorsement of the new leaders of the history of science in the United States—I. B. Cohen, Sam Westfall, Henry Guerlac, Marie Boas, and, of course, Kuhn. But only he stayed true to it. Kuhn believed that his particular strength as a historian was the ability to get inside others' minds, read them, and report back confidently on what he found there. Few of us can or perhaps even wish to practice the disciplined necromancy needed to crawl around in the heads of the dead. Kuhn was perhaps the only student of *Structure* to gain from it the inspiration to compose so severe and narrow a book as *Blackbody Theory and the Quantum Discontinuity, 1894–1912*. Almost everybody who rushed into the vacuum *Structure* created by evicting philosophers had some social construction to push.

Although Kuhn deplored this unintended result, the advent of the constructivists had the important merit of accelerating the integration of history of science with general history. Koyré's accounts of immaculate conceptions, however useful in distinguishing among ideas, needed incarnation in time and space, in the social circumstances, programs and ambitions of those whose thoughts he analyzed so subtly. No doubt, the wider contextualization brought by the social turn reduced attention to the scientific ideas and constructs that Kuhn took to be the defining subject matter of the history of science. He worried that a sort of Gresham's law would take hold and the bad coin of constructivism drive out the good money of intellectual development. He would not be happy to read in the latest general work on historiography that "the Kuhnian model helped bring about a different kind of history fixed less on the detailed explication of past scientific ideas and more so on their social and cultural contexts" (Woolf 2011, 471). This correct judgment should not be read to mean that the history of science has dissolved into social history. I think that our historiography will show that there is still plenty of the good old coin around.

One way to keep the good money in circulation, to escape the degradation Kuhn deplored, is to brave the criticism of scientists. Just as general historians, especially of modern times, must endure the criticism of informed outsiders, so historians of science have the opportunity of exposing to scientists their reconstructions of episodes about which the scientists think they know something. Sometimes their interventions are salutary. An instructive example is the squabble in the late 1990s over a permanent exhibition of the place of modern science in the United States mounted at the Smithsonian Institution's Museum of American History. Its curators decided to emphasize applied science and especially its deleterious effects on the environment. Pesticides, pollution and weaponry occupied more space than the great discoveries that the scientific societies who paid for the exhibition thought appropriate. The scientists were correct in their criticism if not in their methods. For in their quite appropriate determination to avoid hagiography and include the wider ramifications of science, the curators had lost their balance and left out or downplayed science as most scientists had experienced it.

I take this story as a warning that the autonomy we may achieve by driving scientists, philosophers and other naturally interested people from our historiography of science comes with a risk. The ease of playing tricks on the dead increases with our distance from the time in which our victims lived. There are no professional societies except our own to protect the experiences and self-conceptions of historical actors in the remote past from obliteration by historians too eager to impose their own views or too lazy to go beyond them. We have a responsibility to the historical actors we create.

This consideration brings me back to the program of Sarton's unfulfilled Institute. The research tools he called for in 1937 have been created in numbers and to standards that he could not have imagined. The *Isis* bibliographies have doubled their size, from 2000 at the time of his death in 1956, to 4000 last year. The *DSB* and *New DSB* have answered his call for a biographical dictionary. He did not foresee archival projects like those mounted in quantum physics and molecular biology in the 1960s or the energetic collecting of the papers of scientists by universities, professional organizations and learned societies. The resources devoured by letterpress editions of the works of Bohr, Darwin, Einstein, Henry, Lichtenberg, and so on would have astonished him. Then of course there is the incomparable research tool of the web and the scanned documents to which it leads of which none of us had an inkling 20 years ago.

Sarton had higher goals than creating the instruments to make a science of the history of science and obtaining for it a dignified place in general history. He wanted to incorporate everything of any value in general history into the history of science. It may be, as he claimed, that the history of science is the history of civilization. Before undertaking to conquer civilization in general, however, we should be clearer than we are about the advantages of such a takeover to other civilized folk. So, again, what *is* our subject matter?

If the current issue of the *British Journal for the History of Science* is any guide, we haven't the slightest idea. The issue is devoted to "transnational science." There is nothing obviously wrong with that. But what is the science transnationalized? We learn from the editors that "science is constructed as a universal and international phenomenon" and that "the production of scientific knowledge should be understood as the result of a struggle between alternative networks competing for durability" (Turchetti, Herran, and Boudia 2012, 331). These assertions are either empty or scary. If science is a phenomenon, how does it differ from moonshine or a talking dog? If scientific knowledge is the result of a struggle between great networks competing to sustain themselves, how does it differ from market share? What is science? Here is the answer given in the conclusion to the collection on transnational science. "'Science' is something that is constantly being deconstructed and redefined, or, more accurately, dissolved" (Pestre 2012, 426).

Let us hope that we may recrystallize our identity through an account of the development of our profession—an account that meets our standards as scientific historians and that does not cause sympathetic bystanders to laugh. I trust that it will disclose that science is not a phenomenon, although it deals with phenomena, and that it is not a market share, although we may hope to retain and even enhance ours among the many divisions of history.

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Part 1: Personal Recollections

Chapter 1
The Nature of Scientific Knowledge: An Interview with Thomas S. Kuhn

Skúli Sigurdsson

The following interview was published in *Harvard Science Review* (winter 1990) [pp. 18–25] and conducted by Skúli Sigurdsson who at the time was a graduate student in history of science at Harvard University.[1] We publish the original copy by courtesy of the *HSR*. The interview tape was transcribed by Katrin Chua (then editor of *HSR*). The photographs of Kuhn accompanying the text were made by Skúli Sigurdsson and chosen by Kuhn himself (from a whole 36-exposures film). An abridged version of the interview appeared in Persian translation by Elaheh Kheirandish in *Science Policy Quarterly* (Teheran), no. 3 (winter 1993).[2]

Interview

[**p. 18**] *Thomas S. Kuhn, professor of philosophy at MIT, is among the most influential figures in the study of the history of science. He is perhaps best known for his theories on the historical growth of scientific knowledge, which proceeds in what he calls conceptual 'revolutions' or 'gestalt switches.' In this interview, Kuhn discusses the origins of those theories, prominent reactions to them, and their implications for scientific truth.*

HSR: When you were an undergraduate at Harvard, what was it in the sciences that fascinated you and other students of your generation? What made you choose physics in particular? And do you think these motivations have changed over the years?

Kuhn: I came to Harvard in the fall of 1940, terribly proud of having gotten in, only to discover later that I had been one of, say, 1000 students admitted, out of something like 1095 eligible applicants. Yes, situations have changed since those times! But there's a story that will speak to your question about changes in attitude that have arisen since the summer of 1940. I wanted to major in mathematics or

[1] Ph.D. 1991 / dissertation: "Hermann Weyl, Mathematics and Physics, 1900–1927."

[2] Skúli Sigurdsson thanks Nina Ruge for invaluable editorial help in the summer of 2014.

physics, simply because I had enjoyed them and been good at them. I came from a mathematical background, a theoretical outlook. I had taken both chemistry and physics in high school from a man who taught both. But while he knew the chemistry much better, I caught onto the physics. I remember suggesting consequences of what he taught us about the theory of heat, and he told me I was trying to fly before I could walk. But that theoretical turn of mind—theoretical, ontological, cosmological, what you will, but an interest in fundamental problems; that was what drew me to mathematics and physics initially.

Figure 1.1: Thomas S. Kuhn being interviewed November 1989 in his office at MIT; photographer: Skúli Sigurdsson; picture: 11.

So in the summer before I came to Harvard, I talked at length with my father about which of the two I should choose. And I have never forgotten what he said to me, because nobody would say it now. "If you have a strong preference for mathematics," he said, "then I certainly [**p. 19**] think that is what you should follow. But if you don't, perhaps it would be better to major in physics, because in mathematics, if you don't make one of the good universities, the only things to do are to be an insurance company actuary or a high school teacher; whereas in physics, I think there are a few other opportunities. Bell Laboratory and General Electric are very interesting places, and then there are some government positions, like the Bureau of Standards, or the Naval Research Laboratory." As I didn't have a strong preference, I majored in physics.

I don't think I need to comment on the sense in which the situation has changed since that time. And clearly, it's changed in motivation as well. It isn't that you don't have to like physics, or that most people don't, but you don't think that you're giving up a great deal today in order to pursue it. I didn't think I was giving up a great deal either, but the notion that physics was an area of expanding career opportunities was not one people had. There was a *New Yorker* article that appeared after WWII called "Farewell to String and Sealing Wax," in which Sam Goudsmit talked about the enormous changes arising from the institutionalization of physics. That sense of a string and sealing wax career in physics was not unrepresentative of the sort of thing we had at the time.

HSR: How did these changes affect your own studies?

Kuhn: Freshman year was 1940. There was a war on in Europe. Sophomore year, there was Pearl Harbor, and at that point, anybody in physics at Harvard was urged to concentrate in electronics, so as to prepare to help the war effort. So I took a lot of electronics at the expense of physics, and much less liberal arts than I would have liked. For physics and math were by no means the only subjects I liked, and I also had a considerable interest in literature.

I did my best to pursue those interests with some literature courses, and one very important course in philosophy—important, that is, in my own development. I was an editor of the *Crimson*, a member of an undergraduate literary society, that sort of thing. So there were real conflicts. I had the not uncommon problem of being reasonably good at and interested in things that went off on opposite directions.

Now I'm sure you're going to ask me at some point how I got out of physics, and one of the factors was that my interests had always been somewhat torn. But there's certainly much more to the story. After graduating, I wound up working at the Radio Research Laboratory, doing radar counter measures out of the top of the biology building. After about a year's work there, I went overseas to our advanced European Base in England, and there, worked mostly with the air force. We worked on technical intelligence problems, trying to learn about German radar installations, with an eye, of course, to jamming them; and on installing various sorts of equipment in aircraft. When I returned to Cambridge in the summer of '45, things were over in Europe, but not yet over in Japan, and I was uncertain whether I was going to be sent off to the Pacific to do the same sort of thing.

Those experiences were also part of the reason that my feelings towards physics as a career were gradually changing; I didn't find my war science terribly interesting. It's not out of the question that had I gone to Los Alamos as some of my contemporaries at Harvard did, and been working in that environment, I might never have left the field. I suspect I would have, and certainly have no regrets about having done it, but there was something in the fact that I found the sort of work I was doing something of a drag.

Figure 1.2: Thomas S. Kuhn being interviewed November 1989 in his office at MIT; photographer: Skúli Sigurdsson; picture: 23.

Then came a fortuitous situation—when I finally heard that I wouldn't be going to Japan, the fall semester was just about starting at Harvard, and there I was. So I went on and took my degree in physics. But increasingly as I continued my work, I wondered whether a physics career was what I really wanted. I was very conscious of [**p. 20**] the narrowing, the specialization required, and though I had no conclusion on that score, I was beginning to look for alternatives. No one of those seemed more attractive than the rest, until all of a sudden I was asked to assist President [James B.] Conant in teaching an experimental General Education course on the history of science, through readings of case histories. It sounded like a pretty good idea; it would be a good experience, a chance to work with the President of Harvard, and also my first exposure to history of science. So I grabbed the opportunity and found it fascinating.

At our first meeting, Conant turned to me and said "I can't imagine a General Education course in science that doesn't have something about mechanics in it. But I'm a chemist, I can't *imagine* how to do that! You're a physicist, go find out!" So I went out to learn something about the history of mechanics, and it rapidly became clear that if it was going to be a case history, it would have to be built around Galileo, since Newton would have been far too complicated. And to do that, I would have to learn something about what people had believed before Galileo. So I wound up looking at a series of monographs by Alexandre Koyré, called *Etudes galiléennes* [1939], and I started to read Aristotle's *Physics*. And the experience was enlightening.

What Aristotle could be saying baffled me at first, until—and I remember the point vividly—I suddenly broke in and found a way to understand it, a way which made Aristotle's philosophy make sense. It was that case history, and others, that in some sense first got me onto the idea of gestalt switches and changes in conceptual frameworks, which was to show up in the *Structure of Scientific Revolutions* in 1962.

I had this long-standing interest in philosophy. I had been reading a lot of elementary philosophy of science during the war—[Bertrand] Russell, [Philipp] Frank, and [Percy W.] Bridgman, though unfortunately not much [Rudolf] Carnap. And I also was mulling over certain ideas about scientific method that I'd happened upon while being trained in the sciences. There are certain implications about what historical growth of knowledge is that I felt deserved greater consideration. So this project seemed important, worth working on, and something that might be just the thing to take as an alternative to physics. And that's the story of how I got into physics, and how I got out of it.

HSR: In the *Structure of Scientific Revolutions*, you discuss the notion of conceptual changes in the development of scientific knowledge. As you've mentioned, it first arose during your struggle with Aristotle's *Physics*. What, specifically, did this understanding amount to? In what ways, perhaps, is it more than simply making a translation?

Kuhn: What I discovered in studying Aristotle was that a text required interpretation. And by interpretation I mean something similar to what was then quite well known in Europe (although I didn't know it at the time) as *hermeneutics*, but without all the claims of hermeneutics as a way to Truth. It was a way of reading texts, of looking for things that don't quite fit, puzzling over them, and then suddenly finding a way of sorting out the pieces. I had never heard of interpretation in that sense, for I'd never read any continental philosophy. But in reading Aristotle, I began to see what sort of physics this had been, and why it had been taken so seriously, which had not been in the least visible to me before. What I discovered was *not* the fact that you could translate, but rather *that you couldn't*. You can teach Aristotle, but you have to teach some part of his vocabulary in order to do it, and there's no way you can put that vocabulary in its entirety into the vocabulary you had when you came to the text in the first place. So it was untranslatability, rather than translatability that I increasingly saw in studying the history of science.

HSR: Since you published the *Structure of Scientific Revolutions*, there has been widespread reaction to it. In retrospect, what surprises you most at the responses? How do you see some of the misinterpretations of the book as being related to specific problems within philosophy or history of science?

Kuhn: I would first distinguish between philosophy of science and history of science. Mine was a historical approach. But what I thought was important in looking at [**p. 21**] the history was the notion of a revolution, the sort of rupture that the gestalt switch was intended to represent. I was talking about the non-cumulativeness of the development of knowledge, the problem with bringing an older science to the bar of judgment of a later one; about the inappropriateness of speaking of Aristotle as simply having made a mistake when he spoke of heavy bodies as falling faster than light bodies; about the sort of vocabulary I objected to, which took Aristotle as being merely false, the abhorrence of a vacuum as merely a mistake. I found something wrong with the standard way of grinding clearly bright and influential historical figures in the meat grinder of the categories or laws of a later science. These notions were not going to strike people who came to history primarily as historians, and philosophers were certainly going to have a lot to say about the issue.

Of the things that surprised me tremendously in the reactions to *Structure*, a major one was the talk about irrationality, for that was something that had never occured to me. I didn't know how the word 'rationality' functioned in philosophy of science. And so the notion that I was showing the irrationality of science absolutely blew my mind. I did spend substantial time and rhetoric in *Structure* discussing the quite different notion that when people talk about proof in the sciences, it isn't like proof in mathematics; that the former has none of the latter's force of compulsion. I was *not* saying, however, that there aren't good reasons in scientific proofs, good but never conclusive reasons. In formal mathematics, if two people disagree about this being a correct proof, we can take them through it one step at a time, and one of them can be forced to acknowledge the other side. There's just nothing like that in the sciences. That was what I was trying to say in *Structure*. So I was surprised at the extent of the reaction to it as a charge of science's irrationality.

> *I found something wrong with the standard way of grinding clearly bright and influential historical figures in the meat grinder of the categories or laws of a later science.*

I was also surprised at the relativism charge. Not that I didn't see why it was made, but it seemed to me that if relativism was what my thoughts amounted to, it was not nearly so damaging as the sort of relativism it was being taken to be. And it wasn't clear to me that relativism was the right word to be used at all. Essentially, I drew a Darwinian parallel in the first edition of *Structure*, to remind people that getting a better and better instrument (the hand and the eye were standard examples) does not require a process aimed at a pre-existent goal. Evolution isn't guided towards some preconceived perfect form, and I was arguing that science wasn't either. Now while it's clear why Darwin and the notion of evolution upset people, it wasn't clear at all that relativism was the proper charge to level.

HSR: This talk of irrationality became prevalent as the sixties drew on, against the backdrop of much criticism in American society of the Vietnam War. How do you see some of the responses to your book in light of these larger social movements and the criticism initiated by them?

Kuhn: I'm sure that part of the reason the book attracted the sort of attention that it did, particularly among people who were under thirty in the sixties, was for exactly those reasons. It could be used, and was used as a whip with which to beat the sciences. I am told that [Herbert] Marcuse and Kuhn were the heroes on the campus of San Francisco State. After all, that was my second book with the word "Revolution" in the title! I'm sure that *part* of what went on was due to those trends, and I had a number of relatively radical students who came along hoping that I would inculcate the new revolution or something, which I didn't do.

Evolution isn't guided towards some pre-conceived perfect form and I was arguing that science wasn't either. Now while it's clear why Darwin and the notion of evolution upset people, it wasn't clear at all that relativism was the proper charge to level.

I discovered that students who had been attracted to history of science because of this book didn't have a clue, and on the whole neither did my colleagues, as to where this book had come from. I taught people how to read texts, trying to replicate my experience with Aristotle. The people who *did* discover what I thought *Structure* was about were those who took graduate seminars with me, in which we read Kelvin or Maxwell or Galileo or whoever, [**p. 22**] closely, and tried to figure out how those people could ever have said the sorts of things they said. That's always been for me the central part of that book, and of course it scarcely shows. We asked, "Why would he say that?" We found things that didn't make sense, and tried to find a way of reading that would make it make sense. For it is only at that point that a text you thought you understood takes on a somewhat different significance.

> *I am told that [Herbert] Marcuse and Kuhn were the heroes on the campus of San Francisco State. After all, that was my second book with the word 'Revolution' in the title!*

HSR: How would you say your notion of *revolution* differs from more common connotations of the word, in particular, with respect to whether in studying the history of science, our aim is to deconstruct and undermine the basis of science's validity, or rather to reconstruct those foundations?

Kuhn: I was not trying to deconstruct science. I'm still not trying to deconstruct science. I'm not all that sure I understand what deconstruction is. But there's an important element that persists in me that Dr. Johnson's argument against Berkeley was right—that you can refute the person who doesn't believe in material bodies by kicking the stone. Experiment and observation really do play an absolutely crucial role in the development of the sciences. There are many things to be said about the nature of progress in the sciences; the thing that you *cannot* I think say coherently is that they get closer and closer to the truth. But that doesn't mean they don't have a coherent evolutionary development, that there aren't criteria with respect to which they can improve with time. But those are primarily instrumental criteria.

The sort of thing I now say, and was not very far from saying in the last chapter of *Structure*, is that truth, at least in the form of a law of noncontradiction, is absolutely essential. You can't have reasonable negotiation or discourse about what to say about a particular knowledge claim if you believe that it could be both true and false. One has to notice, however, how different all this is from a notion of truth which is a correspondence to something external to the logic, the theoretical system, the conceptual scheme. You have to split those two conceptions of truth quite wide apart, stop working back and forth as though this prerequisite for the sort of discourse which can sustain agreement on different points, which requires a law of noncontradiction and a corresponding notion of truth and falsity, were the same as a notion of Absolute Truth. The first thing is something one cannot get on without. But there are all sorts of ways one can go from talking about the relationships of older and newer theories without having to say the new one makes the old one false. I take theories to be whole systems, and as such they don't need to be true or false. All we need to do is by some criteria or other decide which one we would rather have. In general, this is roughly specifiable, but that doesn't get me into the true-false game. Of course, it doesn't eliminate true-false as very *important*. That's what you do *within* a system,—judge the truth or falsity of statements. *Across* a system you can't apply that sort of calculation.

HSR: Many people have argued that scientific theories are underdetermined by the evidence. That is, more than one theory could adequately account for any given body of evidence. Would you distinguish between that idea and your own, and to what extent do you think that notion can be taken? What about the argument that although many theories may be *adequate*, the nature of scientific gathering of data renders those theories far more determined than we might originally think? What do you think of the notion that science might after all be converging upon a sort of Truth?

Kuhn: I've never worried a lot about the underdetermination thesis, but I've no quarrel with it, at least in the weak form that a theory is underdetermined by any finite body of evidence. The stronger forms, however, seem to me vastly more difficult to prove or to make out than I think people usually take them to be.

One way of making the underdetermination point is to use something like Nelson Goodman's argument that it's always possible to generate an incompatible theory by redefining the terms of the theory from which you started, so that both account for the evidence you actually have. You can use his paradox that all emeralds are "grue" or "bleen," and ask how that's any worse a theory than the one that says emeralds are green or blue. I take those techniques to be available for argument, but also to be not quite to the point, though I would hate to have to say in exactly what respect they aren't! They are brilliant arguments and they're about something important, but they don't cut quite the ice that some people think they do with respect to underdetermination. Nevertheless, I think there's real plausibility about the underdetermination thesis.

However, what I don't find plausible are the arguments that say even with *all possible evidence*, the theories would still be underdetermined. The argument becomes [**p. 23**] problematic as soon as you start assuming such ideal situations, and at that point, I'm *unhappy* with the claim. Furthermore, if that's a reasonable unhappiness, then I simply want to say that I am uncertain what would happen to the argument, even with a *limited* amount of data, if I were allowed to have *total accuracy*; if I didn't have to take into account that data is always approximate and that it leaves a certain penumbra around itself.

I think it's at least possible that with full precision on the observations that I have, which is of course just as unavailable as an infinite body of potential data, then maybe I would not be able to find two equally valid theories either.

Considering all possible data, do you really get Kepler's laws from Newton's? Well, you don't quite. Do you get Galileo's law of fall from Newton's, well not exactly, just near the surface of the earth. So I'm not sure what happens to even this more limited version of the thesis, if one doesn't acknowledge that theories are only *approximately* the same, that the data is the best you can hope for within the limits of error.

That's the way I feel about the question of underdetermination, and although I don't quite want to say it's an entirely different ballpark, I don't think it has direct relevance to the sorts of things I was saying in *Structure*. There are two main sorts of people who talk about the underdetermination thesis. In Emerson Hall, [W.V.O.] Quine and [Hilary] Putnam both talk about it, and both of them would, I think, see me as being somewhat of an idealist. But then the strong program people also talk about underdetermination, in order to show that science has *no* content, and from that point of view I'm on the Quine-Putnam side. So the underdetermination thesis constantly gets talked about, but I can be heard as being on either side of it. I certainly don't think it's a *mistaken* thesis, though I think there are some things one would like to know about just how strong a thesis it is.

> *I take theories to be whole systems, and as such they don't need to be true or false. All we need to do is by some criteria or other decide which one we would rather have.*

HSR: In the early sixties you directed a project, the Archive for the History of Quantum Physics, where you and your co-workers conducted interviews with the scientists who had played key roles in the development of quantum physics. Why do you suppose you were chosen to direct the quantum project, what intrigued you most about it, and did the experience affect your view of the ideas set forth in the *Structure of Scientific Revolutions*?

Kuhn: I was asked to help direct the project because I had a PhD in physics, and was a known historian of science. I was not unique in that respect, but I was one of very few people who had both those qualifications.

I knew as a historian that scientists' recollections of their own work is quite bad historically; that they see themselves as having worked towards the thing they eventually discovered, although when you look back you find that in fact they were looking for something entirely different. So I did not expect that the interviews would produce the sort of information about sources of discovery that the physicists on the committee expected.

But I also knew that if you study the papers against the recollections of the scientists, you often find terribly important clues about the processes the scientists had gone through. Here's what the man says, here's what the paper says, and they're obviously incompatible. Now what could it be that leads him to *this* memory construct as opposed to some other? You often get clues that way. So that's what I thought would occur, and what surprised me, then, was the number of times I got simply "I don't remember […] How would you expect me to remember something like that."

Part of it, as a couple of scientists said fairly explicitly, was that trying to remember is uncomfortable, under these circumstances. The people who write autobiographies have made themselves go through the process, were motivated to go through it. But have somebody come in for five days with a tape-recorder, and they merely don't remember.

I would say that the project had substantially no effect on my views in *Structure*. I never thought that *Structure* was more than a highly schematic sketch. I did not expect any direct lessons. I've always said, assimilate this point of view and this way of doing it, and then see what it does for you when you try to write a history, but don't go out looking at history to see whether this is true or false, to test the ideas. The only test of the ideas, at least at this level of development, is going to be whether having assimilated those ideas, you see the material usefully different. But it's not going to be "Can you always locate the paradigm, can you always tell the difference between a revolution and a normal development?" It's not meant to be applied that way.

> *It is also the case that my concerns are ultimately much more with epistemology than with philosophy of science. I want to know what the nature of knowledge is.*

HSR: When the quantum project was undertaken, historians of science were generally not looking at contemporary science. Nowadays, the emphasis seems to have shifted from the eighteenth century to the late [**p. 24**] nineteenth and far into the twentieth centuries, where science itself has become a much larger, more complex and institutionalized enterprise, with many more texts and much more science to consider. How do you see the changes in history of science in terms of both the *Structure of Scientific Revolutions* and the quantum project?

Kuhn: I think the Quantum Physics project probably did play a role in the development of history of science, in that it labeled the existence of an archive publicly enough so that nobody could write on something without going to look at that material. It wouldn't have been respectable. It almost didn't matter whether the material was good or not. You establish a base-line which sets a level for scholarship, and it helps. I think work is being done not necessarily always very well, but probably with a higher level of responsibility to evidence than it would have been if that material hadn't existed. That's not meant to be a tremendously big claim, and it's not the reason I got into the project. But as I watched what happened later, yes, some people were attracted to twentieth-century stuff because that material was there, and I think it meant that anybody doing twentieth-century stuff had to look at archives, whether the material was in that archive or elsewhere. In that sense the project made history of science a more scholarly discipline.

Now the other question about how, when science gets as big as it has, can we know the texts—I don't know the answer. I see it as a question about practice, not a question about principles. I think the best study in conceptual change I've done is my Planck book [1978], although it's not always been viewed that way. And that doesn't *begin* to tell you about gigantic science. But it sure as hell presents problems of scale not found when working on Galileo, and it was still feasible to write conceptual history. I've never tried anything that gets to post WWII science, and I absolutely see that it's difficult.

But if you feel as I do that there are many more traces left of the stories than their authors and editors think there are, there are going to be clues. The problems are gigantic, but I'm not persuaded that there's nothing to be done about reconstructing conceptual change. How much of it can be done and in what ways, I'm not sure. But I think that whole business of looking for the things that don't make sense still applies.

John [L.] Heilbron and I wrote a paper about the genesis of the Bohr atom, which we started during the Quantum Physics project when we read Bohr's 1913 paper, in preparation for interviewing him. There were 2 or 3 passages in there that made absolutely no sense. Taken as a whole, the paper gives the Bohr model of the hydrogen atom on the one hand, and on the other, an atom with only a ground state, but in which the electron strums all the strings as it falls into the ground state from outside the atom. I don't think traces of that sort are going to have vanished. And they lead back through footnotes and other things into earlier papers, as the Bohr paper led back to a [C.G.] Darwin paper, which proved a very useful piece of background for understanding it.

It's also the case that my concerns are ultimately much more with epistemology than with philosophy of science. I want to know what the nature of knowledge is. I think science is an excellent thing to look at, if you're concerned with epistemology, and that's no novelty on my part—that has been going on since the seventeenth century when science provided epistemological examples. And with that interest, it doesn't make a whole lot of difference to me if things are now different. I see no reason to suppose that the things I think I have learned about the nature of *knowledge* are going to be disturbed by the need to change the theory of *science*. I could be all wrong with respect both to science and to the nature of knowledge, but I would make this separation to explain why I'm less concerned about the question "Is science changing?" than I might be if studying the nature of science weren't in the first instance simply a way of looking at the picture of knowledge.

> *I see no reason to suppose that the things I think I have learned about the nature of knowledge are going to be disturbed by the need to change the theory of science.*

HSR: How do the developments in the last thirty years which we have been discussing, bear upon your interests today? What are your current thoughts and projects?

Kuhn: What I've been working on for the last eight years [**p. 25**] and may be working on for the next five, is a book about the philosophical problems, especially incommensurability, left over from the *Structure of Scientific Revolutions*. I've been going back to the book and looking at whether in fact those thoughts I had are going to work with what's been going on in philosophy of science recently, to see how I can deal with those other ideas. As I've said, when I wrote *Structure*, I hadn't read much philosophy of science, and had no idea how much was going on in that field. I had seen what I thought was something important about the way science and conceptual frameworks worked, and that's what I was writing about. But today I would look at what Quine has to say, what Putnam has to say, and they both have a lot to say about science.

I *think* I see a way in which what I was doing in *Structure* might be made to take account of all that. But I'm not sure. It might be that in these contexts, the ideas in *Structure* will have to be revised, it might not. So I've been reading a good bit of that, and now I think I've got it all ready enough to begin writing. But of course when I write, well there's no guarantee that it will turn out the way I envisioned things when I started. I've learned that the greatest changes come about when the actual writing begins. But this is certainly another one of those books that will be a decade at least in the making, a decade or more, I can't say at this point. But that's what I'm working on now.

Figure 1.3: Thomas S. Kuhn being interviewed November 1989 in his office at MIT; photographer: Skúli Sigurdsson; picture: 10.

Thomas S. Kuhn is the author of:

The Copernican Revolution: Planetary Astronomy in the Development of Western Thought (1957)

The Structure of Scientific Revolutions (1962)

Sources for History of Quantum Physics: An Inventory and Report (1967) [co-authored with John L. Heilbron, Paul Forman and Lini Allen]

The Essential Tension: Selected Studies in Scientific Tradition and Change (1977)

Black-body Theory and the Quantum Discontinuity, 1894–1912 (1978)

Chapter 2
Steve's Question and Tom's Last Lecture: A Personal Perspective

Gerald Holton

I deeply regretted not to be joining you at the star-studded conference, but I shall respond here to the invitation to submit some remarks on the topic set out for us. Our discipline does indeed deserve attention to its own history, and your choice to center attention on Tom Kuhn's celebrated book *The Structure of Scientific Revolutions* of 1962 is eminently reasonable, since among the effects it caused was the resurgence of lively and wide-ranging interests, both in and outside our field.

But the announced topic of the conference also invites some reflection on the prehistory of Tom's book itself. I leave aside the well-discussed possibility that the timing of its composition and publication, without intention, was perfect at that historic period of rupture and national trauma in the 1960s. Also, the long reign of logical empiricism was running out of steam. Thus, for different reasons, many were looking for new paradigms.

Yet, there may also have been some important events in Tom's own life and thoughts leading up to and shaping the famous concepts in his *Structure*. On this possibility there have been some preliminary investigations; further study would be well within the project of a history of the history of science.

When your invitation reached me, I wondered on what specific aspect I could contribute. It occurred to me that I might, on this occasion, think about Tom's creative work in a personal way, being now perhaps one of the few who knew and interacted with Tom in those early days, for over a dozen years.

After all, we had some overlapping lives, intellectually, institutionally, culturally and socially. Born in the same year, we received our doctorate degrees in physics at about the same time, under brilliant and demanding scientists, in the same building (while Harvard University was only just abandoning its quota system with respect to admitting Jewish students). Jim Conant and his hugely ambitious General Education Program excited in both of us intense interest in the history of science.

We also publicly acknowledged our intellectual debts to many of the same powerful scholars (among the contemporaries, Koyré, Sarton, Merton, Nagel, etc., among those from whom we had courses or consulted, Quine, P. Frank, P. W. Bridgman, Van Vleck, Richard von Mises, Raphael Demos, etc.). We both took part in an informal workshop on how best to teach in this new field, under E. C. Kemble and including common friends such as the unforgettable Lenn Nash. Tom and I saw each other, and our families, at many gatherings, and we later corresponded, with Tom generously providing his opinions on some of my work. And not least, all of us were then bathed in the powerful local mythology, although with different reactions to it.

Moreover, we both grew up in a philosophical climate much indebted to logical empiricism; yet, each of us, although in different ways, turned to a very different position, yet in both cases centered on the role of predispositions.

So despite the complexities we all know may hide behind even close friendships, I feel that, for long enough segments, our lives moved along strangely parallel paths, especially during the period of our personal and professional maturing. That fact may give me some standing here, specifically in trying to help answer a persistent question about the history behind Tom's historical work.

That question was raised early and indirectly by Tom's friend and mentor, Harvard's President Jim Conant, in Conant's famous letter, in which he begged off to writing a preface to Tom's *Structure*, with Conant dismissing the conception of paradigm as "a magical verbal word to explain everything," and perceptively using the words "you have fallen in love," to suggest what may have prompted Tom's choice of his main concepts.

The inquiry became quite explicit in Steven Weinberg's essay, "The Revolution That Didn't Happen." While lauding many aspects of Tom's writings, Steve called the description of scientific revolutions "seriously misleading," insisting that changes in understanding nature "have been evolutionary, not revolutionary," and then asked: "What in Kuhn's life led him to his radical skepticism, to his strange view of the progress of science?"

In trying to provide an answer to this question, Steve shared a portion of a letter Tom had sent to him, in which Tom had written of having experienced a crucial "epiphany" around 1947, when he suddenly thought he could understand Aristotle's own mindset about the physics of that period, and so to speak slip into Aristotle's own paradigmatic preference. (Tom referred to the same incident also at other times.)

Tom's response to Steve is surely fascinating. But there may be other contributions to be made on this point. The time and place for one such additional insight came when Tom returned in November 1991 to Harvard to give his last lecture there, at his old home, launching in great style the new, distinguished annual Robert and Maurine Rothschild Lecture series, with his talk entitled "The Trouble with the Historical Philosophy of Science." Some analysis of that event may suggest how to reconsider Steve's question.

In this quest, one has to start with a fact, based on observation and readings, that Tom, while of course a world-class scholar, was internally deeply anguished. (This mixture in great figures is of course not unknown to us historians of science.) Part of his anguish was the result of his well-known shifting disciplinary identity. He first saw himself as a physicist, at a time when the Harvard Physics department was astonishingly flowering. The work of professors there such as Ed Purcell, Norman Ramsey, Julian Schwinger, Bob Pound and Van Vleck set the bar for good work to be done in this field very high indeed. For every graduate student who was inspired by this constellation there was likely to be another to feel discouraged. At any rate, right after having gotten his degree in 1949, Tom said later tersely, "I got out of physics."

His thesis adviser, Van Vleck, let it be known that this move annoyed him greatly, because Van Vleck thought he had wasted his time on his student. But now Tom could begin to train himself to become a historian of science under the auspices of Harvard's President, Jim Conant, co-teaching in an undergraduate course in General Education that centered on case studies of the seventeenth-century Scientific Revolution and its consequences. The profession was still quite young in the USA—there were few universities with history of science programs, Harvard having no such department for years to come.

Tom took his place as a historian of science with a book, meant for undergraduate-level courses, titled significantly *The Copernican Revolution*, though it was not published (in part because of Tom's meticulousness) until 1957. Meanwhile, in 1955, the possibility of a tenure appointment at Harvard was denied him by its Committee on General Education, reportedly because of Tom's then still thin publication record. Tom was fond of that university, and its refusal was a real blow.

Philosophy of science had been a side interest for Tom since his school days, but began to move to the center by 1952–53, when Tom looked for funds to have time for writing a monograph that eventually became the *Structure* book of 1962. Happily, the University of California in Berkeley offered Tom an Assistant Professorship in History of Science, located in both the Department of History and the Department of Philosophy. This arrangement illustrated his straddling of professional identities at the time.

Yet, this arrangement soon caused a deeply upsetting event. As late as 1995, Tom reported in an interview, "a quite destructive thing happened" and "I was extraordinarily angry, as you can guess, and very deeply hurt. I mean that's a hurt that has never altogether gone away." What happened was that when Tom's appointment to a full professorship came up, the Philosophy Department at Berkeley specifically opposed Tom's membership in that department.

From his perspective, he had left physics early, had become a sound historian of science, but his final, public turn into a professional philosopher of science had been questioned in a manner that was hurtful for the rest of his life.

However, there was a way left for him to clearly establish his credentials in the field, although there too the bar was very high (one thinks of Quine and Putnam back "home," and others elsewhere). This possibility, on which he had been working on and off for years, came into full view at Tom's last lecture at Harvard, at the Rothschild Lecture.

Tom began his talk by confessing that the "transformation" of the "image of science," which he thought he had helped to bring about, troubled him because some of his concepts had been used and developed by people who called themselves "Kuhnians," although he regarded their viewpoints as "damagingly mistaken." He was pained to be associated with their misunderstandings. In this feeling he was not alone. There were others who had reached astonishing popular success but suffered the same sort of pain. For example, Bridgman, in a publication in which he reassessed his own writings in the philosophy of science, confessed that regarding "this thing called 'operationalism' [...] I feel that I have created a Frankenstein, which certainly got away from me."

Next, in his lecture, Tom announced that he was currently at work on a new book, "a far larger project," devoted to "a theory which I once called incommensurability," although he regretted that in this talk he could not give details. But, importantly, in this talk he would speak "as a philosopher." A key point was that "for a philosopher who adopts the historical perspective, the problem is [...] understanding small incremental changes of belief" (rather than preoccupation with evaluation of belief itself).

The use of the word "small" in that sentence prepared one to expect next his revisiting his conception of large changes, such as Revolutions. Instead, to my surprise, Tom went into the opposite direction, saying that "scientific development is like Darwinian evolution." He elaborated this viewpoint with his use of related conceptions such as "evolutionary tree" and "speciation."

Of course Tom had briefly touched on evolutionary models toward the end of his *Structure* book of decades earlier, but in the context of chapters there with headings such as "Progress through Revolutions" and "Revolution and Relativism." No longer. Now his evolving view—he called it "reconceptualization"—had brought him, as he declared at the end of his talk, to the need to reinterpret the main parts of his previous thoughts. That, he announced, would be found in the new, to-be-expected work, where, as he put it, "the answer is incommensurability." Much of Tom's promise of a reconceptualized and reinterpreted version of his previous conceptions—as well as his analogy of scientific development with Darwinian evolution—would have appealed to previous critics like Steven Weinberg (and there had been many others). But the proof of the promise had to wait for the book.

One could feel that once more the stakes were high for Tom. Speaking explicitly as a philosopher, his standing in that profession would now hinge on the new work, of which he could give us in his lecture only hints. Tom talked about this important project also later (for example in a long interview, published in 1991). But in the end he was not able to publish the work. And that, in my view, was a chief source of Tom's internal state of dismay, especially in his last decade, as he was trying to reach the new, high professional identity level he had set for himself.

He had always been hard on himself, and had been through the harsh school of making himself anew—physics, history, philosophy—each time with his characteristic, impeccable honorability. As he told his interviewers in October 1995, less than a year before his death: "I am an anxious, neurotic." Sadly, it was worse. There are good reasons to think that near the end of his career Tom considered himself to have been a failure.

Tom would be the only one who would make such a severe judgment. On the contrary, as illustrated by the persistent, widespread attention being paid to his work, his distinguished place in scholarship is secure.

Chapter 3
Thomas Kuhn: A Man of Many Parts
William Shea

I cannot claim to have belonged to the inner circle of Thomas Kuhn's friends, but I was occasionally privileged to see him outside the limelight in which he was compelled to bask. Allow me to recall two incidents, one when he was very angry, and the other one when he was greatly amused. The first one occurred in the 1970s when I happened to accompany Tom to a European university where he had been invited to give a lecture. We were met at the entrance to a large auditorium by the organizer who told Tom that he would escort him to the front row. I attempted to stay behind (front rows always intimidate me even when I am the guest speaker) but Tom insisted that I stay with him and that we pursue the topic we were discussing. When the chairman went to the podium to introduce him, I glanced behind me and saw that there were about a thousand people eagerly awaiting his appearance.

The talk was on one of Kuhn's varied attempts to render incommensurability commensurate. I had expected that it would be followed by the usual question period. But no—! the chairman informed us that we would now hear three "brief" comments. The first speaker rattled on for twenty minutes on how some people might think that Prof. Dr. Kuhn had fallen into "a deep well" of uncertainty, if not contradiction, but that they need not worry because he was going to get him out of there. When the second post-mortem speaker was announced, Tom turned to me and said, "Let's get out of here!" I pretended not to hear but he repeated, "Let's get out of here," in a louder voice. I had my misgivings but I whispered, "Okay." He sprang to his feet and I sheepishly followed him to the entrance of the auditorium in the hope that the audience would assume that we both had a prostate problem and were badly in need of the bathroom. When we reached the lobby, Tom exploded: "I can't stand it anymore! I have become a sounding board, an opportunity for people to preach their own ideas under the guise of discussing my own. I can just hear them saying,"—he added with a suitably professorial tone of voice—"Kuhn is not sufficiently bold or clearheaded!"

Tom felt that he was caught between competing teams who had only one thing in common: their determination to point out where he had gone wrong.

Sociologists thought that he did not go far enough and philosophers asked whether he knew where he was going. “I usually keep my calm,” he added, “but enough is enough!” I believe this emotional outburst tells us something important about this great man, and the pain that he endured at the hands of people who damned him with faint but apparently loud praise.

If Tom could unexpectedly become very angry, he was also capable of greatly enjoying a joke. A couple of years after the meeting I have just mentioned, I attended a small gathering in Sweden where the guest speaker was Tom who, after giving a splendid lecture, remained to hear a colleague who gave a talk on a topic that Tom was exploring at the time: the claim that history and fiction obey the same rules. Tom thought the illustrations were hilarious. He burst out laughing several times during the coffee break, muttering things like “great joke, great truth.” I had never seen him in this excited state and I never saw a repeat performance. Since it sheds light on his personality, let me attempt to reconstruct, however badly, the story that he found so funny.

The speaker wanted to illustrate his claim that to be credible, history must comply with the rules of fiction, and he referred to Nancy Partner’s delightful essay, “Making Up Lost Time: Writing on the Writing of History,” in which she enlists the aid of P. G. Wodehouse, a writer dear to Anglophile academics, including Tom and myself. The hero is a man named Jeeves but the narrator is Bertie Wooster, who is also the fictional author of his own adventures. The part that amused Tom hinges on one sentence: Bertie has just entered the drawing room of his aunt Dahlia who is reading a Rex Stout detective story. Here is how Bertie describes his aunt’s reaction: “Oh, it’s you,” she said, “which it was of course” (Wodehouse 1971, 128).

This fleeting joke brought a smile to Tom’s lips. He grasped the serpentine implications of this one line, or rather of the five words, of the relative clause (“which it was of course”) because the elusive but lingering funniness does not turn solely on the simple joke of a narrator so fluffy-minded that he has to assure his readers that he is, in fact, identical with himself. It punctures, as Nancy Partner puts it, “the fundamental conventions of narrative” and the ways in which language establishes a continuous world of concordant identities.

> Silly Bertie points to himself, a thing of words, here a few equated pronouns: “It’s you […] which it was […]”, and *we* fill in the fictional reference and smile at Bertie’s dimwit literalness (which he literally is)—and the joke is on us and our earnest assent to fictional reality. We know so certainly that this Bertie, who has just walked through aunt Dahlia’s house and entered her drawing room, enjoys a continuousness of identity just like our own. The final, “of course”, has just the right note of fatuous emphasis, a conversational tic gone

> wildly wrong if connected to a statement turning on the essential condition of human identity […] Here in this narrative within a narrative—for Bertie is the fictional narrator of his own inventions, and Bertie is Wodehouse's fiction—Wodehouse, as author, takes only the dumb joke for himself, that Bertie is a bit of a twit, and generously allows Bertie the witty joke on our eager gullibility to have the printed page merge so seamlessly with our own sense of reality. So aunt Dahlia looks up from one light fiction to encounter another: "Oh, it's you", she said. Which it was of course. (Partner 1986, 98–99)

Now what can this explication overkill have to do with the serious business of historical writing? The speaker gave a number of examples but the one that struck Tom concerned William the Conqueror while he waited for the wind to change so that he could set sail from France to England in 1066. Contemporary writers describe his supplications for a change in the weather, and picture him as constantly gazing towards the vane of the church of St. Valérie. The speaker suggested that the historian might want to add something to the description of William waiting for a favorable channel wind. He offered us three choices:

First choice: William felt secretly anxious because he did not know how to swim.

Second choice: He began to embroider a nice tablecloth with scenes depicting his connection with the English monarchy.

Third choice: He experienced frustration and impatience.

Normal professional logic can countenance only the third, "He experienced frustration and impatience." The second choice, "He began to embroider a nice tablecloth with scenes depicting his connection with the English monarchy," is too interesting to even consider, while the first choice, "William felt secretly anxious because he did not know how to swim," is dismissed because contemporary writers did not say that he did not know how to swim.

The moral is perhaps that historians should not become guilty of what Thomas Huxley called "plastering the fair face of truth with that pestilent cosmetic, rhetoric" (Chesterton 1913, 39). I can only guess that Tom would have said that Huxley was laying it on a bit thick, and he would have enjoyed our chuckle.

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Part 2: Historicizing Kuhn

Chapter 4
An Episode from the History of History and Philosophy of Science: The Phenomenal Publishing Success of Kuhn's *Structure*

Kostas Gavroglu

Introduction

One of the most intriguing issues in the history of history and philosophy of science would be to examine how and why some historians and philosophers of science and their work have been able to become (well) known outside the relatively narrow circle of historians and philosophers of science, and of some scientists. Karl Popper and, especially, his views about falsification is such a case. Another case is Thomas Kuhn, his *Structure of Scientific Revolutions* and the notion of paradigm. That Kuhn has become a household name among many communities of scholars, and importantly, among large numbers of people who do not necessarily invoke a professional reason for their interest in Kuhn, is something that many of us have repeatedly witnessed. How did a book which, at the time of its appearance was torn apart by its critics as being philosophically sloppy and historically naive, become one of the most quoted and sold books of the twentieth century? How can we go about examining such an issue? What would be the criteria in articulating a *plausibility argument* for understanding such a success outside the confines of relatively well-defined disciplinary boundaries? I would argue that to understand the phenomenal success of the book, one would have to identify the characteristics of the overall social and political context *after* the book was published, explore *how* the book was publicly perceived and what it was in *Structure* that resonated with the agendas of those seeking alternative practices and approaches in many social domains in the 1960s and 1970s.

There have been many works that attempt to situate Kuhn in the context of the period he worked in and to understand how *Structure* was formed. Perhaps, the strongest thesis is that of Steve Fuller, who argues that *Structure* is an "exemplary document of the Cold War era […] [and Kuhn] a normal scientist in the Cold War political paradigm constructed by James Conant" (Fuller 2001, 5). Fuller examined the ways the Cold War conditions, especially those that were so prevalent at Harvard and initiated by Harvard President James Conant, to whom Kuhn dedicated *Structure*, formed its basic tenets. George Reisch in his book *How the Cold War Transformed Philosophy of Science*, acknowledges that he "owes much to Fuller" (Reisch 2005, 229), but also diverges from him, especially in the ways he contrasts the physicist Philipp Frank's and Kuhn's views about physics, physicists and philosophy of science. He argues that *Structure* spoke persuasively to intellectuals and scientists because "professionalization tended to come with not only epistemic legitimacy but job security" (2005, 233). For Reisch, *Structure* appealed to scientists of different disciplines because it showed the way to intellectual success and that the "path to job security and freedom from political attack were one and the same" (2005, 233).

Another work, perhaps one that bears the most direct relevance to what I shall try to argue in this paper, is Jon Agar's *Science in the 20th Century and Beyond* (2012). According to Agar, in the long 1960s (starting in the mid-1950s and ending in the mid-1970s) science and scientists featured in social movements in three kinds of relationships.

> First, certain scientists and sciences were objects of criticism because they were seen within social movements as tools of their opponents. Second, places where science was done became theaters for social movement demonstration. Third, scientists as activists were contributors to social movements. This third relationship took two forms: their science could be incidental to their involvement in a movement or, most significantly, it could be the cause, the tool, the object and subject of activism. (Agar 2012, 404–405)

The work provides an admirable overview of what the title promises, but, also, it analyses many episodes that had been rather decisive in questioning the prestige of science and its authority. It was through these episodes that a critical discourse against the dominant scientific practices had been articulated. According to the author, this period, among other things, was characterized by conflicting expert testimony in the public sphere which brought forth all the ideological, political as well as the methodological difficulties concerning the discussions about knowledge claims. Furthermore, Agar argues convincingly, for the generation growing up in the 1960s, the images of science and technology were 'contradictory.' This "generation were free to enjoy benefits (domestic technologies, 'high-tech music', synthetic drugs) while consuming critical texts (Kuhn, Feyerabend, Carson, Ehrlich, Commoner, Illich, Schumacher) and recognizing the 'loss of innocence' of science made vivid by anti-nuclear and anti-Vietnam movements" (2012, 429).

What will be attempted in this essay is more akin to the exploration of the career of the book itself. Almost axiomatically, the impressive publishing record of *Structure* (which, having sold almost two million copies, constitutes a unique case in the history of history or philosophy of science) cannot be understood solely with respect to the appeal the book may have had among professors and instructors in the humanities and social sciences, or its inclusion in the reading lists of undergraduate and graduate classes. Neither the ambivalence of some philosophers (and to a much lesser degree historians) of science, who stressed some merits of the book, nor of course the references in the early works on social constructivism, can explain its huge success. Thus, my argument will not be based on those who liked and who strongly criticized the book.

Such a phenomenon needs to be understood in terms of the *public perception* of *Structure*. In what follows, I shall attempt to explore the possibilities of correlating the book's phenomenal success with various events that took place especially in the USA, but also in Britain and, to a lesser extent, continental Europe during the period 1962 to 1969, which is the period between the two editions of the book. Though the Cold War created an all-encompassing ideology and mentality, it may be instructive to note that during the same period there were serious deviations from this hegemonic ideology. During the Cold War era, there were a lot of events and initiatives whose theoretical articulation and practical repercussions clashed with the Cold War mentalities, seriously questioned the status-quo and attempted to propose different alternatives for many aspects of everyday life, be it in industrial production, scientific research, education, the role of women, the emancipation of black people, etc. During the 1960s and 1970s, a large number of scientists became seriously disillusioned with the ways in which science was practiced; they aired their varied criticisms and sought to formulate different alternatives. At the same time, many social and political events brought to the surface the deep grievances of the black community, as well as women and young people who were demanding these issues take center stage. A number of books, which I shall be discussing later on, argued persuasively for radical reorientations in a wide spectrum of academic disciplines, as well as in mainstream social and economic practices and, a few years after they appeared, became standard reading. It may not be unreasonable to argue that a book with such a suggestive title as *Structure*, and publicly perceived as a scathing criticism of the received view about philosophical issues associated with science and its history, could have become a reference point for many of those dissatisfied with the practices of the time.

In this paper I shall attempt to put forward such a plausibility argument (and it is, at best, a plausibility argument). *Structure* is a book that has been discussed and bought by many more people than its originally intended audience and in the process became a kind of cultural icon and a "must-read" for people with a wide range of interests. The book is one of the bestsellers of the twentieth century, as well as the most cited book in the humanities. Though the book appeared in the reading lists of courses on a wide variety of subjects, what happened exclusively within academia cannot be the only explanation for such a success.[1]

But what kind of book is *Structure*? There are, surely, arguments to classify it as a book on the philosophy of science, yet some prefer it as a book on historiography, while others consider it a precursor of the new sociological approaches to the history of science. Strictly speaking, the book does not "belong" to any of these categories. In this paper, the book will be regarded as a long essay about science, as a book perceived as having all the elements of philosophy, history and sociology of science, yet not written with the heavy terminology of these disciplines. It can surely be regarded, even by professionals, as a book that discusses what science "was all about." For many, it was a book that was easy to understand; it emphasized the significance of collective work for the development of the sciences and, importantly, it discussed the grand scheme of things.

It may thus be worthwhile to distance oneself from dilemmas about the "true" nature of the book, and instead examine how the book has been perceived by the wide audience of people whose experiences as citizens made them realize, if only dimly, that perhaps the scientific enterprise was not as "innocent" and "straightforward" as generations of teachers have insisted. Thus, by distancing ourselves from the theoretical issues dealt with in the book and the subsequent reactions by philosophers and historians of science, and seeking to understand the social and ideological context within which such a book made its presence felt, we may gain additional insight into the success of the book. It may also help us to understand the social history of the book itself: not its influence within a rather narrow group of philosophers (and to some extent historians) of science, but the conditions within which the book became what it became.

My inclination is to think of the book as emerging twice: in 1962 (date of first publication) and in 1969 (date of second edition which included the epilogue/postscript).[2] The book's presence was felt among philosophers of science some-

[1] From Kaiser (2012). Concerning the book's citations, see Garfield (1987) and Owen Gingerich, email to the author, November 6, 2009 (on the book's dominance across Harvard's curriculum).

[2] In 1964, two years after the publication of *Structure*, Kuhn left Berkeley to take up the position of M. Taylor Pyne, Professor of Philosophy and History of Science at Princeton University. In 1965, an International Colloquium in the Philosophy of Science was held at Bedford College, London. One of the key events of the colloquium was to be a debate between Kuhn and Feyerabend, who, however, could not attend because of illness. John Watkins replaced Feyerabend, the session being chaired by Popper. In the discussion, after the papers were delivered, Popper, Margaret Masterman and Stephen Toulmin severely criticized the book. Papers from these discussants along with contributions from Feyerabend and Lakatos were published several years later in *Criticism and the Growth of Knowledge*, edited by Imre Lakatos and Alan Musgrave (1970). A few months earlier in 1969, the second edition of

time between its first and second edition when the spokesman *par excellence* of the established order in philosophy launched an attack against Kuhn. Karl Popper, whose *Conjectures and Refutations* was published a year after *Structure*, ever so confident that his own views were exempt from the criterion of falsificationism, in the 1965 conference found nothing right with Kuhn's views. But by the time the *Proceedings* of the conference appeared in 1970, Kuhn had already incorporated his responses to these criticisms in an epilogue/postscript to the second edition of the *Structure*. It was as if the second edition came to complete what the first edition had started. "Something happened" to the first edition and what happened was reflected in the second edition. Notwithstanding the pronouncement in the 1964 *Scientific American* review of *Structure*, that the book was "much ado about very little" (Anonymous 1964). *Structure* was there to stay, having received an uncanny and certainly unusual blessing by the old school: the strong criticism the book received, especially by the politically conservative Karl Popper, "turned" the book into one of the reference points in the trade, providing it with an "anti-conventional" aura. Kuhn's new ideas could not have been ignored, since they undermined—even in a philosophically naive way—the very fabric of the received view. The strong criticism the book received had an additional, yet peculiar, side effect: this long essay about science was also perceived by many within academia as a diatribe against logical empiricism—something that experts knew was not true—and such a perception reinforced its popular expositions that stressed its revolutionary character.

The Public Perception of *Structure*

When discussing such widely circulated books (and not only scholarly ones), one should always be aware of the difference between the character of the consensus among the specialists and experts about the merits of the book, and the social perception of the book. The two are not necessarily identical, and may not even be consistent with each other. The social perception of *Structure* has resulted from the complex mechanisms that shaped the circulation of knowledge about the book: the serious, and less serious, popularizations of the book led to an amazingly large number of people apparently knowing "something" about the book and having an "idea" of what the book was about, without having necessarily read the whole or even parts of the book. Scientific popularization is neither impervious to what is happening in the wider social context nor is it a process where every aspect of what is being popularized is carefully scrutinized by those who popularize it. The social perception of such books is the result of eclectic

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Structure was published with an important postscript. It was in this postscript that Kuhn incorporated his answers and clarifications (especially about the notion of paradigm) in response to his critics.

presentations and the ensuing discussions of what is projected in these books as being the "relevant" aspects of the subject matter. Hence, the public perception of such books appears to be tandem with various social and political prerogatives of the time, rather than exclusively academic or disciplinary ones. Indeed, in the period between the two editions of *Structure*, we do witness a number of such social and political and social prerogatives.

One of the best ways to get a feeling about the public perception of *Structure* is by looking at Kuhn himself. In his interview with John Hogan in 1991 for *Scientific American*,[3] he reminds the interviewer that as he had often said he was "much fonder of my critics than my fans." Kuhn recalled a student thanking him for telling "us about paradigms. Now that we know about them, we can get rid of them." In one seminar, he experienced both students and the professor discussing "how [his] book denied truth and falsity." And when Kuhn tried to explain that within the framework of a paradigm such concepts were, in fact, necessary for the scientists' work, the professor intervened and told him "you do not know how radical this book is." There were instances when things got out of control: "I get a lot of letters saying, 'I've just read your book, and it's transformed my life. I'm trying to start a revolution. Please help me,' and accompanied by a book-length manuscript" (Horgan 2012).

In fact, it has been often noted by anyone who talked to Kuhn that he was greatly distressed by all those who opposed science, and especially in the 1960s, thought they had found an ally for "pure experience" in *Structure*. Kuhn himself had acknowledged that many people thought that science is nothing more than power politics, triggering strong reactions on his part. In addition to all the "misunderstandings" the public perception of his book brought about, another aspect of it made it particularly welcome to many who were becoming uneasy and critical with what had been going on around them, be it in science or politics. Though the notion of progress in the sciences was not free of problems, Kuhn had given it a rather intriguing twist. Science was surely progressing—it was changing—but it was not evolving toward *the Truth*. Hacking expressed it rather succinctly: "he just thought that progress wasn't 'to' something. It was progress away from what didn't work very well, but that there isn't any kind of permanent goal."[4] Such a viewpoint was indeed a radical alternative to the unidirectional notion of progress that was such an integral part of the hegemonic ideology and against which there were, at the time, such strong reactions among many segments of American society.

Concerning the public perception of *Structure*, Kuhn himself was even more forthcoming in one of his interviews:

[3] Interview of Thomas Kuhn with John Horgan, (1991).
[4] Interview with Ian Hacking by Gary Stix, (2012).

> I mean, a lot of the early audience [for SSR] was social scientists …I gradually realized that a lot of the response was coming from social scientists. I thought of the book as directed to philosophers. And I think not a lot of them read it, I think it was picked up much more widely than that […] The sixties were the years of the student rebellions. And I was told at one point that, Kuhn and Marcuse are the heroes at San Francisco State University. Here was the man who had written two books about revolutions, and students used to come to me: it's "thank you for telling us about paradigms, now that we know what they are we can get along without them." All seen as examples of oppression. And that wasn't my point at all! I remember being invited to attend and talk to a seminar at Princeton organized by undergraduates during the times of troubles. And I kept saying, "But I didn't say that! But I didn't say that! But I didn't say that!" And finally, a student of mine, or a student in the programme who would sort of help get me into this and had come along to listen said to the students, "You have to realize that in terms of what you are thinking of, this is a profoundly conservative book." And it is, I mean, it was in the sense that I was trying to explain how it could be that the most rigid disciplines and the most authoritarian could also be the most creative. […] So, it's hard to say how I felt. I thought I was being, I want to say badly treated, badly misunderstood. And I didn't like what most people were getting from the book […].[5]

Throughout the 1970s and to a certain extent 1980s, the book met the fate of what Copernicus, the hero of Kuhn's first book, wrote at the beginning of *De revolutionibus* in his letter of dedication to the Pope. In order to convey the arbitrariness of the hypotheses astronomers of the Ptolemaic tradition used for their calculations, Copernicus likened them to someone who would

> [C]ollect hands, feet, a head, and other members from various places, all very fine in themselves, but not proportionate to one body, and no single one corresponding in its turn to the others, so that a monster rather than a man would be formed from them.[6]

The public perception of *Structure* and the way the catchword paradigm has been (ab)used may have had monstrous overtones for professional philosophers of sci-

[5] Interview with Thomas Kuhn by Aristides Baltas, Kostas Gavroglu and Vaso Kindi, Kuhn (2000, 255–323).
[6] From the Letter of Dedication to Pope Paul II, Rosen (1992, ix-xii).

ence and for Kuhn himself.[7] Nevertheless, the perception of the book by an impressively large audience as a book proposing alternative ways for science gained a dynamic of its own.

The Title

The book had the catchiest of titles. Every word triggered all kinds of connotations in the new realities being formed in the 1960s when science and revolutions were strongly present in the public discourse of, at least, the English-speaking world. *Structure* was less conspicuous in the public domain, yet its meanings and repercussions were strongly contested in the academic environments, at least in the French-speaking world.

The notion of "structure" or its more formal expression "structuralism" had a rather insistent presence in academia and had been a source of major re-orientations in various fields: linguistics, psychology, sociology, economics, literary criticism, architecture and, of course, anthropology have all had a rich history of discussions concerning the possibilities opened up by a structuralist approach to each discipline. Given the difficulties involved in defining such approaches, Simon Blackburn's suggestion succinctly captures most of the characteristics of structuralism as a viewpoint and as a research program: "the belief that phenomena of human life are not intelligible except through their interrelations. These relations constitute a structure, and behind local variations in the surface phenomena there are constant laws of abstract culture" (Blackburn 2008, 322).

Though structuralism was originally put forward in linguistics by Ferdinand de Saussure at the beginning of the twentieth century, it was the work of Claude Lévi-Strauss that from the early 1950s and throughout the 1960s rekindled interest in structuralism. 1962, the year *Structure* first appeared was also a "very good" year for structuralism. It was the year Lévi-Strauss published his seminal work *La Pansée Sauvage*.[8] Though of a different orientation, 1962 was also the year Jürgen Habermas completed his *Strukturwandel der Öffentlichkeit. Untersuchungen zu einer Kategorie der bürgerlichen Gesellschaft*, where the notion of structure featured prominently and where he articulated the concepts that have been so central to political theory ever since (Habermas 1991).

[7] "Today, you can purchase audio and video equipment from Paradigm Electronics in Ontario, Canada; you can buy bonds and stocks from Paradigm Financial Partners in the UK; you can obtain solutions to your human resource problems from Paradigm Shift Consulting Service, Ltd. In India; or—best of all—you can read a provocative Paul Krugman op-ed piece in The New York Times entitled 'The Ponzi Paradigm'," Goldstein (2012, iii).

[8] French edition 1962, English edition 1966.

During the period between the two editions of *Structure*, Noam Chomsky would strongly criticize the structuralist approach in linguistics. His classic books in linguistics were published in the 1960s, all borrowing from the findings and arguments of his first book *Syntactic Structures*, published in 1957. Kuhn joined the MIT in 1979 where Noam Chomsky was already working. Not only did they share the WWII bunker, where their offices were long situated, but also a particular notion, appearing in the titles of their hugely successful works, although they differed on the emphasis of paradigm shift. Jean Piaget, however, had a different view. In his *Structuralism* of 1968, he found a parallelism between Kuhn's notion of paradigm and the notion of episteme proposed by Foucault in his *Order of Things*, which saw seven editions between 1966 and 1967. By the late 1960s, about a decade after Lévi-Strauss was appointed as Chair of Social Anthropology at the Collège de France, a group of French scholars would initiate a systematic criticism of structuralism, building a well-argued conceptual framework while vying for a rather strong presence in the debates about the social sciences and fighting for a hegemonic presence in their academic settings. The arguments of M. Foucault, J. Derrida, R. Barthes and L. Althusser articulating the post-structuralist framework were commanding ever larger audiences and these discussions were also echoed in the USA where, in 1966, a conference was organized at Johns Hopkins University with Derrida, Barthes and J. Lacan among the main speakers.[9]

Science was surely a structured set of beliefs, and logical positivism, so dear to the hearts of most practicing scientists, was a program to unfold the logical structure(s) of science. In 1961, a year before the appearance of *Structure*, Ernest Nagel's *The Structure of Science* was published. It was a book squarely within the tradition of logical positivism. Both books appeared when the set of ideas around structures were starting to be intensely discussed in academic circles. The two *Structure(s)* symbolized, in a way, the end of one era and the beginning of a new one, at least in the ways many philosophers, but not only philosophers, viewed science. If Nagel's *Structure of Science* was rather static and dealt with science ahistorically, Kuhn's *Structure* was forward looking and signified that change (in the form of revolutions) had a structure as well.

In the period between the two editions, the *problematique* concerning structuralism underwent a deep metamorphosis yet, at least in the USA, most of the repercussions of these discussions were basically confined to academia. It was what "happened" to the other two words in the title—science and revolution—that proved absolutely decisive for the book's success.

[9] See Macksey and Donato (2007).

Science

The manifesto of *Undercurrents: the magazine for radical science and the people's technology*, founded in 1972 and published in London, captured rather succinctly the climate of the times:

> Science, we feel, has largely abandoned its original "quest for truth"—if the phrase today sounds naive, it is a measure of that abandonment. *Undercurrents* believes it is possible to evolve a 'sadder but a wiser' science, a science that is aware of its limitations as well as its strengths which will search the hitherto ignored areas of human experience for clues to more meaningful and relative synthesis than is dreamt of in our present philosophies.[10]

One can speculate that if such a manifesto had been written ten years earlier, it would have had almost no audience. The 1960s, however, witnessed serious cracks in the perception of science as a process of a continuous accumulation of new and useful knowledge to be exploited for the benefit of humanity. Many social phenomena appeared to be undermining such an image of science, with serious repercussions. The shocking effects of pesticides, the involvement of scientists in planning the atrocities of the Vietnam War, the renewed discussion (and in many cases application) of lobotomies as a means of containing violence, the energy crisis and the realization that there may be non-reversible environmental damage caused by humans slowly started to mar the image of science.

The first edition of *Structure* in 1962 coincided with the publication of Rachel Carson's *Silent Spring*, another eye-opening book that concerned the environment, whose self-sustainability until then had hardly been questioned (much like the characteristics of science which, before *Structure*, were almost universally thought of as self-evident). *Silent Spring* exposed the amazingly harmful effects of pesticides and in effect gave a great boost to all the feeble discussions about environmental issues. It was also the year that London fog—despite its catastrophic effects ten years earlier and the clean-air act that followed it—caused the deaths of hundreds of people. Starting in 1961 the US Air Force used Agent Orange extensively; its development was the result of work in many laboratories and its effects on the environment and humans, as it was soon realized, were disastrous. Between the first (1962) and second edition (1969) of *Structure*, a strong and very vocal movement criticizing many aspects of scientific practices emerged among scientists and commanded an ever larger audience. This movement culminated with the publication a few years later in

[10] See http://undercurrents1972.wordpress.com/2013/02/06/uc-manifesto/, p. 2.

1972 of the immensely influential booklet titled *Science Against the People* by a group of well-known scientists who exposed the activities of the prestigious JASON committee, This advisory group, comprised of "star" scientists in the USA, were generously financed by the Department of Defense and became deeply involved in developing the anti-guerrilla techniques used by the US Army in Latin America and during the Vietnam War.[11]

Interestingly, such "cracks" in the image of science appeared within a context where success stories of science and technology continued. The reaction to Sputnik resulted in an appeal for more advances in science and technology, and in the 1960s, once TV sets had also invaded households, this flooded the public discourse. The hugely successful Apollo Program, which culminated with three astronauts landing on the moon in 1969, made not only the Americans re-live the triumph of the Manhattan Project. Though not as lethal in its connotations this triumph was equally forceful in the message it conveyed to the Soviet Union. The term "personal computer" seems to have been coined for the first time in 1962.[12] The period between the two editions of *Structure* was the period of human organ transplants: it was the time when liver, lung, kidney and later, in 1967, heart transplants were successfully carried out on humans. In a different direction, William Masters and Virginia Johnson did most of their ground-breaking work on the nature of human sexuality, which helped dispel all kinds of myths about women's sexuality, in the Reproductive Biology Research Foundation founded in 1964. They jointly wrote two classic texts in the field, *Human Sexual Response* and *Human Sexual Inadequacy*, published in 1966 and 1970, respectively. Both of these books were bestsellers and were translated into more than thirty languages. Thus, alongside the strong criticism against the various uses of science and technology, the success stories continued unabated. These and many other developments during the period between the two editions of *Structure* kept the notions of science and technology continually "in the news." The combination of success stories and the problematic (or scandalous, according to some) aspects of science and technology induced many people to rethink both the limits and the repercussions of what science can do and what it "should" do.

But, as we noted, all was not well. It was in 1962 that an article in the *Washington Post* by Morton Mintz exposed a horrifying story: The tranquilizer pill thalidomide was the cause of thousands of children being born without limbs.

[11] See http://socrates.berkeley.edu/~schwrtz/SftP/JASON/Jason.html.

[12] November 3—The earliest recorded use of the term features in *The New York Times* in a story about John Mauchly's lecture the day before at the American Institute of Industrial Engineers. Mauchly, "inventor of some of the original room-size computers," says that "in a decade or so" everyone would have their own computer with "exchangeable wafer-thin data storage files to provide inexhaustible memories and answer most problems." He is quoted as saying "There is no reason to suppose the average boy or girl cannot be master of a personal computer" Mauchly (1962).

The outcry that followed the article led to the banning of this sedative and to laws being passed for stronger regulation of drugs; its manufacture, however, was far from being terminated.[13] In addition, Mintz in a long report exposed the side effects of "The Pill," the contraceptive that was hailed in 1960 as having redefined the role of women. His accusation was that by approving the pill, the US Food and Drug Administration had launched the "greatest uncontrolled medical experiment"[14] in human history since the tests and evidence concerning side effects were hugely inadequate.

The case of the XYY chromosome and its connection to violent behavior was a particularly instructive case. After the unprecedented Watts Riots in Los Angeles in 1965, two publications in prestigious scientific journals—in *Nature* (December 1965) and in *The Lancet* (March 1966)—reported that in a study of 315 male patients in one of the special security hospitals for the developmentally disabled, nine of them were found to have the 47th chromosome.[15] It was reported that these patients were taller than the average height of the other patients and the authors characterized them as being "aggressive and violent criminals." In 1968, *The Lancet* and *Science* published the findings of Mary Telfer, a biochemist at the Elwyn Institute, formerly known as "The Pennsylvania Training School for the Feeble Minded," in which she claimed that acne was the distinguishing characteristic of XYY males, since in her study in the hospitals and penal institutions of Pennsylvania, she had found five tall boys and men who had facial acne.[16] And since the convicted murderer Richard Speck, who had tortured, raped and murdered eight student nurses from South Chicago Community Hospital on July 14, 1966, was acne scarred, it was suggested that the XYY syndrome was associated with aggression and criminality—even though Speck was not a XYY male, although it was reported that he was! In April 1968, the *New York Times* ran a three-part story about these findings, starting with a long first-page article in the Sunday edition, introducing this research the public. *Time* and *Newsweek* were quick to follow. Telfer was the exclusive source for all these articles.

The first comprehensive review article about the XYY syndrome was published by the end of 1968 in the *Journal of Medical Genetics*. The author was Michael Court Brown, director of the Medical Research Center Human Genetics

[13]For an amazing story involving thalidomide, politics and financial dealings, see http://www.theguardian.com/society/2014/nov/14/-sp-thalidomide-pill-how-evaded-justice.

[14]This was originally claimed in the Washington Post in 1962, to be emphatically repeated by Mintz in his review of Maurice Perutz's book, *The Fifth Freedom*, Mintz (1993).

[15]In 1961 the first report of a man possessing a 47th chromosome was published. See Anonymous (1966); Jacobs, Brunton, et al. (1965); Prince et al. (1966); Sandberg et al (1961).

[16]Telfer (1968); Telfer, Baker, Clark, et al. (1968); Telfer, Baker, and Longtin (1968).

Unit.[17] The article reported no statistical differences when the chromosome surveys in prisons and hospitals for the developmentally disabled were compared to those of the population at large. Telfer's results were considered to be seriously flawed, showing selection bias. In May 1969, at the annual meeting of the American Psychiatric Association, Telfer and her colleagues reported that their recent studies did indeed find that there were no differences.

Perhaps no other incident in the 1960s showed in such a dramatic manner how "scientific findings," social events, the hegemonic ideology and the mass media comprised such an integral whole. Though from the very beginning, the methodological flaws in this line of research were pretty clear, the amazing publicity it received from the "serious" scientific journals, newspapers and magazines showed that this kind of interrelationship was indicative of the less-than-objective nature of some scientific work. It can, of course, be claimed that all was well, since in the end the "bad science" of Tefler was exposed. But this was hardly the case. In 1974, psychologist John Money at Johns Hopkins Hospital experimented on thirteen XYY boys and men (ages 15 to 37) in an unsuccessful attempt to treat their history of behavioral problems with chemical castration using high-dose Depo-Provera. The side-effects were weight gain (avg. 26 lbs.) and suicide. This was not a case of science "going wrong." This was a line of research where people were actively involved in attempts to create a paradigm shift: an attempt to find "the seat" of violent behavior in biological entities.

Science—or, at least the scientific enterprise—did not *by definition* appear to be an undertaking pursued by virtuous individuals seeking objective results for the benefit of humanity. One needed many qualifications to reach such a conclusion. And though there had been similar worries in some scientific circles in the early 1950s concerning the build-up of nuclear weapons, the 1960s brought about a deeper sense of disappointment in the role of science to many more people. Increasingly, more and more scientists, students from a wide range of disciplines, intellectuals and of course, philosophers and historians of science were becoming very uneasy with the received view of science.

In less than ten years, Stanley Kubrick made three films containing some of the strongest statements about science and technology "going wrong." For many people, among his three roles in the 1964 film *How I Learned to Stop Worrying and Love the Bomb*, Peter Seller's eccentric scientist "Dr. Strangelove" seemed most realistic. In his 1968 film, *2001: A Space Odyssey*, the protagonist was, in effect, "Hall 9000," a computer capable of speech, speech recognition, facial recognition, natural language processing, lip reading and interpreting. It could also reproduce emotional behaviors, automated reasoning and even play chess!

[17] Brown, Price and Jacobs (1968a, 1968b); Green (1985); Harper (2006, 77–96); Jacobs (1982); Jacobs, Price, et al. (1968).

Whereas, *Clockwork Orange*, released in 1971, depicted the mediation of drugs in containing violence through behavior modification and the role of the state in this, it was actually an adaptation of Anthony Burgess's 1962 novella of the same name, a critical study of psychophysical and psychochemical methods used to "cure" violent behavior. The roles of scientists and government officials were presented as being complementary to projecting a view of science that was at the service of "law and order," with almost no concern for the ethical status of the methods used, their side effects or even their effectiveness. It was a film that examined the kind of science produced as a result of the close relationship between scientists and those holding political power. The "success" of science became its own dead end. What the novella and the film depicted was not so far removed from the situation relating to the XYY syndrome. A combination of bad methodology, conservative politics and the pressure to find "solutions" after the ghetto uprisings gave this discovery impressive coverage. The XYY incident is particularly characteristic of this period since it shows both the vulnerability of science to social forces as well as its self-correcting processes. It was an incident that convinced many people that scientific practice was far from being immune to what was happening in society at large, and often succumbed to the views and policies of the dominant social groups.

In an altogether different framework, between 1965 and 1975, Berkeley physicist Geoffrey Chew challenged the dominant paradigm in physics with his particularly interesting approach to elementary particle theoretical physics. Particle physics was previously dominated by a strict division between elementary and composite particles. Chew initiated a method whereby all particles—elementary and composite—were treated on an equal footing and called his approach "nuclear democracy." "My standpoint here [...] is that every nuclear particle should receive equal treatment under the law," he wrote in 1964. Chew, who was active in the reform activities concerning the changes in graduate physics courses, time and again explained their "unequivocal adoption of nuclear democracy as a guiding principle." He began by contrasting, at some length, "the aristocratic structure of atomic physics as governed by quantum electrodynamics" with the "revolutionary character of nuclear particle democracy." Chew—who had played an active role in the Berkeley Free Speech movement during the 1964–1965 academic year—and his collaborators published many papers in the standard journals and, in fact, claimed moderate success in dealing with the mainstream problems in elementary particle physics, before his method waned, basically because of serious difficulties involved in the calculations.

As Chew's program ran into difficulties, another group became active at Berkeley. In his provocative book *How the Hippies Saved Physics* (Kaiser 2012), David Kaiser told the intriguing story of the Fundamental Fysics Group: a group of physicists, largely from the West Coast of the USA, who insisted that physics as it was practiced in the 1960s had broken away from the culture of modern physics as established by its founders. They argued that to think about physics in a philosophically sophisticated manner, and to deal with all technical aspects without neglecting the conceptual dimensions, was part of the legacy of physics. They felt that the way physics was taught and practiced, the pragmatic culture of doing physics that was particularly prevalent in the US, was heading towards a dead end. Though the group was formed in 1975 and had been preceded by the "Consciousness Theory Group" and the "Physics/Consciousness Research Group," what had triggered these initiatives was Bell's theorem, published in 1964, which demonstrated the possibility of testing the non-locality of quantum mechanics. This was in fact verified in experiments by Aspect and others in 1981. The prospect of the Einstein Podolsky Rosen paradox being accommodated within quantum mechanics led some people to investigate the limits of what quantum mechanics could tell us about our consciousness, something that appeared to be of interest to the Central Intelligence Agency, which funded some of the groups' activities!

"Unhappiness" with the present state of science was also expressed from other quarters in rather extreme forms. After Timothy Leary founded the International Foundation of Internal Freedom in 1962, "experimenting" with LSD became rampant. Public discussions and articles in almost every newspaper and magazine about states of new or higher consciousness became very frequent, putting questions about ethics, limits and freedom of the scientific pursuits on the public agenda. Leary, "America's most dangerous man" according to Richard Nixon, was fired from Harvard the following year and his *Psychedelic Experience*, published in 1964, played an important role in his collaboration with John Lennon of the Beatles for the coming years.

Apart from individual critical reactions to prevalent mainstream scientific practices in the late 1960s and early 1970s, three collective initiatives provided the medium for articulating a systematic criticism of many facets of scientific activities—especially those related to the war in Vietnam. Three journals, accompanied by three collectives, appeared at the beginning of 1970s. *Radical Science Journal* was based in the UK. *Undercurrents, 'the magazine of alternative science and technology'* was also published in England between 1972 and 1984, when it was merged into *Resurgence: Science for the People* which was based mainly in the USA. In the 1969 meeting of the American Physical Society, two well-known physicists Charlie Schwartz and Martin Perl led an initiative to get a resolution passed against the Vietnam War. Though this did not materialize, a group of physicists established a group called "Scientists and Engineers for Social and Political Action" (SESPA), which participated in the 1970 annual meeting of the American Association for the Advancement of Science. The subsequent collective *Science for the People* became rather vocal in similar meetings and began to publish the journal.

It is interesting to note that the British Society for Social Responsibility in Science was founded in 1969 by a large number of well-known academics, including over 40 fellows of the Royal Society with the Nobel Laureate Maurice Wilkins as its first President. Its explicit aim was to explore the individual and collective responsibilities of scientists, to demonstrate political, social and economic factors affecting science and technology, and to draw attention to the implications and consequences of scientific development. The intention was to generate an informed public.

The creation of these three journals and the activities of the members of the collectives resulted in a sharpening of the critique of science, of its practices and, most notably, of the political implications of scientific research in some subject areas. Science and technocracy could not continue their march unscathed. The problems appeared more serious than "bad" applications of otherwise "good" science. The whole fabric of scientific activity, whether in the production of new knowledge or its applications, was perceived as needing serious readjustment. The postwar image of science and the ethos of those associated with its practices undermined the questionable status of the health and safety regulations of government or companies: the effects of pesticides, the laxity of government agencies in granting patents, the strong presence of pharmaceutical companies in research in university laboratories, the involvement of scientists in the war machine, the forum provided by prestigious journals for methodologically questionable scientific work, the dead end of "expensive" physics and even the attempts to escape the restraints dictated by dominant scientific practices. If left to their own devices, neither the scientists nor the government agencies and companies seemed able to achieve the virtuous effect going hand in hand with textbook narratives of what science and scientists should do for society. While in the long run it appeared that a democratic society had the means and the people to bring about at least a partial catharsis, by the end of the 1960s and the beginning of the 1970s the image of science had been severely tarnished.

Revolution

If a number of events played a catalytic role for society at large to reflect on and re-appraise the development, practice, research and applications of science between the two editions of *Structure*, the same period witnessed a rather strong re-orientation concerning another word appearing in the title of the book. No one could ignore the references to revolutions in what was happening among students, hippies and the black community as well as in the colonies in Africa, the movements in Latin America, China and, of course, Vietnam. Neither citizens in general nor the intellectuals in particular were indifferent to this. Whether friend or foe of these actual or potential political and social upheavals, no one could afford to dismiss them as fleeting, transient and ephemeral situations. The word "revolution" was no longer associated solely with the threat from the Soviet Union, and society at large became used to hearing the term and discussing its implications. Much like "science," "revolution" also became the talk of the town—admittedly a very large (global) town.

The completion of what was long considered as the paradigmatic revolution was heralded in 1962: the protracted uprising of the Algerians against the French, culminating in the declaration of their independence. In 1963, Hannah Arendt published her influential book *On Revolution*. Interestingly the most popular phase of the "revolutionary" Beatles coincides with the period between the editions of *Structure*: Their first hit *Love me Do* was released in 1962 and the band broke up in 1970, having recorded the song *Revolution* in 1968. Three years earlier, in 1965, Bob Dylan recorded *Mr. Tambourine Man* and his "revolutionary" album *Highway 61 Revisited*.

Another event with momentous repercussions was the Cultural Revolution in China, initiated by Mao Zedong himself in 1966 and lasting until 1976. Many scientists and scholars both in the USA and (Western) Europe were very sympathetic to the Cultural Revolution because, among other things, one of its aims was to create alternative sciences in agriculture and medicine.

In 1968, the expression "Green Revolution" was inaugurated for the first time, associating the word revolution with something whose beneficial repercussions were almost identical to utopian pronouncements. In the same year, the director of the United States Agency for International Development, William Gaud, who later received the Nobel Peace Prize for Peace, in a speech before the Society for International Development talked about the vast possibilities the new technologies could provide for agriculture. He was convinced that the technical developments contained the "makings of a new revolution. It is not a violent Red Revolution like that of the Soviets, nor is it a White Revolution like that of the Shah of Iran. I call it the Green Revolution."[18] Sadly the fate of this revolution led to serious catastrophes of established agricultural patterns and practices, and led to increased poverty among the poor in various nations.

1962 was also the year when a declaration known as the Port Huron Statement spelled out the principles and aims of the Students for a Democratic Society (SDS), which would play an absolutely decisive role in many of the developments

[18] "The Green Revolution: Accomplishments and Apprehensions" Address by William S. Gaud (administrator at Agency for International Development, Department of State, USA) to the Society for International Development, March 8, 1968.

among American students and youth during the coming decade. The manifesto condemned the role of large corporations, blamed the government for poverty, reproached racism and called for a participatory democracy.

Last but not least, the years between the two editions witnessed one of the most tempestuous events in American history: the uprisings in black neighborhoods, especially those in Watts and Detroit. The Civil Rights Movement following Martin Luther King Jr.'s "I Have a Dream" speech in 1963 and his declaration of the Program Alabama was followed by the Selma to Montgomery marches. The rest is history: the establishment of the Black Panther Party in 1966, the formation of the Weather Underground Organization in 1969 and the countless assassinations of emblematic figures signified the deep and radical changes, whether abhorrent or welcome, that would affect everyday life. Revolution was no longer something foreign to American society nor was it an abstract concept. It was there, menacing or liberating, depending on who you were, but surely not something to be indifferent about.

The Watts riots (or rebellion) references what occurred in one of the most impoverished neighborhoods in Los Angeles in 1965. The arrest of a black motorist by a highway policeman sparked riots that lasted for six days and could not be contained, even after troops of the National Guard moved in. After a curfew was imposed and the riots subsided, there were thirty-four dead, a thousand injured and four thousand arrested. The investigation that followed found that the reasons for the riots were the abominable living conditions of the people living in the Watts neighborhood. The Watts riots and the following events in Detroit in 1967, which were brought about by essentially the same reasons as the Watts riots, resulted in even more casualties. These events became emblematic symbols for the most radical aspects of the Civil Rights Movement. The urban riots were at the beginning conveniently regarded as the expression of violent behavior by innately violent individuals. Yet, soon they came to symbolize the plight of the black community in the USA.

In fact, this was the same period when the Revolution(ary) became visible. The murder of Che Guevara in Bolivia in 1967 caused his image to be shown almost everywhere, also portraits in the famous iconoclast, Andy Warhol's, pantheon. Graffiti, blouses, t-shirts and posters helped the image of revolution invade private spaces and become part of people's appearances. For better or for worse, fewer and fewer people could afford to be indifferent about "The Revolution(ary)." Thus spurred by momentous world events in the period between the two editions of *Structure*, the word "revolution" became deeply entrenched in the public discourse.

The public discussions about Kuhn's book outside the narrow circle of philosophers of science (since historians of science hardly participated in the discussions), its popularizations and the references to it, all took part in the context surrounding these events. Science was no longer something to be unconditionally worshipped. Between those who were uncritically talking about science and those who were inaugurating the anti-science movements, many people began to seek a third approach whose faint yet definite path was now becoming feasible. The same thing happened to revolution. Revolution was no longer a reference to characterize the birth of two "nations": one in 1776, which personified everything that was (absolutely) good, and one in 1917, which personified everything that was (absolutely) bad. An alternative approach, with its excesses and contradictions, was also being articulated. Within such a framework, a book with such a title as *Structure* could hardly have gone unnoticed in the 1960s and 1970s. This does not go to say that all who were attracted to its title read it closely, nor do I imply that the title in itself is responsible for the book's success. Surely, however, in the specific conditions of the period, such a title greatly helped the propagation of the book and increased the number of people who became acquainted with its contents through its many and varied popular expositions.

Kuhn's book appeared in a period when, on one level, there were concerted efforts to normalize educational programs in accordance with the hegemonic Cold War mentality, and "prove" that the USA could do better things in space, in technology (as the famous Kitchen Debate between Nixon and Khrushchev showed during the opening of the American National Exhibition in Moscow in 1959), in cinema, in economy and in education. On another level, there was the formation of a multiplicity of viewpoints which, by the mid-1960s and throughout the 1970s, would strongly challenge established and long-cherished values and ideals, mainly in the USA and, then, in many European countries. The book appeared and slowly took off during a period of intense criticism of the ways in which science was produced, practiced and applied. The 1960s and 1970s became a period of both radical criticism of the sciences *and* of a search for alternative models concerning the production, practices and applications of science. Such discussions and controversies were not part of the anti-science trends of the 1960s. Quite the opposite: painstaking efforts were made to articulate a new paradigm, in education, in the ways that science was practiced and applied, in energy consumption and even in personal relationships. There was an overall feeling that American society was in search of a paradigm shift. A paradigm shift appeared to be the common aim of those who were critical of many aspects of the sciences and technology. Kuhn's book, surely without its author's blessings, became a kind of reference point for many people who were unsatisfied with the *status quo*.

It was a book with a specific title, which according to its popular accounts argued that there could be changes in the sciences, and not necessarily through only well-defined methods and rational undertakings. It that insisted the new paradigm could be incommensurable with the old; it had no difficulty catching the imagination of many people who were discontented and even disgruntled with both the way in which science was pursued and the ways that society was run.

It was not unreasonable for people who were frustrated with the social function of science to have thought that *Structure* could provide clues for an alternative approach, or that it would help them understand the reasons why so many things, at least in the sciences, went wrong. Since more and more people began to associate the book with the notion of paradigm change, it may have appeared that *Structure*—which in the minds of many was about science and scientists and not a strictly philosophical book—had the answers. Scientists could relate to the book in their everyday lives; students could find a critique of education. People participating in social movements (whether for civil rights, the running of the universities, pro-peace, "science for the people," and so on) found justification to diverge from rational ways to change the status quo. Self-proclaimed revolutionaries considered the possibility of erasing old memories and starting a clean slate, regardless of whether these "readings" could hold when the contents of the book were analyzed in accordance with the rules of academic discussion. Society at large and its various sub-cultures do not always obey the rules of academia when perceiving and appropriating ideas expressed in books. It is these processes that "made" the book into a cultural artifact.

Books in Search of New "Paradigms"

> I knew someone at Princeton, who congratulated me on avoiding being a guru. And she said I could so easily have been the Marshal McLuhan of science.[19]

The Gutenberg Galaxy: The Making of the Typographic Man by Marshall McLuhan was published in 1962. The author argued about the deep interconnections between communication technology (even from ancient times) and cognitive organization, bringing about dramatic repercussions in the ways societies are organized. This pioneering work in cultural and media studies was followed in 1964 by a work that provided a further solid basis for media studies. *Understanding Media: The Extensions of Man* opened new vistas for discussions about the new artifact that was invading every household in Western societies, and the author codified his views with a phrase that would become a catchphrase of our times: "the medium is the message" (McLuhan 1964, 7). The book was catalytic

[19] Interview with Thomas Kuhn, see footnote 5.

in initiating a discussion about the non-neutrality of technology and that when assessing the "wonders" of science, regarding technology as simply the application of scientific innovations may not be a particularly fruitful way of understanding technology.

But the period between the two editions of *Structure* saw the publication of a number of books that have, since their publication, played a rather decisive role in raising public awareness by questioning some of the long held "untouchable" beliefs of Western societies.

Silent Spring by Rachel Carson was also published in 1962. It revealed the catastrophic effects of pesticides, especially on birds, as well as the neglect of industries, particularly the chemical companies, and government officials to impose safety measures. It became the first book to help make the American public aware of environmental issues and led to the ban of the widely used DDT in agricultural practices. Interestingly, many of the synthetic pesticides used were being developed through military funded research. A strong boost to her own research were reports relating pesticides to carcinogenesis. Although her book was based on a mass of technical data, Carson's message was not technical: it emphasized the effects humans have on nature and the hitherto unimagined repercussions of such effects. Years later, while assessing the effects of the book, Mark Hamilton Lytle would write that Carson "quite self-consciously decided to write a book calling into question the paradigm of scientific progress that defined postwar American culture" (Lytle 2007, 166–167).

Two other influential books were published in 1962. Given the context within which Milton Friedman's *Capitalism and Freedom* was written, it advanced a critique of US government big spending and argued that economic freedom was a prerequisite for political freedom. Though such views became dominant after the late 1970s, at the time they vied for a change of paradigm. The other book was *The Other America: Poverty in the United States* by Michael Harrington. The author, a former Catholic disillusioned and "shocked by the faithlessness of the believers" declared himself an atheist and became involved in left-wing politics. In his book, he argued that almost 25% of Americans lived in poverty and, since the data upon which his thesis was based was freely available, he spoke of how the poor were made invisible by the Americans themselves. Policies that were first initiated by President Kennedy and subsequently named by the Johnson Administration "War on Poverty" were traceable in Harrington's ideas about social welfare.

The *Feminine Mystique* by Betty Friedan, published in 1963, made the dissatisfaction of middle-class women public, in a time where many thought they were a segment of the population who seemingly "who had it all"—husbands with good jobs, houses in good neighborhoods, children going to good schools.

The issues raised were initially intended to be published as an article, but when no journal would willing publish it, Friedan instead decided to write a book, researching the lives of middle-class women, the discussion of the role of education, women's magazines and advertisements targeting housewives. "The problem that has no name" turned out to be hugely successful, sparked the second-wave feminist movement and contributed to radical changes in American society, not least of which was the establishment of the National Organization of Women in 1966. Her book was preceded by an article of similar content by another champion of American feminism, Gloria Steinem.

The Making of the English Working Class by E. P. Thompson was published in 1963 (and revised in 1968) and brought to the fore the culture and practices of the working class, especially of the artisans and workers, attempting to "rescue the poor stockinger, the Luddite cropper, the "obsolete" hand-loom weaver, the "utopian" artisan [...] from the enormous condescension of posterity" (Thompson 1963, 2). Thompson forcefully argued for a different kind of social history, where he would rescue the working class from being treated solely in terms of statistics, thus bringing in a humanist element to the writing of history. In 2013, in an article celebrating the 50th anniversary of its publication, Robert Colls, a cultural historian noted that "in its day his book was the biggest paradigm-shifter of the lot" (Colls 2013, 7).

One-Dimensional Man: Studies in the Ideology of Advanced Industrial Society by Herbert Marcuse was published in 1964 and became an emblematic book for another social phenomenon of the period: the formation of the "New Left." The book presented a forceful criticism of both capitalism and socialism as applied in the Soviet Union, and discussed the new forms of social repression in both societies, analyzing the repercussions of consumerism in the undermining of the revolutionary potential in Western societies.

Unsafe at Any Speed Ralph Nader was published in 1965 and was highly critical of the automotive industry. The book revealed the indifference of the auto industries to safety, and the fact that they did not utilize reliable test results in order to incorporate the necessary changes in the design of cars. If Carson's book provided the rationale for a comprehensive environmental movement, Nader's book gave the same impetus to the consumers' movement and made its author the unquestionable "leader" of consumers' interests. Nader's subsequent lobbying led to the establishment of the US Environmental Protection Agency in 1970.

Science and Survival by Barry Commoner was published in 1966 (and his *The Closing Circle* in 1971). He argued for a change in the whole structure of the industrial basis of capitalism to conform with the laws of ecology that he had first formulated. He forcefully argued for the notion of sustainability, and very large audiences became acquainted with the notion and its implications. His was a different paradigm, proposing the "eco-socialist" model to replace the "limits of growth" thesis, by arguing that it was the capitalist industries, rather than overpopulation, that were responsible for the ecological problems.

These books had a large circulation and almost all of them have since remained in various "100 most important books" lists, most notably that of *Time* magazine with its huge readership—independent of what the validity of such lists may be. Nevertheless, such lists are indicative of the public perception of these books and it is surely the case that these books have challenged dominant values, practices, policies and viewpoints, resonating with the demands expressed through many social issues of the period.

Interestingly Nader's book, along with Friedan's and Carson's, together with the works of Keynes, Dewey, Marx, Hitler, Mao, Compte, the Kinsey Report, Lenin and Darwin appeared on the list of most harmful books in the nineteenth and twentieth centuries of the *Human Events*, the site of "powerful conservative voices."[20]

What I have discussed above is, of course, not an exhaustive list. Many other books of similar character, which were widely discussed yet perhaps not as catalytic as the ones mentioned, were published in the period between the two editions of *Structure*. The *Age of Revolutions*, the first book of a planned trilogy by Eric Hobsbaum, was published in 1962. Between 1964 and 1966, Richard Feynman's lectures in physics, were published, bringing a totally new approach to undergraduate physics teaching. In 1963, the *Letter from the Birmingham Jail* by Martin Luther King and the authorized version of Che Guevara's *Reminiscences* were both published. Two iconoclastic books appeared in the next two years and found a very large readership. In 1964, Timothy Leary's the *Psychedelic Experience: A Manual Based on the Tibetan Book of the Dead* appeared, and a year later the "comedian" Lenny Bruce published his *How to Talk Dirty and Influence People*. Paul Freire's well-thought strategy for a *Pedagogy of the Oppressed* appeared in 1967 and as did David Cooper's *Psychiatry and Anti-Psychiatry*, which would be decisive in the debates that reconsidered psychiatric practices. In 1968, Eldridge Cleaver, a founding member of the Black Panther Party founded in 1966, published his *Soul on Ice*, which became hugely popular. In 1969, Hilary Rose and Steven Rose published *Science and Society*, which severely criticized British science policy and became one of the first books ever written on science policy. In 1973 Levy Leblond, a well-known French physicist from Orsay, published *Autocritique des sciences*. The work was the result of discussions and popular publications around the themes of "eco-socialism" initiated by the collective Open Science (Science Ouverte), which was established in 1966 by the biologist Max de Ceccatty, the philosopher François Dagognet and the mathematician André

[20] See http://www.humanevents.com.

Warusfel. In that same year, *Small Is Beautiful: Economics As If People Mattered* by E. F. Schumacher was also published. The author of the essays collected in the book presented an incisive critique of economic development at a time when the energy crisis came to question the model of development in Western societies. The author traced much of what was wrong in postwar capitalism to its strong adherence to what he called "gigantism." Peopled-centered economics, he argued, would make an environmentally sustainable progress possible.

For many people, *Structure* became part of this constellation of books that through their incisive criticism of various aspects of dominant values and practices were, in effect, putting the demand for a change of paradigm on the social and political agenda, whether in the role of women in society, economic development, technology, ideology or the writing of history.

Concluding Remarks

During the period between the two editions of *Structure*, a number of social issues were publicly negotiated through books that, eventually, commanded large readerships and came to symbolize the new social movements and a new public consciousness. There was a deep metamorphosis in the public perception of the status of the black community, women, university students, America's military might, the environment, industrial production, historiography and other aspects of social and academic life. The scathing criticism of the status quo and the search for new paradigms went hand in hand. These books became emblematic of the new era, and so did *Structure*. It described the past of the sciences and the *structure* of its revolutionary changes in ways that were perceived as homologous with whatever was happening "out there."

More specifically, within such a framework an increasing number of scientists were becoming dissatisfied with the dominant trends in the social function of science, seeking alternative ways of organizing and applying science. *Structure*—independent of its philosophical problems—became some kind of reference point. To many it signified a critique of the traditional way science was viewed. Paradigm change implied that changes were indeed possible. Incommensurability meant that the "old" state of affairs would not linger on in the "new." The importance of consensus around a paradigm that *Structure* claimed raised hopes about different practices if consensus could be achieved with respect to different values. The questioning of progress in science helped the discussion about the possibilities for other modes of social development. Though such a "neat" codification was surely unacceptable to professional philosophers, it did form a framework that provided some kind of theoretical justification to many of those who were frustrated with what was going on in both society at large and within the field of science.

Discussions among philosophers of science did not seem to deter a large number of people from considering the book as addressing many of the issues that were bothering them. Fuller has perceptively noted that

> The appeal of SSR is founded on its ability to compel readers without demanding too much engagement in return. It is [a] narrative that is indefinitely adaptable to user's wishes […] certainly the book does not encourage deep reading […] [it has a] non threatening prose style, which contains relatively little technical language invites the reader to participate in correcting its flaws and completing its argument. But this invitation is less to interpret than apply the text […] a common thread that runs through the formal and informal comments people make about the book is that it is quite thin in their own field of expertise, but truly enlightening in some other field. (Fuller 2001, 31–32); (Reingold 1991, 389–409)

Indeed, *Structure* was many things to many people or (slightly) different things to different people. And such a characteristic was particularly "convenient" in the 1960s and 1970s for the success of the book.

The period between the two editions of *Structure* embraced many events that forced a reconceptualization of both revolution and science in the minds of an amazingly large number of people. The civil rights, student and anti-war movements brought the realities of uprisings very close to home, to which American society had seemed immune to more than a generation before. What was understood as happening in lands far from the US, in the mid-1960s became part of the everyday experience of American society. No one could afford indifference. Independent of whether Kuhn's views were formed during the period when American educational exigencies were adapting to the Cold War, one cannot ignore the fact that at the same time, at the height of the Cold War, all kinds of new critical approaches to the ways the sciences were practiced started to play an increasingly important role. These events brought about deep divides and lasting changes within the scientific community. The sales figures of the book imply that eventually, Kuhn's book was appropriated by an increasing number of people who wanted to bring forth changes in many aspects of American society.

It was during this very same period that the social perception of science and scientists, and the assessment of what these scientists were doing, became rather critical. If the sentiment in American society in the post war years was "in scientists we trust," then this long-cherished unconditional trust in what the scientists were engaged in started to wane. It was not a question of scientists' ethics; it was the crisis within the strong ties between science and democracy that many thought was irrevocably broken. A whole generation that was raised to believe that science and democracy were strongly correlated was realizing that neither science nor democracy was faring well.

This uneasy and confusing social context made Kuhn's book a respectable choice for all those who had become disillusioned by the occurrences of the era, and despite its success stories, even in the sciences. *Structure* became a point of reference for those who wanted to understand what was happening in the chaotic developments which, in one way or another, touched them. For those with some scientific background and who felt that the old ways were over, Kuhn's book, with its comprehensible philosophy and its history, which gave a sense of relevance, was, at least, a good starting point.

So we see that in the period when Kuhn's book began its career, there was a deep political, social, institutional and ideological realignment among various groups of scientists. This brought all kinds of reactions, criticisms and, most importantly, a search for alternatives; a search of alternative ways of how to do science, what kind of science to do and how to apply it. Paradigm shift, though it used in a totally different way than Kuhn used it, became the "term" that unified the disgruntled. This is not to say that Kuhn and his book had a leftist or even a radical agenda. Nevertheless, how books and their ideas are appropriated in societies often has little to do with what the authors believe, or with what the expressed aims of the book actually are.

As I stressed at the beginning, the point of this essay is to give credence neither to the hypothesis that the phenomenal success of the book was not primarily due to the philosophical discussions it initiated, nor to the sympathetic views that some scientists expressed towards it, but rather because of the general social climate in the USA. In order to substantiate such a hypothesis, a number of events—especially those that directly or indirectly questioned the dominant views and practices of science—have been discussed to show how the demands or trends among many social groups in the USA appeared to *resonate* with the social perception of Kuhn's book.

It is not inconceivable that the public reception of *Structure* was tinged with an aura of radicalism, since it was a book with such a "radical" title and it was, at the same time, criticized severely by the established philosophers of science. Thus, misreading this work in the 1960s and early 1970s should not be taken as a sign of collective inability to understand the details of the arguments in *Structure*, but rather as a way to appropriate a cluster of ideas which appeared to be in alliance with the ideas of all those who had become disillusioned with science and its practitioners.

I have tried to bring together a number of events whose beginnings can be traced in the period between the two editions of *Structure*—whether publishing enterprises or large-scale social and political movements—that strongly criticized many of the constitutive pillars of modern Western societies: the role of women, the status of black people, science, economic development, industrial production—and persuasively put forth alternatives. Neither the books nor the social movements were marginal events. The books and the discussions around social events generated ideas, proposals and practices that for sometime caught the imagination of a large number of citizens. The problem for discussion was the phenomenal success of the book or rather, the phenomenal sales figures of the book, which made it a unique—and only—success story on such a scale in the history of the history, and/or philosophy and/or sociology of science.

Acknowledgments

I have greatly benefited from comments by Theodore Arabatzis, Thanassis Lagios, Vasia Lekka, Grigoris Panoutsopoulos, Manolis Patiniotis and Ana Simões. I thank them all.

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Chapter 5
Kuhn's Paradigm of Paradigms: Historical and Epistemological Coordinates of *The Copernican Revolution*

Pietro Daniel Omodeo

> *I shall not try to explain here the reasons and causes that produced the spiritual revolution of the sixteenth century. It is for our purpose sufficient to describe it, to describe the mental or intellectual attitude of modern science.*
>
> Alexandre Koyré (1943)

> *It was a revolution beside which the French Revolution was a child's play, a world struggle beside which the struggles of the Diadochi appear insignificant. Principles ousted one another, intellectual heroes overthrew each other with unheard-of rapidity [...] All this is supposed to have taken place in the realm of pure thought.*
>
> Karl Marx and Friedrich Engels (1846)

The Historical and Epistemological Centrality of Copernicus for Kuhn

The Renaissance astronomer Nicholas Copernicus, his scientific achievement, its impact and the reception of the heliocentric planetary theory occupied a special place in Thomas Kuhn's reflections on science, both historical and philosophical. Kuhn often referred to Copernicus as the first of a progeny of genial scientists; modern heroes whom he deemed to have produced major shifts in epistemic developments.

> [T]he major turning points in scientific development [are] associated with the names of Copernicus, Newton, Lavoisier, and Einstein. (SR, 6)[1]

In his classic of historical epistemology, *The Structure of Scientific Revolutions*, Kuhn constantly referred to Copernican astronomy as an insightful case apt to illustrate his basic notions of 'paradigm' and 'scientific revolution.'

As a matter of fact, *Structure* was preceded by a monograph on this crucial historical case, *The Copernican Revolution* (1957). Kuhn probably composed the two works in parallel.[2] At least he had conceived them together. In fact, as early as 1952 he had successfully applied for a Guggenheim fellowship, which he wanted to use to complete a monograph on the Copernican issue along with another one on scientific revolutions in general for the *International Encyclopedia of Scientific Revolutions* (Marcum 2005, 13). Evidently, *Copernican Revolution* and *Structure* are the two sides of one and the same endeavor. The historical side was a preparation and a support for philosophical speculations while the theoretical one guided the historical inquiry and was implemented on the latter's basis. On this purpose, Noel M. Swerdlow remarked that

> *The Copernican Revolution*, Kuhn's first published attempt at an answer [to the problems of methodology of scientific research], may be understood as a great case history of one of the monumental changes in the history of science in order to provide an explanation of how so great a revolution happens. In this sense, it is his first scientific revolution. (Swerdlow 2004, 75)

The Copernican turn in planetary astronomy served for Kuhn as a lense through which later intellectual breaks in the history of science could be understood. In this sense Copernicus was Kuhn's παράδειγμα παραδειγμάτων, the paradigmatic case of all paradigms. "What he [Kuhn] needed was a historical exemplar—so John Heilbron—He found it in the Copernican revolution. The story of the shift from geo- to heliocentrism as he simplified it for pedagogical purposes, from Ptolemy and Aristotle to Copernicus and Newton, made a superb play for his theater of warring paradigms. The two opposite systems straightforwardly made two different worlds" (Heilbron 1998, 508).

[1] I will refer to *The Structure of Scientific Revolutions*, Kuhn (1996), hereafter cited as SR followed by the page number in this third edition. Similarly, I will use CR as an abbreviation for *The Copernican Revolution*, Kuhn (1959).

[2] Cf. Kuhn, "Preface" to the 1962 edition of *The Structure of Scientific Revolutions*. SR (vii): "The essay that follows is the first full published report on a project originally conceived almost fifteen years ago."

Copernicus was not just the protagonist of one among many revolutions. Rather, he became *the symbol* of the Scientific Revolution. As a consequence, Kuhn's first book cannot be read, understood and criticized *solely* from the viewpoint of history. *The Copernican Revolution* is a point of departure for a correct assessment of his philosophy of science.

Kuhn stated that history of science and epistemology are two entangled genres, albeit separated. They are closely inter-related although historians and philosophers belong to two different disciplinary fields and have different goals. Indeed, the former construct plausible narratives while the latter seek something that is "true at all times and places."[3] In Kuhn's *curriculum vitae* the two professions coexisted, as he himself observed in a biographical note, in the talk "The Relations Between the History and the Philosophy of Science," delivered in 1968:

> To say that history of science and philosophy of science have different goals is to suggest that no one can practice them both at the same time. But it does not suggest that there are also great difficulties about practicing them, alternately, working from time to time on historical problems and attacking philosophical issues in between. Since I obviously aim at a pattern of that sort myself, I am committed to the belief that it can be achieved. (Kuhn 1977, 5)

Surprisingly, in this passage Kuhn downplayed the dependency of the historical moment on the epistemological or *vice versa*. He presented the relation between the two fields of investigation as a thematic overlapping, as an "interdisciplinary" instead of "intra-disciplinary" relation. Copernicus was the author of one scientific upheaval, if seen from a historical perspective, but also *the model* revelatory of the structure *of any scientific revolution*, from the universalizing viewpoint of philosophy.

Kuhn was not the first who allotted to Copernicus the role of a founding father of modern science. On this account, he mostly relied on Alexander Koyré, one of the *innovators* of the history of science whom he openly acknowledged in the preface to *Structure*.

It should be immediately remarked that the reception of Koyré's historiography played an important role in the ideological confrontations of the Forties and the Fifties dividing the world into two camps, west and east of the *Iron Curtain*. In influential publications on the history of early modern science, such as *Études galiléennes* (1939) or the later *From the Closed World to the Infinite Universe* (1957), Koyré explicitly offered a *spiritual* conception of the Scientific Revolution as descending from the *heavens* (both literally and symbolically), which he

[3] Kuhn (1977, 6), from "The Relations Between the History and the Philosophy of Science."

explicitly opposed to the socio-economical and technological accounts of scientific advance proposed by Marxist scholars. As Yehuda Elkana put it in a colorful way, "[Koyré's] studies became a paradigm for history of science as history of disembodied ideas."[4] Since Kuhn's theory of paradigms and of revolutions represented in many respects a generalization (an *epistemologization*) of Koyré's conception of the Scientific Revolution,[5] *Structure* was enhanced by participating of the Koyréan symbolic capital. Therefore, before discussing Kuhn's Copernicus, it will be expedient to consider some key elements of the Cold War mentality affecting the history of science of those years. I will especially point out the Marxist challenge that made the Koyréan approach appear as a viable counter-program in the Anglo-Saxon West.

Koyréan Commitment

By employing historical notions such as 'Scientific Revolution' and 'Copernican Revolution,' Kuhn revealed himself as a 'son of his age,' a reader and follower of Koyré, whom he acknowledged in *Structure* alongside others like Anneliese Maier and Arthur O. Lovejoy (SR, *Preface*, viii). The choice of these authors is by no means casual. All of them were *historians of ideas* investigating the abstract entities of theory as independently as possible from material aspects. In an entry on "The History of Science" for the *International Encyclopedia of the Social Sciences* (1968), Kuhn pitted the Duhemian school, in which he included Koyré, against historiography exposed to Marxist influences. The asymmetry of his treatment of (and judgment on) the two schools is striking.

On the one hand, he extolled the French conservative historian of science Pierre Duhem as capable of "disclosing" new prospects, namely the historical singularity of medieval and Renaissance science. Duhem's reconstructions of medieval history, so Kuhn, shed light on the ground out of which "the *new science* sprang." Kuhn added that, "more than any other, that [Duhemian] challenge has shaped the modern historiography of science. The writings which it has evoked since 1920, particularly those of E. J. Diksterhuis, Anneliese Maier, and especially Alexandre Koyré, are the models which many contemporaries aim to emulate" (Kuhn 1977, 108). One should not be deceived by the apparent facticity of the statement. The reference to the alleged success of Duhem's school is prescriptive. Kuhn counted himself as one of the "contemporary emulators" of

[4] Elkana (1987, 115). Yet, according to Elkana, the contextual awareness of Koyré's historiography was the indirect source of post-Kuhnian historical sociology of science. Elkana (1987, 144): "Koyré *genuit* Kuhn; Kuhn (and Merton and a few others) *genuerunt* the Historical Sociology of Scientific Knowledge." I will discuss Kuhn's sociology *without society* later.

[5] For a brief overview of Koyré's idea of the Scientific Revolution, see Hall (1987).

the medievalist. In an article appearing in the *Études d'épistémologie génétique* (1971), Kuhn explicitly committed himself to that French legacy by mentioning Koyré as "the man, who, more than any other historian, has been my *maître*."[6]

On the other hand, as the 1968 encyclopedia entry goes on,

> Still more recently, one other set of influences has begun to shape contemporary work in the history of science. Its result is an increased concern, deriving partly from general history and partly from German sociology and Marxist historiography, with the role of nonintellectual, particularly institutional and socioeconomic factors in scientific development. Unlike the ones discussed above, however, these influences and the works responsive to them have to date scarcely been assimilated by the emerging profession [of the historian of science]. For all its novelties, the new historiography is still directed predominantly to the evolution of scientific ideas and of the tools (mathematical, observational, and experimental) through which these interact with each other and with nature. Its best practitioners have, like Koyré, usually minimized the importance of nonintellectual aspects of culture to the historical developments they consider. [...] As a result, there seems at times to be two distinct sorts of history of science, occasionally appearing between the same covers but rarely making firm or fruitful contacts. (Kuhn 1977, 109–110)

Not only does Kuhn side with Koyré's critique of the materialist excesses of the Marxist historians of science, but also treats the two approaches, the intellectual and the socio-economical, as "incommensurable paradigms," making virtually no contacts. Again a descriptive-sounding statement has a prescriptive intention.

It seems appropriate to quote here Roy Porter's comment on the ideological divisions of Cold War history of science:

> As part of the rejection of everything Marxist in the years of the Cold War, Anglo-American history of science was to distance itself from all such concerns with the social roots and even the social fruits of science. Instead, from the 1950s it became profoundly fascinated with the internal intellectual challenges posed by science. (Porter 1990, 35)

[6] Kuhn (1977, 21). The article is entitled "Concepts of Cause in the Development of Physics."

Kuhn makes no exception. In this regard his work, especially "*Structure* does not so much transcend the Cold War mentality as expresses it in a more abstract, and hence more portable, form" (Fuller 2000, 6).

The Other Side of the Ideological Divide: Marxist HPS

In the history of science, Cold War ideological confrontations famously began at the *International Congress of the History of Science and Technology*, held in London, in 1931.[7] On that occasion, the Russian leader Nikolai Bukharin led a Soviet delegation of historians presenting their Marxist viewpoint on the history and philosophy of science. As the British X-ray crystallographer John Desmond Bernal, himself a Marxist, reported in a picturesque manner,

> The Russians came in a phalanx uniformly armed with Marxian dialectic, but they met no ordered opposition, but instead an undisciplined host, unprepared and armed with ill-assorted individual philosophies. There was no defense but the victory was unreal. […] Their appeal to dialectic, to the writings of Marx and Engels, instead of impressing their audience, disposed them not to listen to the arguments which followed. (Bernal 1949, 338)

Bernal immediately perceived the ideological dimension of the confrontation he had witnessed. Bukharin and his group tried to extend the political struggle of the Russian Revolution from immediate political confrontation to cultural production in general and the history and philosophy of science in particular. British academics were quite unprepared for such challenges. As Bernal stressed, the Soviets defended "a point of view, right or wrong; the others had never thought it necessary to acquire one" (Bernal 1949, 336). The Soviet viewpoint on science was the opposite of that which Koyré was to become a champion of. It focused on the material and the socio-economical factors of scientific progress. It was aimed to contrast a widespread historiography dealing with *internal* theoretical developments and technicalities or with the biographies of "Romantic geniuses," such as Copernicus, Galileo and Newton.

In order to grasp the leading ideas shared by the Soviet delegates, it is useful to isolate a few crucial theses expressed by their leader. In London, Bukharin

[7] This and the following two sections are a partial reworking of the talk "Reflections on History of Science and Cultural Hegemony at the Threshold of the Cold War," delivered at the 2013 Moscow conference *Social and Human Sciences on Both Sides of the 'Iron Curtain'* (Poletayev Institute for Theoretical and Historical Studies in the Humanities – National Research University "Higher School of Economics," October 17–19).

delivered the talk, "Theory and Practice from the Standpoint of Dialectical Materialism," which began with an address to the "fundamental questions of philosophy: the question of the *objective reality of the external world*, independent of the subject perceiving it, and the question of its *cognisability*" (Bukharin 1931, 11ff.). He asserted that objective material reality is the necessary presupposition of science that cannot be renounced: "Epistemology which is praxeology must have its point of departure in the reality of the external world: not as a fiction, not as an illusion, not as a *hypothesis*, but as a basic fact" (Bukharin 1931, 16).

Already in his very successful elementary introduction to Marxist philosophy, *Historical Materialism: A System of Sociology* (first issued in Russian in 1921, and soon translated into French in 1921, German in 1922 and English in 1925), Bukharin supported philosophical materialism, and reflected on the conditions for science and its aims. Scientists, he wrote, seek for general laws, either natural or social: "In nature and society there is a *definite* regularity, a *fixed* natural law. The definition of this natural law is the first task of science. This causality in nature and society is objective" (Bukharin 1921, 20). Thus, in principle, history is predictable:

> An eclipse of the Sun does not depend either directly or indirectly on human desires [...] The case with social phenomena is entirely different, for they are accomplished *through* the will of men. [...] Socialism will come inevitably because it is inevitable that men, definite classes of men, will stand for its realization, and they will make so under circumstances that will make their victory certain. Marxism does not *deny the will but explains it*. (Bukharin 1921, 51)

Note that, although human will counts as a factor of social transformation, still it is not free because it is determined just as natural phenomena. The developmental law regulating both nature as well as society is one and the same. In Marxist jargon it is called dialectics.

This naturalization of society and historical processes fostered Bukharin's lively interest in the natural sciences and in epistemology. Many pages of *Historical Materialism* were dedicated to science, philosophy and their mutual relations, especially chapter VI, "The Equilibrium between the Elements of Society." Bukharin's theory rested on the Marxist distinction between an economic *structure* and a political and cultural *superstructure*. The latter comprises "the social and political system of society [...]; manners, customs and morals [...]; science and philosophy; religion, art and finally, language" (Bukharin 1921, 150). According to Bukharin, science belongs to the realm of *ideology*, a concept coextensive with culture. "Science"—he wrote—"is a unified coordinated system of thoughts, embracing any subject of knowledge in its harmony"

(Bukharin 1921, 208). Its *fundamental epistemological principle* is that "every science is born from practice" (Bukharin 1921, 161). As Bukharin affirmed at the London conference, "science or theory is the continuation of practice." Its social function is "orientation in the external world and in society, the function of extending and deepening practice, increasing its effectiveness, the function of a *peculiar* struggle with nature" (Bukharin 1931, 20). Accordingly, science is so closely connected with technological advance that it is completely dependent on it. This emerges from two basic theses of epistemological import: "1. that the content of science is given by the content of technology and economy; 2. that its development was determined among other things by the tools of scientific knowledge" (Bukharin 1921, 169). As to the former point, Bukharin remarked that the technical-economical basis of scientific advance is witnessed by many historical instances, in which different scholars carried out discoveries simultaneously and independently from each other:

> The content of science is determined in the last analysis by the technical and economic phase of society; these are the 'practical roots,' which explain why an identical scientific discovery, invention or study, may be achieved simultaneously in different places, perhaps quite 'independently.' (Bukharin 1921, 164)

Note that, according to Bukharin, the practical determination of science does not mean an utilitarian (say, Baconian) conception: "It is not a question of the *direct* practical importance of *any* individual principle [...] It is a question of system as a whole" (Bukharin 1931, 20). It is also a question, from a historical viewpoint, of recognizing "that *genetically* theory grew up out of practice" (Bukharin 1931, 19). For historical examples of science emerging from practice, Bukharin often mentioned Ernst Mach's reconstructions. Although he did not follow the latter's *empiriocriticist* epistemology, whose principle of economy had been severely criticized by Lenin, Bukharin derived historical examples and perspectives from Mach, whose philosophy had been widely received in Russia between the XIX and the XX centuries (Steila 1996).

A famous case study resulting from the application of the general principles so far outlined was the presentation of the Soviet delegate Boris Hessen. His paper, "The Social and Economic Roots of Newton's *Principia*," is generally seen as the most significant historical essay presented at the 1931 conference. Gideon Freudenthal and Peter McLaughlin have synthesized his concept in the following three points:

1. Theoretical mechanics developed in the study of machine technology;

2. Conversely, in those areas where seventeenth-century scientists could not draw on existing technology the corresponding disciplines of physics did not develop;
3. Ideological (theological) constraints descending from the political constellation affected crucial philosophical concepts of Newton's physics (such as matter).
(Freudenthal and McLaughlin 2009, 2–3)

The fact that the unity of theory and practice is obscure to most scientists and philosophers of science is a social-historical byproduct of labor division. In capitalist society, specialization and abstraction go hand in hand. The connection between *theory* and *praxis*, between science and its social roots and aims are only mediated in a world in which intellectual labor and physical labor are kept apart, and the latter is subordinated to the former. One aim of socialist society, so Bukharin, was to blur progressively the distinction between intellectuals and physical workers. In his vision of the future, theory and practice were destined to merge. The connection of science and economy should be established on a new basis expanding the model of economical planning to scientific production.

Reception of Marxist Historical Epistemology and Reaction to It

After World War II, a small but visible group of historians, especially in the West, continued the line traced by the Soviet delegates and produced significant social and material accounts on the history of science (Young 1990). As to the *Wirkungsgeschichte* of the Soviet challenge, Joseph Needham, in the second edition of *Science at the Cross Roads: Papers Presented to the International Congress of the History of Science and Technology [...] by the Delegates of the U.S.S.R.* (1971), claimed that a flourishing externalist tradition of studies in the history of science had emerged in the wake of the 1931 conference. According to him, "[Hessen's] essay, with all its unsophisticated bluntness, had a great influence during the subsequent forty years" (Needam 1971, viii). Furthermore, Needham acknowledged that his own multi-volume *Science and Civilisation in China* (1954, 1st vol.) was a result of stimuli from Bukharin, Hessen and the other Soviet delegates.

In a sense, the sociology of science launched by Robert Merton was linked with the same legacy. In *Science, Technology and Society in Seventeenth-Century England* (1938), he stated that he derived from Hessen important insight concerning the relation between science, technology and society in the age of Isaac Newton:

> In the discussion of the technical and scientific problems raised by certain economic developments, I follow closely the technical anal-

> ysis of Professor B. Hessen in his provocative essay, "The Social and Economic Roots of Newton's Principia," in *Science at the Cross Roads* […] Professor Hessen's procedure, if carefully checked, provides a very useful basis for determining empirically the relations between economic and scientific development. These relations are probably different in an other than capitalistic economy since the rationalization which permeates capitalism stimulates the development of scientific technology. (Merton 1938, 501–502, n. 24)

Furthermore, a long critical assessment of Hessen's theses is to be found in George Clark's *Science and Social Welfare in the Age of Newton* (1937), a book devoted to the investigation of "the cooperation of science and economic life" (Clark 1970, 2), Chapter III "Social and Economic Aspects of Science," was almost in its entirety a detailed discussion of Hessen's 1931 article, in a rather critical but respectful way (Clark 1970, 61–86).

Bukharin and his group's provocation influenced significant developments of the history of science after World War II not only in a positive manner but also in the form of negative reactions. *Science at the Cross Roads* marked the beginning of an ideological bifurcation documented, in the 1940s, by the theoretical opposition between Edgar Zilsel's materialist approach to early-modern science and Koyré's intellectual historiography, and by the clash between 'internalist' and 'externalist' historians of science. Just as *externalist* history of science emerged from the Marxist camp, the *internalist line* had an *ideological* character, too. As a matter of fact, the majority of Anglo-American historians of science responded to the Soviet challenge by wiping out from their considerations all elements *external* to pure theory. Earlier authors, who did not belong to Marxist historiography and philosophy of science but stressed material or social aspects in the genesis and development of science, were also marginalized from the prevailing narrative, as was the case with Ernst Mach and Leonhard Olschki.

As indicated by Wolfgang Lefèvre, Koyré's 1943 essay "Galileo and Plato" can be seen as a manifesto of the anti-materialistic, anti-communist line (Lefèvre 2001, 11–13). Koyré's article begins with a brief overview of the adversaries' theses:

> This revolution [the Scientific Revolution] is sometimes characterized, and at the same time explained, as a kind of spiritual upheaval, an utter transformation of the whole fundamental attitude of the human mind; the active life, the *vita activa* [i.e., the πράξις] taking the place of the θεωρία, the *vita contemplativa*, which until then had been considered its highest form. […] [According to this perspective,] the science of Descartes—and a fortiori that of Galileo—is nothing else

> than (as has been said) the science of the craftsman or of the engineer. (Koyré 1943, 400)

Koyré is quick to add: "I must confess that I do not believe this explanation to be entirely correct." Actually, his intention is to show that this explanation is *completely wrong*. He reduces the practical and social reconstruction of early-modern science to a form of Baconianism: "The attitude we have just described is much more that of Bacon [...] than that of Galileo or Descartes" (Koyré 1943, 400–401). This is the typical misunderstanding of Bukharin's and Hessen's positions.[8] Koyré confuses individual intentions with social functions, and immediately perceived application of knowledge with practical factors at a socio-economical level supporting certain practices and lines of natural investigation.

Koyré maintained that Galileo's and Descartes's "science is not made by engineers or craftsman, but by men who seldom built or made anything more real than a theory" (Koyré 1943, 401). This is his main point. Galileo's *mathematized physics* was a form of Platonic contemplation of the numbers and geometries hidden behind the natural phenomena. Just as Galileo was a Platonist (Koyré 1943, 424), the Scientific Revolution was a "spiritual revolution" (Koyré 1943, 403). This matches with the cliché image of Koyré, whose view has been often reduced to the following three points: mathematical Platonism, rejection of the sociology of sciences and rational idealization of the scientific process (Redondi 1987, 2–3). To these, Georges Canguilhem added a fourth pont, a proto-Kuhnian "anti-continuisme résolu" leading to a "théorie des révolutions scientifiques [laquelle] a bien été le moteur d'une révolution dans l'histoire des sciences et confère aux travaux de Koyré leur unité originale" (Canguilhem 1987, 9).

Kuhn was well aware of the polemical meaning of Koyré's disembodied approach. As he observed in the footnote of the article "Mathematical versus Experimental Traditions in the Development of Physical Science," appeared in 1976 on the *Journal of Interdisciplinary History*:

> Note also the way in which distinguishing between [a pluralistic approach to science or a unitarian] [...] deepens and obscures the now far better known distinction between internalist and externalist approaches to the history of science. Virtually all the authors now regarded as internalists address themselves to the evolution of a single science or of a closely related set of scientific ideas; the externalists fall almost invariably into the group that has treated the sciences as one. But the labels 'internalist' and 'externalist' then no longer

[8] Cf. Freudenthal and McLaughlin (2009, 9).

> quite fit. Those who have concentrated primarily on individual sciences, e.g. Alexandre Koyré, have not hesitated to attribute a significant role in scientific development to extrascientific *ideas*. What they have resisted primarily is attention to socioeconomic and institutional factors as treated by such writers as B. Hessen, G.N. Clark and R.K. Merton. But these nonintellectual factors have not always been much valued by those who took the sciences to be one. The 'internalist-externalist debate' is thus frequently about issues different from the ones its name suggests, and the resulting confusion is sometimes damaging. (Kuhn 1977, 32, n. 1)

Kuhn does not contribute to illuminate the reader about the implicit issues at stake he hints at. Koyré is more explicit. In fact, in a dense footnote in the Galileo-and-Plato article he makes his anti-Marxist intention clear. Koyré contrasts there his own views with those of Marxist exponents (Koyré 1943, 401, n. 6). In particular, he indicates two works stemming from the Frankfurter Schule: Franz Borkenau's *Der Übergang vom feudalen zum bürgerlichen Weltbild* (Paris, 1934) and Henryk Grossmann's rectification, *Die gesellschaftlichen Grundlagen der mechanistischen Philosophie und die Manufaktur* (Paris, 1935). Whereas Borkenau's image of "Descartes artisan" is quickly dismissed as an "absurdity," Grossmann's writing is referred only for its criticism of Borkenau's too simplistic economicism, and not for its counter-proposal, which in many respects coincides with that of Hessen.[9] After them, Koyré turns to Leonhard Olschki, treating his interpretation of Renaissance science as the outcome of the technological culture of the late Middle Ages, as if it was just the same interpretative line of Borkenau, Grossmann and Zilsel. For that 'socialist sin,' Olschki has to be banned from historiography of science, as well. Koyré mentions also Zilsel's essay "The Sociological Roots of Science" (*The American Journal of Sociology* 47, 1942) for its stress on "the role played by the 'superior artisans' of the Renaissance in the development of the modern scientific mentality." For Koyré science cannot be anything else than a *mental* issue, even when presenting the viewpoints of those who contend this assumption and stress its extra-mental origins. Remarkably, Koyré makes no mention to the Soviet papers of the 1931 conference. In general, he avoided mentioning even the name of Marx apart from one *lapsus*, in a postscriptum of 1961 to an essay of 1930, "Les études hégeliennes en France," which is revelatory of his profound aversion to Marx and his followers:

> [E]nfin—*last but not least*—l'émergence de la Russie soviétique comme puissance mondiale et les victoires des armées et de

[9]Cf. Freudenthal and McLaughlin (2009).

> l'idéologie communiste [...] Hegel *genuit* Marx; Marx *genuit* Lenine; Lenine *genuit* Staline.[10]

To sum up Koyré's perspective, he intentionally construed an immaterial and spiritualist alternative to the *dangerous* social and material historiography of science. Thereby he inaugurated a politically-correct historiography that was to be embraced by influential US-American scholars. Among them, Thomas Kuhn, who was a close collaborator of the anti-communist designer of education policies, James B. Conant, praised Koyré as one of the most important recent scholars in his field. In another essay, "Alexandre Koyré and the History of Science: On an Intellectual Revolution," Kuhn extolled the merits of the former, in particular of his *Études galiléennes* (Paris, 1939), as a work inaugurating a novel approach:

> Within a decade of their appearance, they [the *Études galiléennes*] and his subsequent work provided the model which historians of science increasingly aimed to emulate. More than any other scholar, Koyré was responsible for [...] the historiographical revolution. (Kuhn 1970, 67)

Does not this claim for a *historiographical revolution* in a *historical* discipline sound bizarre? Probably, as Kuhn observed relative to the 'internalist-externalist debate,' also this *Koyréan Historiographical Revolution* was "about issues different from the ones its name suggests, and the resulting confusion is sometimes damaging." The immateriality, or "Platonism," of Koyré's proposal offered an alternative to out-dated positivism which, at the same time, avoided the pitfalls of socio-economical historiography. This conferred Koyréan history of science all the characteristics needed for the construction of an anti-communist History and Philosophy of Science.

The Harvard Entwurf of a HPS for a "Free Society"

Kuhn's academic formation was affected by the militant and anti-communist cultural climate of Harvard in the 1940s and 1950s. His mentor, Harvard president Conant, occupied crucial political positions. During the Second World War, in 1940, he became a member of the National Defense Research Committee and one year later he became its chair. He then entered the cabinet supervising the atomic bomb project and had direct responsibility for the uranium fission. As reported in a biographical memoir issued by the American Academy of Sciences, "on Conant's recommendation in the spring of 1942, this project was expedited

[10] I quote from Elkana (1987, 141).

by direct, industrial-scale plant construction carried forth simultaneously on four different ways of preparing fissionable material for atomic weapons. Three of the four methods were successful, and all contributed to the successful [sic!] bomb of 1945" (Bartlett 1983, 100). In the 1950s, Conant became chairman of the Anti-Communist Committee on the Present Danger.

Concerning Kuhn's student years, they were marked by the World War. "After Pearl Harbor—so Conant in his autobiography—and until V-J Day in August 1945, Harvard was primarily a university at war. Before the academic year 1941–1942 was over, a gradual exodus had begun. Some took commissions in the armed forces, some in civilian war agencies; almost without exception, the physical scientists were enrolling in one or another of the government-supported secret laboratories located in various institutions of higher learning" (Conant 1970, 363). During the wartime Kuhn made himself visible with public declarations in favor of the president's policy. He authored an editorial in the daily student newspaper, The *Harvard Crimson*, in which "he supported Conant's effort to militarize the universities in the United States. The editorial, of course, came to the attention of the administration, and eventually Conant and Kuhn met" (Marcum 2005, 6). Conant had also organized a committee whose task was to outline the program for "a General Education in a Free Society," whose ideological commitment is clear.

Kuhn greatly benefited from the power and visibility of his mentor. It has been remarked that "Kuhn's intellectual gestation at Harvard (1940–1956) enabled him to acquire, with little effort of his own, […] 'the strength of the weak ties.' […] Kuhn had a singularly strong tie to Conant, who in turn had many weak ties to opinion leaders in American society" (Fuller 2000, xiv).

Part of Conant's educational project was to disseminate scientific knowledge among the general public, in an age when scientific-technological programs required the support of a wide public opinion. At Harvard he planned classes of history of science for upper-level undergraduates, merging humanities and sciences. In 1947, he appointed Kuhn as an assistant and, in the fall of the following year, sponsored him a Harvard Junior Fellowship, which Kuhn spent to initiate his investigation of the history and philosophy of science. One of the first fruits of this research was his textbook on early modern astronomy, *The Copernican Revolution* (1957), which appeared in Conant's series of *Case Histories in Experimental Science* (Swerdlow 2004, 71–76). In the preface Kuhn cherished his benefactor:

> Many friends and colleagues, by their advice and criticism, have helped to shape this book, but none has left so large or significant a mark as Ambassador James B. Conant. (CR, xi)[11]

In return, Conant endowed Kuhn's book with a foreword, which began with a reference to the ideological curtain that was being built in the aftermath of the Second World War:

> In Europe west of the Iron Curtain, the literary tradition in education still prevails. An educated man or woman is a person who has acquired a mastery of several tongues and retained a working knowledge of the art and literature of Europe. By a working knowledge I do not refer to a scholarly command of the ancient and modern classics or a sensitive critical judgment of style or form; rather, I have in mind a knowledge, which can be readily worked into a conversation at a suitable social gathering. An education based on a carefully circumscribed literary tradition has several obvious advantages: the distinction between the 5 to 10 percent of the population who are thus educated and the others makes itself evident almost automatically when ladies and gentlemen converse.

Conant's words imply that east of the Iron Curtain the humanistic tradition had been interrupted along with the abolishment of "ladies and gentlemen." He implicitly excluded the communist camp from the "Western culture," which he celebrated as the educational basis for the *free society* he considered himself a "Social Inventor" of (Conant 1970). Note, in the abovementioned passage, the elitist understanding of culture as the privilege of a small group of gentle conversant people, not to be confused with populace. Note moreover the Eurocentric viewpoint.

Koyré was not less exclusive and Eurocentric than Conant.[12] *From the Closed World to the Infinite Universe* was affected by acute hellenophilia: "The conception of the infinity of the universe, like everything else or nearly everything else, originates, of course, with the Greeks."[13] In this cultural context, Koyré became the paradigm for an elitist-rational, Eurocentric and *spiritualized* history of science, to be opposed to the economy-and-technology narrative of those sympathizing with socialist ideas. On his part, Kuhn did not limit himself to continue the Koyréan program for the history of science. He also implemented on its basis a politically-correct philosophy.

[11] At that time Conant was US Ambassador in Western Germany.
[12] A "hardcore elitist" according to Elkana (1987, 129).
[13] Cf. Conner (2005, 117).

The resulting epistemology, that of the *Structure*, was irreconcilable with the most important theses of the Marxist program outlined by the Soviet scholars. To notice this opposition, it is sufficient to consider the following crucial epistemological assumptions of Kuhnian epistemology:

1. *Irrelevance of the economic structure*—In the *Structure* no technical or practical aspects significantly account for the historical development of science. The economical basis is completely absent. Thus, the *structure* underlying science has nothing to do with the socio-economical basis. It is rather a conceptual framework. Science is a cumulative but discontinuous intellectual process, framed in conceptual structures and punctuated by revolutions of thought.
2. *Individualism of discovery*—Second, Kuhn's scientists are not creative as a collectivity but only, rarely, as individuals. The community of those practicing "normal science" is rather a conservative majority. Accordingly, Kuhn assumes that scientific discovery is individual.
3. *Mysticism of discovery*—Kuhn does not dismiss or explain the *mystery* of discovery, in one word, *geniality*, which is the inexplicable element in intellectual history: "The new paradigm […] emerges all at once, sometimes in the middle of the night, in the mind of a man deeply immersed in crisis" (SR, 89–90).
4. *Contingency of historical development*—Fourth, the development of science is contingent. Kuhn, even more than Koyré, was convinced of this. A historiography centered on technology and economy menaced to foster deterministic views. This, at least, was Bukharin's idea of Marxist historiography, on which also his program in HPS rested. This could be seen as the weak point of Marxist historiography, namely determinism. What Koyré and Kuhn were probably unaware of (or rather not interested in) was the fact that Bukharin's naturalization of social processes was lively debated and even criticized also within the Marxist camp. György Lukács and Antonio Gramsci, to mention two influential Marxist thinkers, wrote harsh criticisms of Bukharin's deterministic viewpoint, which they regarded as misled and fatalistic.[14] Moreover, in the Soviet Union scholars prone to a *scientist* Marxism and a determinist understanding of historical development (the so-called 'mechanists') and Bukharin were involved in heated philosophical polemics and were strongly reprimanded in the early 1930s.[15] During this quarrel, scientism and scientist Marxism, labeled as "mechanistic materialism," were condemned in Soviet Union by the Communist Academy,

[14] See Sochor (1980, 707–712); Omodeo (2010).
[15] Cf. Kolakowski (2005, 841).

in 1929, and this condemnation invested also Bukharin's philosophy, seen as "anti-dialectical pseudo-Marxism" (Mirsky 1931, 652).

The Copernican Question: a Material or a Celestial Question?

In the cultural climate of the Cold War, Kuhn's anti-determinist, anti-economicist and intellectualistic historical epistemology represented a politically correct alternative to approaches suspected of being too close to the ideology of the Soviet camp. Regarding the *Copernican Revolution* the question to be addressed concerns the role Copernicus played from the point of view of the historical and epistemological debates of the time. For this assessment, I would like to briefly recount the meaning ascribed to the heliocentric astronomer in earlier accounts on early modern science.

According to a reputed history of physics, Ernst Mach's *Die Mechanik in ihrer Entwickelung: historisch-kritisch dargestellt* (1883), cosmology played a subaltern role in the development of physics, since the connection between mechanics and planetary theory occurred at a late stage in the history of mathematical physics (thanks to Kepler and especially Newton) (Mach 1942, 231 ff.). Mechanics evolved independently of astronomy, regarding its theoretical premises (e.g. the Archimedean legacy) as well as its material, technical and social roots. The development of statics from ancient times to the Renaissance, one reads, "illustrates in an excellent manner the process of the formation of science generally. [...] These beginnings point unmistakably to their origin in the experiences of the manual arts" (Mach 1942, 89). As far as dynamics is concerned, celestial physics was an extension of terrestrial mechanics to astronomy. Quite naturally, this stress on the practical roots of science met with the approbation of Bukharin and his like. By contrast, Koyré's later narrative of the Scientific Revolution as originating from a cosmological turn was fit to immaterialist conceptions about the emergence of early modern science. As such, it offered a counter-history to the history of physics propounded by Mach and his direct or indirect followers.

A critical point in Mach's narrative lies in neglecting the import of the physical questions that arouse from post-Copernican astronomy. In particular, the thesis of terrestrial motion had to be accompanied by a new explanation of gravity and motion accounting for the vertical fall of heavy bodies in a dynamic system. (The geocentric and geostatic model of Aristotle and Ptolemy just took for granted that heavy bodies have a simple tendency toward the cosmological center of the world *quo* center of gravity). Mach seemed to regard as relevant for the history of physics only the mechanical treatment of the solar system as a whole. Koyré took the opposite direction. Contrary to Mach, he emphasized the role of astronomy in order to construe a foundational myth of modern science.

He depicted the Scientific Revolution as a break with the past that began with a cosmological transformation. That Copernican transformation was continued by scientific-philosophical thinkers (only *pure* thinkers!) like Giordano Bruno, Galileo Galilei and René Descartes, and concluded with Newton's new synthesis of terrestrial and celestial physics. Furthermore, Koyré reduced this process to a few central ideas. In physics he allotted primary importance to the principle of inertia, first conceived by Galileo as a solution to the open problems of the heliocentric theory (Koyré 1978, 131). Koyré thus assumed that the scientific revolution had a *prologue* and an *epilogue* in the heavens, with Copernicus's system at its outset and Newton's unification as its destination. Hence, contrary to Mach's opinion, modern dynamics originated from celestial concerns. For Koyré celestial mechanics was not the extension of a science stemming from practice but, rather, it was a discipline much closer to theology than to technology. Plato was for him the philosopher who inspired in Renaissance authors an almost religious reverence for mathematical abstraction. Kuhn's image of Copernicus "the Platonist" fits well in this scheme:

> Neoplatonism is explicit in Copernicus's attitude toward both the Sun and mathematical simplicity. It is an essential element of the intellectual climate that gave birth to his vision of the universe. (CR, 131)

As far as the epistemological premises are concerned, Mach did not consider science a pure and disinterested endeavor. Rather, he postulated a *principle of economy* for both epistemology (theory choice as dependent on thought economy) and historiography (science as rooted in practical needs of the human species). This "vulgar" perspective met severe oppositions. Among others, Max Planck began a philosophical polemic with him as early as in 1908. On the occasion of a public talk at the University of Leiden, he attacked Mach's principle of economy, which he deemed to be philosophically undesirable because it led to relativism. Moreover, Planck claimed that such an explanatory principle was in disagreement with the history of science. As he declared, the most important modern scientists—*in primis* Copernicus, Kepler and Newton—were motivated in their inquiries by their desire to reach the truth, that is, by their aspiration toward objective knowledge, and not for the idle affirmation of their own "*Illusionen*." Here the relevant passage from Planck's talk follows:

> Zum Schluß noch ein Argument, das vielleicht auf diejenigen, welche trotz alledem den menschlich-ökonomischen Gesichtspunkt als den eigentlich ausschlaggebenden hinzustellen geneigt sind, mehr Eindruck macht als alle bisherigen sachlichen Überlegungen. Als die großen Meister der exakten Naturforschung ihre Ideen in

> die Wissenschaft warfen: als Nikolaus Kopernikus die Erde aus dem Zentrum der Welt entfernte, als Johannes Kepler die nach ihm benannten Gesetzte formulierte, als Isaac Newton die allgemeine Gravitation entdeckte, als Ihr großer Landsmann Christian Huygens seine Undulationstheorie des Lichtes aufstellte, als Michael Faraday die Grundlagen der Elektrodynamik schuf [...], da waren ökonomische Gesichtspunkte sicherlich die allerletzten, welche diese Männer in ihrem Kampfe gegen überlieferte Anschauungen und gegen überragende Autoritäten stählten. Nein – es war ihr felsenfester, sei es auf künstlerischer, sei es auf religiöser Basis ruhender Glaube an die Realität ihres Weltbildes. (Planck 1958, 28)

Planck's narrative, akin to later ones by Koyré and Kuhn, supposed titanic efforts on the part of individual epistemic warriors aiming to besiege the bulwarks of tradition. No dirty interests animated their efforts, nor could biological or social-economical drives account for the origins of science in its highest form. Planck assumed that science was born out of a disinterested desire for truth. Yet, Planck was not Duhem. Correspondingly, his philosophical outlook was different also from the later Koyréan. In fact, Planck did not look for a spiritualized conception of science. Rather, he sided with the positivistic ideal of science and a materialistic view of nature. Scientific advance, so Planck, is a de-anthropomorphizing process moving away from subjectivity and striving for objectivity.

> Worauf es hier einzig und allein ankommt, ist die Anerkennung eines solchen festen, wenn auch niemals ganz zu erreichenden Zieles, und dieses Ziel ist [...] die vollständige Loslösung des physikalischen Weltbildes von der Individualität des bildenden Geistes. Es ist dies eine etwas genauere Umschreibung dessen, was ich oben die Emanzipierung von anthropomorphen Elementen genannt habe, um das Mißverständnis auszuschließen, als ob das Weltbild von dem bildenden Geist überhaupt losgelöst werden sollte; denn das wäre ein widersinniges Beginnen. (Planck 1958, 27–28)

By contrast, Koyré's historiography focuses precisely on the relation between *Weltbild* and *bildender Geist*. There is no space for "objectivity" in his historical reconstructions. He never looks at the relation between scientific investigation and the 'world.' As has been noted, he "wrote in terms of the interaction of what his heroes thought about the world and what they thought about their knowledge" (Elkana 1987, 116). Accordingly, "the ideas to which Koyré attributes the greatest importance as factors of change are all ideas about knowledge and not about the world" (Elkana 1987, 118). In the first place, in his eyes, scientific changes are epistemological and metaphysical. One can say, that they are "shifts of paradigm" in the Kuhnian sense. Even more than in the *Études galiléennes*, in *From the Closed World to the Infinite Universe*, Koyré made his epistemological-metaphysical focus noticeable. In this work, he pointed to two pillars of the cosmological revolution, namely mathematization and infinity, which are epistemological and metaphysical, respectively. Both pillars had *spiritual* import for him. Koyré had already argued for the Platonic flavor of the Galilean use of mathematical abstractions in physics in other writings. As far as infinity is concerned, in the *Closed World* boundlessness was seen as the application of a theological-metaphysical concept to nature. In fact, the idea of the *infinite universe* had theological origins. For the *economy* of this story, Koyré predated the beginning of the modern revolution of thought known as Scientific Revolution and traced it back to the speculations of a theologian and metaphysician, Cardinal Nicholas Cusanus.

However concerned about the spiritual dimension of science, Koyré never renounced the idea of a *rationale* underlying the history of science, perhaps in the form of a teleological drive. Accordingly, the image of the infinite universe *had to* triumph over that of a closed world just as the heliocentric system *had to* triumph over the Aristotelian-Ptolemaic cosmos. Kuhn radicalized the spiritual element of Koyré's narrative by radicalizing the contingency of the paradigm shift from geocentrism to heliocentrism. It is curious but symptomatic the manner in which he constantly resorted to metaphors and termini stemming from religion to depict the early dissemination of the Copernican system. Here a list of significant passages follows:

> The state of Ptolemaic astronomy was a scandal before Copernicus' *announcement*. (SR, 67)

> Copernicanism made few *converts* for almost a century after Copernicus' death […] This difficulties of *conversion* have often been noted by scientists themselves. (SR, 150–151)

> Those who Copernicus *converted* to the concept of a moving earth […] (CR, 183)

> [H]e could embrace a cosmological *heresy*, the earth's motion. (CR, 184)

> Maestlin […] *gained a few converts*, including Kepler, for the new astronomy. (CR, 187)

> The group of *avowed Copernicans* […] (CR, 187)

> [W]hatever their *beliefs* about the position and motion of the Earth. (CR, 187)

> Copernicus's innovation seemed absurd and *impious*. (CR, 188)

The image here suggested is that of a faith dealer and his apostles preaching a new gospel. This idea of the affirmation of the Copernican theory is indeed very far from Galileo's call for *sensate esperienze* and *certe dimostrazioni*. Rather, it matches with Kepler's account of his discovery of the Platonic *Mysterium cosmographicum*, the secret harmony underlying the Creation, in terms of divine enlightenment. It is not without reason that Kuhn assigned to Kepler the decisive role to make "the Copernican system work" (CR, 131). As one reads in *Copernican Revolution*, Kepler had "the decisive revolutionary role" of completing the heliocentric theory (planetary laws and *Rudolphine Tables*) and making it endure.[16]

In the following, I shall consider the historical and epistemological implications of the religious metaphor.

Copernican versus Ptolemaic Faith

Much has been written about Kuhn's best seller on the history of early modern astronomy. The most exhaustive study on internal and external factors in the conception and reception of Kuhn's *Copernican Revolution* is a monograph by Michał Kokowski, issued in 2001 as a volume in the series *Studia Copernicana*.[17] Another significant assessment is Robert S. Westman's "Second Look at Kuhn's *The Copernican Revolution*." In this paper, Westman points out the imagery of warfare used by Kuhn to depict the reception of Copernicus but also remarked that "the notion of conversion is an important corollary of the incommensurability thesis in *The Stucture the Scientific Revolutions*" (Westman 1994, 93–94). It is from the religious metaphor, although it is perhaps "not well developed in *Copernican Revolution*" (Westman 1994, 94), that I would like to start a historical-epistemological assessment and argue that Kuhn's recourse to it is revealing of theoretical difficulties entailed in his approach.

The first difficulty in Kuhn's narrative concerns the relationship between Copernican and Ptolemaic astronomies regarded as the opposition between

[16] Cf. Westman (1994, 104).

[17] Kokowski (2001). I would like to stress the relevance of Part 1, section I.4, also dealing with the Conant-Kuhn connection; of Part 2, section I,2, providing an overview of the first reactions to Kuhn's *Copernican Revolution*; and Part 2, chap. 4, where Kokowski critically discusses its limits (see also Kokowski (1993)). I shall like to thank Prof. Kokowski for sharing with me several of his views on Kuhn, his philosophy and work.

two *incommensurable paradigms*. Kuhn maintained that, since the heliocentric planetary system was the essential aspect of Copernicus's achievement, this was the only issue at stake in the dissemination of his major work, *De revolutionibus orbium coelestium* (Nuremberg, 1543).[18] Additionally, he conceived of "Copernican astronomy" as a paradigm with a coherent deductive-like structure. This means that, according to him, all elements of Copernicus's work and post-Copernican astronomy were systematically interconnected.

This premises led Kuhn to dubious conclusions, for instance that the Copernican background was essential for every advance in Renaissance astronomy, for instance, for those determining the supra-lunar nature of comets and *novae* during the sixteenth century.

> Late sixteenth-century astronomers repeatedly discovered that comets wandered at will thorough the space previously reserved for immutable planets and stars. The very ease and rapidity with which astronomers saw new things when looking at old objects with old instruments may make us wish to say that, after Copernicus, astronomers lived in a different world. (SR, 116)

That the observation of comets and supernovas in the second half of the sixteenth century undermined certain Aristotelian assumptions about the nature of the heavens is historically true (Tessicini and Boner 2013, "Introduction"). That this fact directly or indirectly stemmed from Copernicus is false, as can be easily argued considering the overview of sources on the comet of 1577–1578 included in Tycho Brahe's *De mundi aetherei recentioribus phaenomenis* (1588).[19] This is famously the work in which the Danish astronomer described his own geoheliocentric system for the first time. How many of the authors of cometary tracts reviewed by Brahe conceived of comets' observation as relevant for the heliocentric cause? Probably only a couple among them, for instance Michael Mästlin and Thomas Digges, and perhaps Cornelius Gemma. But they were a minority.[20]

Besides, Kuhn's viewpoint neglects the variety of early interpretations of Copernicus's work depending on the different interests and motivations of its readers.[21] Renaissance scholars confronting *De revolutionibus* did not mainly focus on the so-called hypotheses, that is, terrestrial motion and solar centrality and immobility. Many of them regarded *De revolutionibus* as the basis for new astronomical tables, such as Erasmus Reinhold's *Prutenicae tabulae* (1551);[22]

[18] Since then it has become a sort of challenge for Copernicus's scholars to count the early "Copernicans." See, for instance, Tredwell and Barker (2004).
[19] The standard reference is Hellman (1944).
[20] See Westman (2011, chap. IX).
[21] I have reconstructed various thematic lines of reception of Copernicus in Omodeo (2014).
[22] Cf. Gingerich (1993).

others, from Kaspar Peucer to Brahe, appreciated Copernicus's geometrical models renouncing Ptolemy's equant; others, like Rheticus, were enthusiastic about the substitution of a Ptolemaic 'anomaly' for the terrestrial circle about the Sun, which also provided a yardstick (the Earth-Sun distance) to establish planetary distances.[23] Only for the last issue the heliocentric theory was central. Thus, one might legitimately doubt whether adherence to heliocentrism is sufficient to define a Renaissance follower of Copernicus, as Kuhn did. Furthermore, is there anything more striking than the epistemological and philosophical differences among 'realist Copernicans' in the late sixteenth century? Take the cases of Giordano Bruno, Galileo Galilei and Johannes Kepler. Can one say that they worked within the same 'paradigm,' that they shared common ideas about science and nature, only due to the fact that they accepted the rotation of the Earth about its axis and around the Sun?[24]

Contrary to the incommensurability thesis, Kuhn himself had to notice that Copernicus worked in the wake of Ptolemy, from whom he derived his methodology, his conceptual tools and the structure of his major work. For those reasons, Kuhn called him "radical" and "conservative" at the same time (CR, 148), and his book "revolution-making" rather than "revolutionary" (CR, 135).

A further evidence against incommensurability showing the permeability of geocentric and heliocentric systems is documented by the exchange of arguments in favor of and contrary to terrestrial motion and the heliocentric system between famous scholars such as Brahe and Rothmann in their correspondence and, later, Galileo's indirect response to Brahe's criticism of Copernicus.[25] However, the proliferation of heliocentric and hybrid or geo-heliocentric planetary systems during the sixteenth and seventeenth centuries is the clearest evidence that the choice between different options concerning cosmological order was not just that between two major systems. The Jesuit astronomer Giovanni Battista Riccioli, in his *Almagestum novum* (1651), even enlisted—perhaps with some exaggeration—eight different geocentric options (including geo-heliocentric and homocentric world systems) and two geokinetic variants (one, in which only the daily rotation is admitted, and heliocentrism).

[23] Cf. CR (174–177). For a brief overview of the advantages of Copernicus's heliocentric theory in relation to the geocentric, see Swerdlow (2004, 88–90).

[24] For an assessment of the epistemological differences among "realist Copernicans," see Omodeo (2011).

[25] On the geo-heliocentric debates, see Granada (1996); on Galileo's reactions to Brahe, see Bucciantini (2003, 23–48, chap. 2, "Padova: Pinelli, Tycho, Galileo").

Experimentum Crucis?

A second historical-theoretical difficulty in Kuhn's account lays in the fact that the incommensurability thesis and the reduction of the historical meaning of Ptolemy and Copernicus to planetary hypotheses make *the choice between* those *two paradigms* extremely elusive, *almost inexplicable*, or even fortuitous.

> Paradigms gain their status because they are more successful than their competitors in solving a few problems that the group of practitioners has come to recognize as acute. [For instance…] Ptolemy's computations of planetary positions. (SR, 23)

This is Kuhn's starting point. Yet he has difficulties to apply it to Copernicus's case, due to the impossibility to indicate a decisive experiment capable of establishing the superiority of the heliocentric system over the geocentric during the Renaissance:

> "Crucial experiments"—those able to discriminate particularly sharply between two paradigms—have been recognized and attested before the new paradigm was even invented. Copernicus thus claimed that he had solved the long-vexing problem of the length of the calendar year. (SR, 153)

It should be remarked *en passant* that Copernicus was not able to provide the solution to the calendar reform which was carried out by the Jesuit mathematician Christopher Clavius for Pope Gregory XIII, in 1582, relying both on the *Alfonsine Tables* as well as on the "Copernican" tables of Reinhold. The determination of the length of the year and its application for the calendar reform, to which Copernicus referred in his book, cannot be regarded as "crucial experiments" testing two competing sets of planetary hypotheses.

Kuhn hints at "special telescopes to demonstrate the Copernican prediction of annual parallax" as an example of "predictions from the paradigm theory" (SR, 26). Indeed, the absence of any observable stars' parallax and the fact that the starry heaven appeared to be a sphere always bisected by any horizon for any observer on Earth, even after Galileo's inauguration of telescopic astronomy, was one of the main astronomical arguments against the circumsolar revolution of the Earth.

> Even Copernicus' more elaborate proposal was neither simpler nor more accurate than Ptolemy's system. Available observational tests […] provided no basis for a choice. […] Ptolemaic astronomy had failed to solve its problems; the time had come to give a competitor a chance. (SR, 75–76)

Kuhn was thus forced by historical evidence to acknowledge that the assumption that any criteria could univocally determine the choice between two competing paradigms does not fit the Copernican-Ptolemaic divide.

These difficulties can also be remarked by the contradiction between Kuhn's idea of a 'switch,' introduced in order to account for scientific revolutions, and the timing of the Copernican reception as it occurred in fact. On the one hand, in fact, he admitted that Copernicanism might count as an exception to the all-at-once-emergence thesis:

> In other cases, however—those of Copernicus, Einstein, and contemporary nuclear theory, for example—considerable time elapses between the first consciousness of breakdown and the emergence of a new paradigm. (SR, 86)

On the other hand, he deemed *paradigms' transition* to happen like a *sudden gestaltic switch* (SR, 111 and ff.).

> The new paradigm […] emerges all at once, sometimes in the middle of the night, in the mind of a man deeply immersed in crisis. (SR, 89–90)

The religious metaphor about the Copernican conversion maintains the pathos of a sudden revelation.

Plurality and Unity in Science

I have so far argued that the hypotheses-centered interpretation of Copernican and Ptolemaic astronomies as paradigms entails several difficulties linked to the Kuhnian theory of scientific revolutions. In fact, it can be demonstrably objected that,

1. *Ptolemaic and Copernican planetary approaches were permeable and commensurable*;
2. *There was no experimentum crucis* that could be used to establish the superiority of the heliocentric alternative in all respects;
3. *The transition from geocentrism to heliocentrism* did *not* happen as *a sudden conversion-like event*.

Kuhn was not willing to take into account the theoretical consequences of these historical statements. In particular he neglected evidence of commensurability through controversy and hybridization because he assumed that plurality is a clear symptom of crisis. He contrasted in fact the incertitude of theories' proliferation as a crisis state to scientific advancement within a well-established theoretical framework (what he referred to as "normal science"), for instance the Copernican planetary theory.

> Copernicus' […] famous preface still provides one of the classic descriptions of a crisis state. (SR, 69)

> Proliferation of versions of a theory is a very usual symptom of crisis. In his preface, Copernicus complained of it as well. (SR, 71)

> There was no longer one Ptolemaic system but a dozen or more, and the number was multiplying rapidly with the multiplication of technically proficient astronomers. All these systems were modeled on the system of the *Almagest*, and all were therefore 'Ptolemaic.' But because there were so many variant systems, the adjective 'Ptolemaic' had lost much of its meaning. The astronomical tradition had become diffuse. (CR, 139–40)

Contrary to historical evidence, Kuhn assumed that Copernican astronomy was different from Ptolemaic and was able of substituting plurality for unity. Apart from the fact that this interpretation is at odds with the proliferation of cosmological and planetary models after Copernicus, one might legitimately ask: why should unity be superior to variety since the history of Renaissance astronomy witnesses rather to the contrary?

This remark can be extended to other periods and intellectual shifts. For instance, in an article on Kuhn's employment of his epistemological categories to the emergence of quantum theory ("a scientific revolution *par excellence*"), Jochen Büttner, Jürgen Renn and Matthias Schemmel argued, against the gestaltic-switch thesis, that "crisis" was the outcome rather than the source of theory discontinuity and that crisis might even count as a feature of "normal science" (Büttner, Renn, and Schemmel 2003, 56).

Controversy versus Linguistic / Conceptual Misunderstanding

Kuhn's conversion-like treatment of the Copernican paradigm shift downplays the argumentative strategies employed in the controversy over the heliocentric and geokinetic theories. Scholars' choice between terrestrial mobility and immobility was indeed complex and depended on the weight they attached to special aspects at the expenses of others, as well as on their philosophical and cultural choices, and their political and religious bias: e.g. the lack of observable stellar parallax and the physical and scriptural difficulties were enough for Brahe to reject terrestrial motion but not for Bruno, Galileo and Kepler who developed very different counterarguments depending on their philosophical backgrounds and convictions.

By contrast, Kuhn tended to treat controversies as mere misunderstandings. For instance, he linked Copernicus's preference for mathematical-astronomical harmony, as opposed to Aristotelian natural philosophy, to his lack of comprehension of the reasons against terrestrial motion resting on terrestrial physics:

> But an excessive concern with the heavens and a distorted sense of values may be essential characteristics of the man who inaugurated the revolution in astronomy and cosmology. The blinders that restricted Copernicus' gaze to the heavens may have been functional. They made him so perturbed by discrepancies of a few degrees in astronomical prediction that in an attempt to resolve them he could embrace a cosmological heresy. (CR, 184)

Kuhn's statement has no historical evidence since Copernicus's education, his concerns about scholastic and theological opposition to his hypotheses as well as his first-hand knowledge of Aristotle bear witness to the contrary.[26] Thus, Copernicus's writings document his awareness and commitment in favor of precise epistemological and philosophical views instead of blindness depending on his disciplinary affiliation as a mathematician. Let us consider also this quotation:

> Since paradigms are born from old ones, they ordinarily incorporate much of the vocabulary and the apparatus, both conceptual and manipulative, that the traditional paradigm had previously employed. […] Consider, for an example, the man who called Copernicus mad because he proclaimed that the earth moved. […] Part of what they meant by 'earth' was fixed position. […] Correspondingly, Copernicus' innovation was not simply to move the earth. Rather, it was a whole new way of regarding the problems of physics and astronomy, one that necessarily changed the meaning both of 'earth' and of 'motion.' (SR, 149–50)

It might be true that the meaning of many traditional concepts changed alongside the geokinetic perspective. Yet, the fact that concepts had to be defined anew does not imply that "Copernicans" and "anti-Copernicans" did not understand each other, as Kuhn's incommensurability thesis suggests. Bruno, for one, explicitly referred in his writings to the fact that the expression "world" (Latin, *mundus*, and Italian, *mondo*) had a different meaning according to the

[26] See for instance Goddu (2010).

Aristotelian definition of "cosmos" than according to the Epicurean one as "celestial body," which he preferred.[27] He moreover added a third definition of *mundus*, now obsolete, as star-centered planetary system. This example testifies that Renaissance and early modern intellectuals were not incapable of discussing definitions and understand others' philosophical approaches in spite of the fact that they could rebut certain definitions and approaches as undesirable or wrong. This is also clear from the most celebrated *Dialogue Concerning the Two Chief World Systems* by Galileo, which is in fact a discussion of arguments and counterarguments in favor of geocentrism and heliocentrism. Furthermore, Kuhn's example of "the man who called Copernicus mad" is out of purpose to illustrate the alleged linguistic and conceptual changes depending on "paradigm shifts." That man was in fact Martin Luther who is reported to have rejected the heliocentric system on the basis of scriptural passages. I really doubt that, on this point, Luther could miss the Copernican meaning of 'earth' and 'terrestrial motion.' He simply dismissed this opinion knowing what he was rejecting. Furthermore, *pace* Kuhn, Luther barely referred to the Bible and not to Aristotle or Ptolemy, and did not really care about the Aristotelian definition of earth as a heavy and fixed element but only about the literal meaning of certain scriptural passages.

Concluding Remarks

The issue of paradigms and paradigms' shifts has been crucial in the reception and discussion of Kuhn's epistemology from the very beginning. In particular, incommensurability, connected with the thesis of the gestaltic switch, seemed to many commentators to downplay or even neglect the centrality of rational argumentation in the development and discussion of scientific theories. Kuhn faced the criticism of irrationality on several occasions, beginning with his "Reflections on my Critics" that was included in the proceedings of the 1965 *International Colloquium in the Philosophy of Science* (Lakatos and Musgrave 1970, 231–278). In a postscript to the 1969 edition of the *Structure*, Kuhn answered to his critics' objections to the non-argumentative character of the choice between two paradigms—brief, of a "scientific revolution."[28] In the section entitled "Exemplars, Incommensurability, and Revolutions," he just reaffirmed his point of view stressing the difference between a scientific controversy that takes place within a given framework of accepted rules and premises (i.e., "normal science") and discussion over the premises of the scientific discourse themselves. Con-

[27] See Omodeo (in press).

[28] Cf. Kokowski (2001, 136–138). For critics of Kuhn's concepts of paradigm and paradigm shift, see Lakatos and Musgrave (1970).

troversies over foundational aspects ultimately rest on persuading colleagues and new generations within the scientific community. To corroborate his thesis, Kuhn thus introduced a sociological element into epistemology. Still, this shall not obscure the profound difference between such minimalist sociologization of science and Marxist historical materialism. Kuhn's perspective did not abandon the intellectualistic understanding of scientific advance and never embraced in his treatment socio-economical and political factors. "Sociology" for him never meant anything more general than academic interactions and exchanges at the level of the scientific community. Nor did Kuhn ever try to overcome the individualist characterization of discovery. By contrast, in his theory the moment of discovery remained the inexplicable moment of paradigm shift—notwithstanding the fact that "awareness" could precede the full unfolding of a "paradigm."[29] In this sense, the epiphany-and-conversion metaphor is revealing of Kuhn's radicalization of contemporary claims for the intrinsic intellectualism of science. On this account, he went much further than his *maître à penser*, Koyré.

Steven Fuller argued for the structural correspondence between the Kuhnian paradigms and incommensurable Cold-War worldviews (Fuller 2000, 175). As we have seen, there are passages in Kuhn's writings documenting that his epistemology echoes political constellations. This is for instance evident in the manner he contrasted the 'French school' of Duhem and Koyré against the historiography affected by social preoccupations. Apart from this, I deem the attempt at intellectualization / spiritualization of science to be not less dependent on Cold-War and post-World-War cultural 'paradigms.' The crucial problem was the propagandistic necessity, within American democracy, to foster the wide support on the part of public opinion for scientific investments aimed to warranty the military superiority of the United States, even after the horrors of technological war had cast irredeemable doubts on the linearity and irreversibility of scientific progress. Kuhn offered an understanding of science restoring the 'innocence' of its public image. As Westman put it,

> What Kuhn neglected to say in *Copernican Revolution*, however, was that postwar science no longer gained its legitimacy in a sociopolitical order dominated by ecclesiastical universities but from an alliance amongst secular disciplines and secret agreements between the military, science, and bureaucratized universities. Science no longer earned its authority by showing its harmony with the Book of Genesis but by using radar technology to control the invisible realm across which airplanes were guided to their targets. (Westman 1994, 114)

[29] SR (86), where Kuhn pits the "consciousness of breakdown" to the "emergence of a new paradigm."

Conant's program of scientific popularization was closely connected with these political issues and Kuhn's *Copernican Revolution* proved the most successful textbook in his *Case Histories in Experimental Science*. As Kostas Gavroglu argued in a recent conference on *Science as Cultural Hegemony* (Barcelona, 22–24 January 2014), "scientific popularization and the various forms of knowledge in circulation are involved in the process of continuous rearticulations of the dominant hegemonic ideology."[30] The political program behind Conant's popularization efforts was precisely directed toward the US civil society aiming to create a public opinion supportive of the tremendous costs of war and post-war science.[31]

The religious vocabulary employed by Kuhn to describe the emergence of heliocentric astronomy is not just a matter of words (elsewhere Kuhn also employs military metaphors like "battle" and "victory").[32] Rather, it is symptomatic of certain difficulties entailed in his notions of paradigm and paradigms' shift which, in turn, were rooted in Cold-War mentality. In the postscript to the 1969 edition of the *Structure*, Kuhn explained the persuasive character of paradigm choice through religious imagery:

> The conversion experience that I have likened to a gestalt switch remains […] at the heart of the revolutionary process. Good reasons for choice provide motives for conversion and a climate in which it is more likely to occur. Translation may, in addition, provide points of entry for the neural reprogramming that, however inscrutable at this time, must underlie conversion. But neither good reason nor translation constitute conversion, and it is that process we must explicate in order to understand an essential sort of scientific change. (SR, 204)

As I argued on the basis of the Copernican case, there are some major difficulties concealed under the announcement-and-conversion metaphor. To the first class of difficulties belong the incommensurability thesis and its gestaltic-switch corollary accounting for the (alleged) lack of decisive experiments or arguments in favor of one of the two irreconcilable paradigms. A further issue is the one-idea-centered concept of paradigm, according to which intellectual history deals

[30] I am quoting from the conference pre-circulating paper.

[31] Cf. Nieto-Galan (2011, 453): "As chairman of the Anti-Communist Committee in the 1950s, and designer of science education policies, James B. Conant, Kuhn's mentor, strongly supported an uncontroversial, neutral science, which was to be transmitted to the younger generations as a taken-for-granted worldview far from any critical reflection on the material conditions of thought. The Structure reinforced the idea that the scientific process remains essentially the same whenever and however it occurs." As standard references on popularization and cultural hegemony, see Shapin and Barnes (1977) and Cooter and Pumfrey (1994).

[32] For a treatment of Kuhn's rhetoric strategies in support of his narratives, see Kokowski (2001, 160–199).

with the production and effects of single ideal entities (say, the *heliocentric system, inertia*, or the *great chain of being*, to mention some of Kuhn's sources of inspiration)[33] instead of the constant combination and reorganization of clusters of ideas. In order to account for the historical development of science, the Copernican case suggests to recognize the dialogical-argumentative character of the natural discourse, the permeability of different worldviews and approaches to nature, as well as the composite character of natural and scientific conceptions. The latter are ideas' clusters marked by plurality and variety, rather than total systems hinged on one idea or a small set of ideas.

Still, as I have argued at length, the Kuhnian problematic cannot be reduced to the modeling of science and scientific processes. My main point has been to show that Cold-War mentality (if one prefers, "Cold-War *ideology*") significantly pervaded Kuhn's epistemological premises and conclusions. The historical axioms looming behind the thoughts and conceptions of Kuhn and of his contemporaries or immediate forerunners shall be investigated, questioned and reassessed, taking into consideration the material context out of which they emerged. After the end of the Cold-War Era and of its the ideological divides, we can better detect the political-cultural concerns and limitations lying behind the epistemological discourse of those years. Economic determinism and disembodied narratives seem to be the two opposite pitfalls that the exponents of the opposite camps were not always able to avoid in their role as intellectuals belonging to one of the *Two Chief World Systems* of the Cold-War Era. As for Kuhn, his understanding and practicing of historiography and philosophy was inscribed within these geo-cultural coordinates. As I said, the influence of Harvard president Conant, as an *organic intellectual* of McCarthyist US should not be underestimated. Kuhn's historiography, epistemology and even *popularization of science* represent a clever and successful unfolding of the cultural agenda of his time. Thus, notwithstanding the author's claims for *structural meta-historicity*, one can consider the Koyréan legacy and his account of Copernicanism to be deeply rooted in the political climate of the time. In conclusion, not only did the *Copernican Revolution* anticipate the epistemology of the *Structure* but, more importantly, political-theoretical assumptions guided and even distorted the historical reconstruction of Copernican astronomy.

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[33]Cf. Koyré (1939) and Maier (1951) for inertia-centered interpretations of classical mechanics. Cf. Lovejoy (1936) for the history of the idea of "the Great Chain of Being."

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Chapter 6
Contemporary Science and the History and Philosophy of Science

Olival Freire Jr.

Introduction

A reader of *Structure of Scientific Revolutions* (Kuhn 1970) is surely struck by Max Planck saying a new theory is accepted only when the supporters of the old one have died. This citation had a wider audience than physicists and historians of physics and it has been taken as emblematic of the idea of the incommensurability of paradigms. A second glance at this citation suggests that we reflect on the interaction with and the involvement in recent and contemporary science by historians and philosophers of science, taking particularly the case of quantum theory and Thomas Kuhn's views on it. Kuhn himself trained firstly as a theoretical physicist and later became a historian and philosopher of physics; he was also an admirer of quantum theory as one the most influential achievements in science. He was the head of Archives for the History of Quantum Physics (*AHQP*), a huge project launched in the early 1960s to collect documents and oral histories related to the creation of this physical theory. He devoted one of his main books on the history of science to this subject: *Black-body Theory and the Quantum Discontinuity 1894–1912* (Kuhn 1978). Thus I would like to take Thomas Kuhn and his *Structure of Scientific Revolutions* as a case for discussing the involvement of historians and philosophers of science in recent and contemporary science. In other words, I will take the relationship between one of the major scientific accomplishments Kuhn was influenced by, that is quantum theory, and his own work on the history and philosophy of science.

Furthermore I am particularly interested in the enduring controversy over the interpretation and foundations of quantum theory and Kuhn's own views on it. The debates on these issues first arose at the time of the theory's inception, lasting until the early 1930s. Subsequently, they were revived during Kuhn's lifetime in the 1950s. Thus, I would like to discuss issues such as: How Kuhn's works and views were shaped by quantum physics, its first dominant interpretation and the ongoing controversy that followed it. As it is widely acknowledged that Kuhn's *Structure of Scientific Revolutions* has had only scant influence on the historiography of science (and the debates at this workshop reflect this), I would like to know if the historiography on the quantum controversy reflects the Kuhnian corpus. Finally, if not through his published works, one wonders how Kuhn, through letters and unpublished material, reacted to the revival of the quantum controversy. This paper attempts to deal with these issues. After a brief review of the attention philosophers paid to quantum theory, I focus on Kuhn's case, mainly presenting Mara Beller's criticisms towards *Structure of Scientific Revolutions*. I then move beyond Beller's criticisms to compare the historiography on the quantum controversy, and the very existence of this controversy, with Kuhn's published and unpublished papers. I conclude by coming back to the general question of the role played by recent and contemporary science in the scholarship produced by historians and philosophers of science.

Quantum Debates and Philosophers

Kuhn was not the first philosopher to become interested in the debates on the interpretations and foundations of quantum theory. In the early stage of those debates, philosophers such as Karl Popper, Hans Reichenbach, Gaston Bachelard, Grete Hermann and Alexandre Kojève ventured into the field once the reserve of professional physicists. In the 1950s, with the controversy reheated especially because of the appearance of the causal interpretation suggested by David Bohm, there was new fuel for the philosophy of science. However, while in the 1930s philosophers mostly produced works of a more epistemological nature, in the sense of providing a critical analysis of an existent scientific theory, they now divided along the same lines as physicists. Some were sympathetic towards Bohm's enterprise, as in the case of Paul Feyerabend who praised Bohm's *Causality and Chance*, as containing "an explicit refutation of the idea that complementarity, and complementarity alone, solves all the ontological and conceptual problems of microphysics" (Feyerabend 1960, 321). Others aligned with Bohr's point of view, notably Norwood Hanson, who maintained that "when an interpretation of a theory has been as successful as this one [Copenhagen interpretation] has been, there is little practical warrant for the 'alternative interpretations' which have, since Bohm, been receiving prominence" (Hanson 1959, 1). And yet, there were cases, such as Bachelard, who retired from the debate as it became heated and de Broglie reconverted to the deterministic description of quantum phenomena (Freire Jr. 2004a). Since then, the debate on the foundations of quantum physics has been an attractive topic for philosophers of science. In the 1960s a new batch of philosophers entered the quantum field. Some were trained both in philosophy and physics, such as Abner Shimony, others were trained in physics, such as

Jeffrey Bub and Mario Bunge, and some of these physicists were philosophically minded, such as Bernard d'Espagnat. From the 1970s on there was a true industry of philosophically inclined investigations on the foundations of quantum theory. In hindsight, it can be said that it is hard to find a scholar with some training in physics and an interest in philosophy of science who has not devoted some attention to the issues in this field. As we will see, Kuhn seems to be one of those rare cases.

Thomas Kuhn and the Interpretation of Quantum Theory

The case of Kuhn and the quantum debates is not new in the literature. The first to spot the problem was the philosopher and historian of science, Mara Beller. She concluded her *Quantum Dialogue – The Making of a Revolution* (Beller 1999) by revisiting the debates on the quantum theory to make the contrast between dialogue, which she considered had been instrumental in the creation of quantum physics, but which had been abandoned just after its inception, and paradigms, which jointly with normal science and the incommensurability of paradigms, became a core concept in Kuhn's ideas about how science evolves. For Mara Beller (Beller 1999, 287–306), "the notion of paradigm has not only clear totalitarian implications but also dogmatic ideological roots." These roots Beller found in the alleged "close historical links […] between the notion of incommensurable paradigms and the ideology of the Copenhagen dogma." Beller's targets were Norwood Hanson's and Thomas Kuhn's views on the ways science evolves and the criticisms they leveled at Bohm's causal interpretation. As for disclosure of potential conflicts of commitment, it should be noted that Beller indeed sympathized with Bohm and the causal interpretation. While Hanson is the most documented case of such a link, insofar as he was involved with the criticism of Bohm's ideas and the defense of Bohr's, Kuhn's case is by far the most interesting, given the wider audience of his *Structure of Scientific Revolutions*. However, evidence of Kuhn's interest in the quantum controversy is scant. Beller cites just one fragment in which Kuhn makes reference to Bohm: "A similar […] feeling seems to underlie the opposition of Einstein, Bohm, and others, to the dominant probabilistic interpretation of quantum mechanics." However, the full citation is a little weaker as it includes "though more moderately expressed" between "similar" and "feeling" (Kuhn 1970, 163).

Kuhn was the head of the *AHQP* project (Kuhn et al. 1967) and in this capacity he became familiar with the quantum controversy, but only with the initial controversy until the early 1930s. According to the project report, "to reduce preparation time and also the number of men to be interviewed, the period to be covered systematically by interviews was terminated in the very early thirties rather than at the end of that decade as originally planned" (Kuhn n.d.). Thus, the revival of the hidden variables in the early 1950s, to use Max Jammer's words (Jammer 1974, 278), was beyond the scope of the *AHQP*. Furthermore, a quick perusal of the unpublished documents deposited in Thomas S. Kuhn Papers at MIT, in particular folders concerning his correspondence with Feyerabend, Lakatos and the *AHQP* project, did not provide evidence of Kuhn's interest in the debates about the interpretation of quantum theory, neither before nor after his writing of *Structure.* As enticing as Beller's suggestion is, it lacks plausible corroboration with documentary evidence. Thus let us go beyond Beller's argument and exploit Kuhn's attitude to the controversy over the interpretation of quantum physics further.

Beyond Beller's Criticism – What Kuhn Missed from the Quantum Controversy

Kuhn lived to see the quantum controversy play a role in the development of our own understanding of quantum theory as well as receive the attention of historians and philosophers. I am mainly speaking of the whole work—both theoretical and experimental—related to Bell's theorem, which led to the acknowledgment of entanglement as an irreducible quantum feature. Bell's theorem contrasted quantum theory with any attempt to complete quantum theory having local realism as an assumption. It was published in 1965 and from the early 1970s to the early 1980s there was a rush to perform experiments to decide on the disjunction carried out by Bell's theorem (Freire Jr. 2006). Experiments were resumed in the late 1980s exploiting technical advances (taking as sources of photon pairs photons from parametric down conversion in non-linear crystals) and merging these experiments with the then burgeoning field of quantum information. The experiments confirmed the quantum predictions, thus confirming much of the strangeness of quantum theory but they also helped to direct attention beyond physics to the debates on the foundations and interpretation of the quantum theory.

It is noticeable that the interest in foundations of quantum physics triggered by activities related to Bell's theorem did not pass unnoticed by historians, sociologists and philosophers of science. One of the most remarkable cases is the writing and publication of *The Philosophy of Quantum Mechanics – The Interpretations of Quantum Mechanics in Historical Perspective*, in 1974, by the historian of physics Max Jammer.

In fact, Max Jammer seems to have been the first author to grasp the historical relevance of the subject. He had intended to write a book on the development of relativistic quantum mechanics and quantum field theories after the completion of his book on the conceptual origins of quantum mechanics. However, his plans were changed when he realized that a new and more urgent subject had appeared.

Since Jammer's own appraisal of his change of plans is so evidential of the intellectual climate in the early 1970s concerning the foundations of quantum theory, I beg the reader's pardon for quoting in extenso his preface, written in 1988, to the second edition of his *The Conceptual Development of Quantum Mechanics* (Jammer 1989, emphasis is mine).

> As stated in the preface to the first edition in 1966, I had hoped to continue this line of research with a sequel volume on the conceptual development of relativistic quantum mechanics and quantum field theory. However, John Stewart Bell's paper on hidden variables which appeared in the July 1966 issue of the "Reviews of Modern Physics", together with his paper on the Einstein-Podolsky-Rosen paradox, threw new light on the interpretations of quantum mechanics. They initiated a development in which, among many others, the experimentalist John F. Clauser, who at that time attended my lectures at Columbia University in New York, and the theoretician Jeffrey Bub, with whom I had long discussions at the Minnesota Center for the Philosophy of Science in Minneapolis, were actively involved. *Prompted by these developments, I wrote "The Philosophy of Quantum Mechanics"* (Wiley-Interscience, New York, 1974) […]
>
> The reader may perhaps wonder why a book on the development of modern physics, dealing with historical issues that apparently are "faits accomplis", should have to be revised and emended, especially as Werner Heisenberg and Paul Dirac had approved the final draft. The reason is, of course, that the development of quantum mechanics, said to have reached its apex about sixty years ago, is nevertheless still an unfinished business today.
>
> In fact, the conceptual revolution brought about by quantum mechanics is so radical and penetrating that any theoretical innovation discovered today is apt to produce a re-interpretation and re-evaluation of results obtained in the past. A good example is the 1935 Einstein-Podolsky-Rosen Paradox which was presented at the end of the 1966 edition, but whose real significance became clear only through the above-mentioned work of Bell and his followers beginning in 1966.

Jammer was not the only scholar to have his attention diverted by the renewal of the controversy on quantum theory. Still focusing on the 1970s and the 1980s, we may cite studies by Pinch (1977), Brush (1980), Harvey (1980), Harvey (1981), Benzi (1988), Cross (1991), Graham (1972), in addition to works in the philosophy of science, such as Redhead (1987), and popular science books, such as Bernstein (1991). Noteworthy is the interest that the then new sociology of science, from the Edinburgh school, dedicated to the subject, with the works by Pinch, Harvey and Cross. Surely, the very existence of an ongoing scientific controversy attracted the attention of new practitioners of sociology of science; after all, scientific controversies cannot be understood by framing them exclusively with theoretical and experimental reasons. Other factors, even non-cognitive ones, need to be used in order to render them as intelligible events. In addition to the controversy related to non-locality, we would also like to point out the entire work on the very existence of a measurement problem in quantum theory was developed in the 1950s and the 1960s, but this need not be a concern here. In the 1980s, there were in fact two major intellectual events related to quantum theory. Firstly, physicists widely accepted entanglement, or quantum non-locality, as a new physical feature predicted by theory and corroborated by experiments. Second, historians, sociologists and philosophers were working on the process—a scientific controversy—which had led to the establishment of this new physical feature and its philosophical meanings, and to taking sides in the ongoing controversy.

Thus one may ask why Kuhn did not, as far as I am aware, say something on the debates on the foundations of quantum theory. Resuming Beller's views of the paradigm as a totalitarian concept for the practice of science with its dogmatic roots in the way the Copenhagen interpretation was preached and accepted, it is fair to ask if indeed the paradigm idea did not play its role in preventing Kuhn from noticing and valuing the renewed controversy over quantum theory. However, as we have seen, there is scant documentary evidence for Beller's claims. Thus, they will remain an overstatement, albeit a plausible one. The fact remains that Kuhn had all the skills as a physicist, historian and philosopher to contribute to the analysis of these debates, and he missed the opportunity. At the very least, we deal here with a weakness, or a lacuna, in Kuhn's reflections and legacy.

Philosophers and historians continued to be attracted to the study of the quantum controversy. Directly or indirectly related to it, we may list the following studies, among others, in the last two decades.[1] Taking these considerations as backdrop, it is easier to understand why this continued and enlarged scholarly work in the history and philosophy of science did not enter into a dialogue with Kuhn's works. In brief, Kuhn was silent on the major intellectual events related to the quantum controversy. In the rare cases where there was a dialogue, such

[1] Beller (1999); Bromberg (2006, 2008); Byrne (2011); Camilleri (2009a, 2009b); Cushing (1994); Forstner (2008); Freire Jr. (1999, 2003, 2004b, 2005, 2006, 2007, 2009, 2011a, 2011b, 2015); Freire and Lehner (2010); Freire Jr., Pessoa Jr. and Bromberg (2010); Gilder (2008); Howard 2004); Jacobsen (2007, 2012); Kaiser (2007, 2012); Olwell (1999); Osnaghi, Freitas and Freire Jr. (2009); Paty (1993, 1995); Pessoa Jr. (1998); Pessoa Jr., Freire Jr. and De Greiff (2008); Schlosshauer (2011); Wick (1995).

as Beller's, it resulted in a strong criticism of Kuhn's views. In a lower tone, at the beginning of my research on these subjects, I claimed that Kuhn's view could not help us understand the renewal of the quantum controversy (Freire Jr. 1999).

Conclusion

In defense of Kuhn, one can argue that historians and philosophers may be influenced by recent and contemporary science, but they do not necessarily follow contemporary science. I illustrate this point with two cases related to the quantum controversy. The first one is close to Kuhn, as it involves one of his students and enduring correspondents, the historian Paul Forman. The connections between Forman's claim of the social roots of acausality in quantum mechanics and the causal interpretation of quantum mechanics suggested by Bohm in 1952 are conspicuous. While Forman was aware of Bohm's work, he had not been particularly influenced by it. Instead, he was influenced by Einstein's enduring criticism of quantum mechanics, which Forman read as a quest for determinism. "What did impress me was Einstein's attachment to the goal of causal description," Forman recently recalled (Freire Jr. 2011a). The second one concerns Abraham Pais who, despite concluding his biography of Einstein after Alain Aspect's influential experiments on Bell's theorem in 1982, missed the far-reaching influence of the EPR *Gedankenexperiment* in twentieth-century physics (Pais 1982, chapter 25c).

Let us conclude by dismissing Beller's stronger claim and assuming a weaker one, that is, that Kuhn's inclinations in the quantum debate, even keeping silent on the renewal of the controversy, were in agreement with Bohr and Heisenberg, rather than with Einstein and Bohm. Let us assess this weaker statement. This stand did not contribute to Kuhn's appreciation of the intellectual, far-reaching meaning of the renewal of the controversy over the quanta. As we have seen, Kuhn was neither the first nor the last philosopher of science to take sides in the quantum dispute. In fact, almost all the philosophers of science in the twentieth century with some training in physics involved themselves in this controversy. Let us push the question further, could it have been otherwise? Since historians and philosophers do not work in cultural vacuums, I do not think so. The influence of achievements in quantum theory and the complementarity views on Kuhn are no different from the influence of Newtonian science on Kant's epistemology, the French revolution on Hegel's philosophy of history, or Darwinian evolution on current evolutionary epistemology. If Kuhn's philosophy was biased by the influence of quantum physics, and it seems to us it was, he was not alone in this kind of influence. Ultimately Kuhn did and had to have views on recent and contemporary science, views that shaped, at the very least, his research choices.

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Chapter 7
Kuhn in the Cold War
Ursula Klein

Fifty years after the publication of *The Structure of Scientific Revolutions*, historians and philosophers have been celebrating Thomas Kuhn and simultaneously criticizing him with respect to almost every part and parcel of his work.[1] Historians of science, in particular, question his emphasis on theory, his concept of overarching paradigms guiding the way science is done, as well as his concept of a universal structure of scientific revolutions. Mario Biagioli gave voice to recent historiographical trends, stating that "*Structure* was history-making and, half a century later, has itself become history" (2012, 479). In this essay, I am not concerned with Kuhn's *Structure* but with his equally influential *Mathematical Versus Experimental Traditions in the Development of Physical Science* (1976), which I will put into the context of Cold-War historiography of science.

In almost of all his historical and philosophical studies, Kuhn highlighted the role played by theory in the sciences. His interest in theory, in particular physical theory, is nicely illustrated by an episode Ian Hacking reported in his *Representing and Intervening* (1983). Hacking recalled that his colleague C. W. F. Everitt once wrote two papers for the *Dictionary of Scientific Biography*. One of them was on Fritz London, who was a theoretical physicist, and the other on his brother, the experimental physicist Heinz London. "The biography of Fritz was welcomed by the *Dictionary*," Hacking observed, "but that of Heinz was sent back for abridgement. The editor (in this case Kuhn) displayed the standard preference for hearing about theory rather than experiment" (Hacking 1983, 152).

Hacking's famous argument that experiments can have a life of their own implied a clear question mark concerning the scope of Kuhn's approach, with its emphasis on theory and paradigms (1983). Historical studies of experimentation in the 1980s, such as Latour and Woolgar's *Laboratory Life*, Shapin and Schaffer's *Leviathan and the Air Pump* and Galison's *How Experiments End* further undermined the significance of Kuhn's approach.[2] The main issues now discussed were scientific facts, intervening laboratory practices, instruments, tacit knowl-

[1] See, for example, the collection of essays in *Historical Studies in the Natural Sciences* 24 (5), 2012.
[2] Latour and Woolgar (1979); Shapin and Schaffer (Shapin and Schaffer 1985); Galison (1987).

edge, experimental representation and social hierarchy in the laboratory. Kuhn's view of science had even less of an impact on the new studies of material culture and materiality in the sciences.[3] Most of the questions highlighted in the latter studies, particularly those concerning the ways in which material objects condition scientific inquiry, are only of marginal importance in Kuhn's work. Thus, it is perhaps not too far-fetched to argue that Kuhn's approach and his approaches emphasizing practice and material objects are incommensurable.

There is one famous essay, however, which seems to contradict the now common view that Kuhn highlighted scientific theory at the expense of experimentation and material culture. His *Mathematical versus Experimental Traditions in the Development of Physical Science* (1976) not only addresses issues concerning the experimental sciences, but also the role of instruments in experimentation.[4] It seems to manifest a genuine interest in the experimental, or what he called "Baconian sciences," as well as in their technological context. The goal of this essay is to shed light on Kuhn's interest in writing the latter essay. Putting this essay into the context of Cold-War ideology, I will argue that Kuhn tried to find a middle ground between materialist explanations of early modern science and anti-Marxist arguments against the latter approach.

Kuhn's "Mathematical versus Experimental Traditions"

In his essay "Mathematical Versus Experimental Traditions," first published in 1976, Kuhn demarcated the 'classical physical sciences' from the 'Baconian sciences.' The Baconian sciences were, according to him, a novel type of sciences emerging in the period of the Scientific Revolution, and they were experimental sciences. By contrast, the classical physical science had a long tradition, but they were thoroughly reconstructed in the sixteenth and seventeenth centuries. Kuhn pointed out that the new Baconian sciences did not pursue theoretical goals, although theory (mainly corpuscular philosophy) often lurked in the background. Instead, their "typical products were the vast natural and experimental histories" (1977, 43). He further stated that Baconian experiments forcefully intervened into nature and that intervention required instruments. Hence, he argued that in "less than a century physical science became instrumental" (1977, 44). More interestingly, he observed that artisanal workshops were sites for the construction of scientific instruments as well as "subjects for learned concern," and he further

[3] See, for example, Lefèvre (1978); Latour (1987); Pickering (1995); Rheinberger (1997).

[4] Kuhn (1977, 31–65). Kuhn's *The Structure of Scientific Revolutions* (1962) includes no more than remarks on experiments and the experimental sciences; his essay *The Function of Measurement in Modern Physical Science* (1961) is more concerned with the quantification of physics than experimentation per se; *A Function for Though Experiments* (1964) is located at the borderline of experimentation and theory; see Kuhn (1977, 178–224, 240–265).

mentioned that some Renaissance and early modern "artist-engineers" participated in polite learning (1977, 57, 55).

Why did Kuhn study these kinds of issues that clearly deviated from the type of problems discussed in *Structure* and most of his other publications? It is by no means evident that natural and experimental histories were structured by paradigms, and that the concepts of "anomaly," "crisis," and revolutionary replacements of paradigms are able to grasp the work undertaken in experimental contexts. I argue that Kuhn's *Mathematical versus Experimental Traditions* was indeed a detour with respect to the bulk of his work, and that this detour was provoked by an ideological campaign of historians of science during the Cold War. In what follows, I briefly outline this campaign and then show how Kuhn positioned himself within it.

In 1959, Marshall Clagett published a collection of papers entitled *Critical Problems in the History of Science*, which was based on a conference that had taken place three years before at the University of Wisconsin. Kuhn was present at this conference. In a contribution to it, entitled *The Scholar and the Craftsman in the Scientific Revolution*, the historian of science Rupert Hall took issue with some recent arguments concerning the early modern sciences and their technological and economic context (Hall 1959). Without naming any of his opponents and in an almost perfect objective rhetoric, he vehemently rejected the argument that technological change and the accompanying social revaluation of craftsmanship and technical knowledge was one of the causes of the Scientific Revolution. While he conceded that artisanal and engineering practices had stimulated early modern scholars and provided opportunities for new scholarly observation, he mainly argued that the transformations in the Scientific Revolution were achievements just of scholars, not of any other persons living and working outside the academic world. According to Hall, the Scientific Revolution was an internal scholarly process, the result of an "internal strife" between "academic innovators" and "academic conservatives." Hall further emphasized that these "quarrels of learned men had as little to do with capitalism as with the protestant ethic" (1959, 7). The crucial point, according to him, was that the academic innovators had modified their "attitude" towards the arts and crafts, whereas the academic conservatives kept their traditional themes (1959, 16). Thus the academic innovators began to perceive things—most importantly "the success of craft empiricism"—that the conservatives continued to ignore. Hall argued that this change was entirely subjective; it had nothing to do with changes of production, trade and commerce: "It [the success of craft empiricism] was always there to be seen," and therefore "the change was in the eye of the beholder" in the early modern period (1959). Hall's critic cumulated in the clear demarcation of scholars from craftsmen and the rejection of what later historians called the "scholar-and-craftsman thesis." "This seems to me to be the defect of the view," he stated, "that sees the new scientist of the seventeenth century as a sort of hybrid between the older natural philosopher and the craftsman" (1959, 17).

Cold-War Historiography of Science

Who exactly were Hall's opponents? In the 1930s and early 1940s, Boris Hessen (1931), Franz Borkenau (1934), Henryk Grossmann (1935), Robert K. Merton (1938) and Edgar Zilsel (1941/42) had published studies on early modern interconnections between science, technology and the economy. Among these authors, Merton, an accepted member of the scientific community in the US, was perhaps the most unwelcome person.[5] In 1938, he had published an essay entitled *Science, Technology and Society in Seventeenth-Century England* (see below). Hall's formulation of the scholar-and-craftsman thesis, as well as his remarks about the protestant ethic and capitalism, point exactly in the direction of Merton. In a later essay, published in 1963, he formulated his critic of Merton more openly (Hall 1963). The "brilliant young scholar" Merton, he informed his readers, did not just argue the obvious, namely that "no one writing the history of science would ever divorce it completely from society's beliefs and structure." Rather, he dared to offer "principles of historical explanation," which "are complementary to, if they do not replace, those offered by the historian of science" (Hall 1963, 1). In other words, the brilliant young man was a threat to all good historians of science. What Hall did presumably not foresee, let alone wish, was the fact that his attack increased Merton's publicity and contributed to a re-publication of his 1938 essay in book form (Merton 1970).

In *Science, Technology and Society*, Merton argued that the Protestant ethos created a favorable milieu for the early modern sciences. This part of his book became later known as *the* Merton thesis. The bulk of this book, however, was concerned with a different issue, namely technology and capitalist economy as a context of the early modern sciences. In the main part of *Science, Technology and Society*, which was first criticized and later almost completely ignored, Merton presented a number of compelling case studies that led him to conclude that technical objects and socio-economic problems had an impact on early modern scientists' choice of problems.[6] They often provoked "shifts of interests" in scientific inquiry, he argued, or created "derived scientific interests." Referring to

[5] Merton was a student of George Sarton, who had invited him in 1938 to publish his "Science, Technology and Society in Seventeenth-Century England" in the journal *Osiris*. He became one of the most influential American sociologists.

[6] It should be noted that the term "scientist" is not fully appropriate with respect to the early modern period. But here and elsewhere in this paper I use the terms that historians used in the 1960s and 1970s.

the construction of early modern fighting ships, for example, Merton pointed out that "all the major problems [in this field] had become the object of scientific study" (1970, 178). This was a clear causal argument about social and technical stimulations of scientists' interests, which was directly opposed to Hall's view. Moreover, in his discussion of case studies Merton also pointed out that "the inventor and the scientist were often one" (1970, 146). Hall transformed the latter observation into a theoretical argument named "scholar-and-craftsman thesis."

As Merton's arguments partially overlapped with those of Zilsel and other authors identified as Marxists, they fell under the spell of a predominantly anti-Marxist ideology among Cold-War historians of science. Clearly, the main goal of Hall's argument was the identification of an intellectual target for an anti-materialist and anti-Marxist crusade, covered by a polite and apparently open-minded style. There were only a few members of the historical community who had some doubts that Merton was clearly on the wrong path, and Kuhn was among them. Instead of ignoring Merton's arguments, he included them in his teaching and began to publish on related issues.[7] In 1968, he wrote that "attempts to set science in a cultural context," such as Merton's, "might enhance understanding both of its development and of its effects."[8] He conceded that Merton's view owed "something to Marxist historiography," but he was also uncomfortable with the fact that his approach was "attacked with vehemence", as was exemplified by Hall's paper published in Clagett's volume (Kuhn 1977, 115). A better way to deal with it, he proposed, was "the revision of the Merton thesis" (Kuhn 1977, 117, 118). The result of this revision was his *Mathematical versus Experimental Traditions.*

It is thus not surprising that there are many thematic intersections between Kuhn's essay and Merton's book. But Kuhn did more than just taking up historiographical issues previously highlighted by Merton, Zilsel and others. Discussing also the theoretical dimension of the theme, he tried to find a theoretical middle ground between Hall's and Merton's view. In 1968 he wrote: "If Merton were right, the new image of the Scientific Revolution would apparently be wrong" (Kuhn 1977, 116). The "new image" was R. Hall's and Alexandre Koyré's that postulated that the "radical sixteenth- and seventeenth-century revisions of astronomy, mathematics, mechanics, and even optics owed very little to new instruments, experiments, or observations" (1977). Needless to add that this implied the denial that technology played any significant role in the Scientific Revolution. In this distinct historical situation, Kuhn proposed a new argument that had also been largely ignored by Merton, Zilsel, Hessen, Borkenau and Grossman, who had all focused on the mechanical and mathematical sciences as well. Kuhn reminded

[7] I thank John Heilbron for this information.
[8] The paper entitled *The History of Science* is reprinted in Kuhn (1977, 105–126, see p. 113.).

the community of historians of science that early modern "science" should not be equated with astronomy, mathematics, mechanics and optics. Instead he argued that the "new image" must also take into account the seventeenth-century studies of electricity, magnetism, chemistry and thermal phenomena along with the ideology of Baconianism. A "revised Merton thesis," he stated, must also promote our understanding of these experimental sciences (1977, 118).

In 1976, Kuhn had fully developed his argument. On the one hand, he conceded in his "Mathematical versus Experimental Traditions" that economy and technology actually had a significant impact on the early modern sciences. Yet, on the other hand, he also emphasized that the impact of economy and technology was restricted to a distinct part of the early modern sciences, namely the experimental or Baconian sciences. As a consequence, his *Mathematical versus Experimental Traditions* took the edge off Merton's approach. Whereas Merton had not distinguished between different traditions of science when he discussed interconnections between the early modern sciences, technology and economy, Kuhn divided the field into two clearly different traditions, stating that well into "the nineteenth century the two clusters, classical and Baconian, remained distinct" (1977, 48).

What is more, he linked this distinction with a normative judgment: only his "classical physical sciences" met his criteria of science in the proper sense. By contrast, before the nineteenth century, he argued, the Baconian sciences were "underdeveloped" and practiced by "amateurs."[9] Hence unlike the classical sciences, "research in these fields added little to man's understanding of nature during the seventeenth century" (1977, 118).

Kuhn's *Mathematical versus Experimental Traditions* revised the Merton thesis mainly by restricting its significance to those sciences that were not proper "science." Both Merton's and Hall's arguments were correct if restricted to their appropriate field of application. Whereas technology and economy had a significant impact on the emerging experimental sciences, which lacked features of the developed sciences, they did not affect astronomy, mechanics and other developed "classical sciences." Revisions of the latter during the Scientific Revolution were internal processes, well described by Hall and Koyré. The good historians of science could be relieved: their view of the Scientific Revolution was perhaps incomplete, but it was basically correct. Kuhn had made a lame duck of Merton.

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Chapter 8
Science, Criticism and the Search for Truth: Philosophical Footnotes to Kuhn's Historiography

Stefano Gattei

> *The natural result of any investigation is that the investigators either discover the object of search, or deny that it is discoverable and confess it to be inapprehensible, or persist in their search. So, too, with regard to the objects investigated by philosophy, this is probably why some have claimed to have discovered the truth, others have asserted that it cannot be apprehended, while others again go on inquiring.*
>
> *Sextus Empiricus*

The Historical Turn in Twentieth-Century Philosophy of Science

From an epistemological point of view, the twentieth century was characterized by two quite different approaches to scientific methodology. On the one hand, in the first three decades of the century, philosophers of science were chiefly concerned with logic and the philosophical analysis of language: science was regarded as paradigmatic of empirical knowledge and scientific language was correspondingly regarded as the characteristic element of any language purporting to describe the world. On the other hand, in the second half of the twentieth century the concern of philosophy of science shifted considerably, differentiating itself from that of the philosophy of language. It became increasingly involved in the dynamics of theories, in the change of scientific categories and in the great intellectual revolutions, thus seeing history of science as the acid test of rival methodologies.

This fact is extremely significant, not only from a purely philosophical point of view, but also from the wider cultural perspective. And while more than one philosopher contributed to this important shift of focus, Thomas Kuhn undoubtedly played a major role. From the historical point of view, this mere fact makes Kuhn one of the most significant philosophers of the past century, and if we think of his influence on such diverse and far-away fields, our consideration of his contribution grows further. Indeed, few philosophers (and even fewer historians) of science have influenced as many readers as Kuhn; whether one agrees or disagrees with him, no one can deny that the key notions of his philosophy ("normal science," "revolution" or "incommensurability," for instance) and some of the terms he introduced (most notably, "paradigm" and its derivatives, such as "paradigm shift") have been at the very center of the heated philosophical controversies that characterized the last decades of the past century. Kuhn's 1962 seminal work, *The Structure of Scientific Revolutions*, has become a modern classic, used (and misused) by different people in diverse contexts as the token in various ongoing disputes. Providing a common reference for cross-disciplinary discussions, it has affected debates across fields as different as historiography, sociology, politics, economics, psychology, theology, literature, feminism, cultural studies, art, education and more. Half a century after the publication of *The Structure of Scientific Revolutions*, Kuhn's shadow hangs over almost every field of intellectual inquiry.

All too often Kuhn is portrayed as the philosopher chiefly responsible for the demise of Logical Positivism. This picture, however, is mistaken from several points of view. Kuhn certainly played a major role in the "historical turn" that marked philosophy of science in the last third of the past century, thereby contributing to the radical shift of focus from logic and language analysis to a more historically informed approach, concerned with the dynamics of theory change and conceptual change. From many and often fundamental points of view, however, Kuhn did not manage to break entirely with the preceding philosophical tradition: his works are laden with principles belonging to that very empiricist philosophy he was determined to reject. Furthermore, only a partial challenge of positivism and empiricism can actually account for the genesis of Kuhn's philosophical perspective. Incommensurability, the notion of progress, the rejection of the concepts of truth and verisimilitude and the very thesis of "world change" (one of the theses deemed most radical and characteristic of Kuhn's philosophical stance) are all consequences of the empiricist elements that his philosophy retains. Appearances to the contrary notwithstanding, the implicit presuppositions and the stated principles of Kuhn's philosophy are not very different from those of the logical positivists or logical empiricists he was determined to reject.

Paradigms and Truth

Truth plays a very small role—if indeed, any—in Kuhn's seminal work. In the first edition of *The Structure of Scientific Revolutions* Kuhn hardly referred to the concept of truth: he had no need for it, not even in order to characterize and explain progress:

> The developmental process described in this essay has been a process of evolution from primitive beginnings – a process whose successive stages are characterized by an increasingly detailed and refined understanding of nature. But nothing that has been or will be said makes it a process of evolution toward anything. (Kuhn 1962, 170–171)[1]

In the 1969 "Postscript" to the second edition of the book he introduced two arguments against the notion of truth implicit in the traditional view of progress as increasing verisimilitude. To quote Kuhn's own words at some length:

> A scientific theory is usually felt to be better than its predecessors not only in the sense that it is a better instrument for discovering and solving puzzles, but also because it is somehow a better representation of what nature is really like. One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. Apparently generalizations like that refer not to the puzzle-solutions and the concrete predictions derived from a theory but rather to its ontology, to the match, that is, between the entities with which the theory populates nature and what is "really there".
>
> Perhaps there is some other way of salvaging the notion of "truth" for application to whole theories, but this one will not do. There is, I think, no theory-independent way to reconstruct phrases like "really there"; the notion of a match between the ontology of a theory and its "real" counterpart in nature now seems to me illusive in principle. Besides, as a historian, I am impressed by the implausibility of the view. I do not doubt, for example, that Newton's mechanics improves on Aristotle's and that Einstein's improves on Newton's as instruments for puzzle-solving. But I can see in their succession no coherent direction of ontological development. On the contrary, in some important respects, though by no means in all, Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's. (Kuhn 1970b, 206–207; see also 1970a, 205–207)

[1] He then urges us to give up the concept itself in order to get rid of some of the problems which have afflicted the history of Western thought: "We are all deeply accustomed to seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance. But need there be any such goal? Can we not account for both science's existence and its success in terms of evolution from the community's state of knowledge at any given time? Does it really help to imagine that there is some one full, objective true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal? If we can learn to substitute evolution-from-what-we-know for evolution-toward-what-we-wish-to-know, a number of vexing problems may vanish in the process. Somewhere in this maze, for example, must lie the problem of induction" Kuhn (1962, 171).

Kuhn's arguments against a progressive approach to the truth are therefore of two kinds: an epistemological argument and a historical one. However, the latter seems to be contradicting the former: for if the notion of truth is inconsistent, how can history tell us that successive theories do not succeed in more closely approaching the truth? And furthermore: how does Kuhn explain the affinity between Einstein's and Aristotle's theories, given the incommensurability that separates them?

But the first argument is unclear, too. Hoyningen-Huene interpreted it in the following way:

> The […] argument is epistemological; it proceeds from the assumption that it's essentially meaningless to talk of what there really is, beyond (or outside) of all theory. If this insight is correct, it's impossible to see how talk of a "match" between theories and absolute, or theory-free, purely object-sided reality could have any discernable meaning. How could the (qualitative) assertion of a match, or the (comparative) assertion of a better match, be assessed? The two pieces asserted to match each other more or less would have to be accessible independently of one another, where one of the pieces is absolute reality. But if we had access to absolute reality […] what interest would we have in theories about it? (Hoyningen-Huene 1989, 263–264)[2]

But if this is the sense in which the above quoted passage from Kuhn's "Postscript–1969" is to be understood, then his argument is quite a weak one. Why does the fact that we know that there is a correspondence between a theory and reality require independent access to each of them? Take, for example, as Alexander Bird suggests, the correspondence between a key and a lock: I know there is a correspondence between the thread form of the key and the gears of the lock not because I have independent access to those gears, but because I know that that key opens that lock. Secondly, what Hoyningen-Huene called "insight" is clearly false. For:

> […] we have an intuitive notion of the possibility of error and of ignorance. And Kuhn must share this, since the only satisfactory explanation of the origin of anomalies is that the world is not exactly as our theories say it is. If error and ignorance can be shared

[2] Scientific progress must therefore be interpreted, according to Kuhn, not in terms of an increasing approximation to the truth, but only as an instrumental improvement of scientific knowledge: "Conceived as a set of instruments for solving technical puzzles in selected areas, science clearly gains in precision and scope with the passage of time. As an instrument, science undoubtedly does progress" Kuhn (1979, 206); see also (1962, 172–173) and (1970b, 206).

> by all of us, then there must be a way things are that is "beyond" theory. Kuhn is conflating metaphysical, semantic and epistemological questions here. Even if it were impossible to assess the assertion of a match, that would not make that assertion meaningless, unless one had some sort of verificationist view about meaning […]. (Bird 2000, 227–228)

That is, we can speak of truth, even in the absence of a criterion for truth.

From Paradigms to Lexicons

The basic idea of traditional epistemology, a correspondence theory of truth that assesses beliefs on the grounds of their ability to reflect the world, independently of the mind, cannot account for the change of the very beliefs, according to Kuhn. Therefore, it must be rejected and replaced with a weaker conception, internal to the lexicon itself.[3] For if a statement can be properly said to be true or false within the context of a given lexicon, the system of categories embedded in the lexicon cannot be, *per se*, true or false.[4] By relinquishing the correspondence theory of truth, Kuhn rejects the idea that the system of categories of a theory may reflect the world-in-itself, independently of theory. We may speak of truth only within the context of a given lexicon, that is, we may only assess the assertions stated within a given lexical context: "lexicons are not […] the sorts of things that can be true or false":[5] their logical status is that of the meaning of words in general,

[3] Kuhn (1991, 95–99; 1992, 115; and 1993, 244–245). From the early 1970s onwards, Kuhn gave up using the word "paradigm" and replaced it with "lexicon" in order to highlight the important role of linguistic aspects in his view.

[4] See Wittgenstein (1969, § 205): "If the true is what is grounded, then the ground is not *true*, nor yet false"; and "[…] why should the language-game rest on some kind of knowledge?" (§ 477; see also § 559). According to Wittgenstein, a language game presents no gaps, since together with its possible moves it also defines the space which makes those very moves possible: just as the rules of the game define which moves belong to it, so the grammar of the language circumscribes what is meaningful. Nothing meaningful can therefore remain outside its boundaries and establish itself as a mark of the incompleteness of the language game (incommensurability). A game to which new rules are added is not a richer game, but simply a new game (paradigm shift). Therefore, a language game is criterion to itself, like the sample standard meter unit preserved at The International Bureau of Weights and Measures of Sèvres, near Paris, it is not itself measurable, since it is not possible to measure what is to be the unit of measurement: its possessing a length cannot be ascertained, but it is a feature which displays itself in the way we use it when measuring (see Wittgenstein (1953, Part I, § 50)). On Wittgenstein's views of truth, see his (1953, Part I, §§ 71, 77 and 133); and (1969, §§ 105, 370, 403, 457–458 and 519).

[5] Kuhn (1993, 244). Kuhn made this concept quite explicitly already in *The Structure of Scientific Revolutions*: "there is no standard higher than the assent of the relevant community" Kuhn (1962, 94).

that is, of a convention we can only justify in a pragmatic way.[6] Truth is internal to lexicon in the sense that its use is restricted to assessing claims made within the context of the lexicon: truth claims in one lexicon are not relevant for those made in another, nor can truth be applied to a lexicon itself.

In other words, Kuhn decidedly rejected the idea that the structure that constitutes the theory might reflect the way the world is, independently of theory. The lexicon embodies a linguistic convention that marks the distance between the reality described by a theory and the theory describing it in different ways:

> Experience and description are possible only with the described and describer separated, and the lexical structure which marks the separation can do so in different ways, each resulting in a different, though never wholly different, form of life. Some ways are better suited to some purposes, some to others. But none is to be accepted as true or rejected as false; none gives privileged access to a real, as against an invented, world. The ways of being-in-the-world which a lexicon provides are not candidate for true/false. (Kuhn 1991, 104)

Lexicons are assessed on the basis of their ability to serve a particular function, not to reflect reality. To quote again Kuhn's own words:

> [W]hat replaces the one big mind-independent world about which scientists were once said to discover the truth is the variety of niches within which the practitioners of these various specialties practice their trade. Those niches, which both create and are created by the conceptual and instrumental tools with which their inhabitants practice upon them, are as solid, real, resistant to arbitrary change as the external world was once said to be. But, unlike the so-called external world, they are not independent of mind and culture, and they

[6] The similarity with Carnap is striking. According to Carnap, internal questions can be answered by referring to the logical rules of a given linguistic framework. In this case, we have genuine theoretical questions, to which the notions of "correct" or "incorrect," "true" or "false" clearly and unproblematically apply. Researchers sharing a given linguistic framework can engage in theoretically genuine disputes about such internal questions. On the contrary, external questions essentially involving a choice among different linguistic frameworks, are not genuinely rational in this sense. For, in the latter case, we are confronted with questions of a purely pragmatic or instrumental character about the adequacy or appropriateness of a given framework, designed in view of a given aim. This means, in the first place, that answers to external questions cannot be assessed by appealing to dichotomies like "correct" or "incorrect," "true" or "false", but nearly always involve problems of degrees. Secondly, such a distinction implies that answers to external questions are necessarily relative to the goals individual researchers aim at—more cautious researchers, fearing to contradict themselves, could, for example, prefer the weaker rules of intuitionist logic, while those interested in a wider applicability of physics may opt for the more binding rules of classical logic. See, for example, Carnap (1936–37, 1956).

> do not sum to a single coherent whole of which we and the practitioners of all the individual scientific specialties are inhabitants. (Kuhn 1992, 120)

The idea that lexicons (or paradigms) are not and cannot be true or false *per se* is but a variant of Logical Positivism's justificationism: it is the idea that truth is grounded on the solidarity of beliefs within a given scientific community, an immediate consequence of Kuhn's highlighting of the communitarian character of science. Positivists as well placed particular emphasis on community: they regarded communal collaboration as important for the production and justification of scientific knowledge, which they in turn regarded as important for the unity of science. It is this very emphasis that fuels Kuhn's conception of science as a social institution and his attempt to define scientific knowledge, if not truth itself, in terms of the consensus of belief that is forged among its members.[7]

Coherence Theory and Correspondence Theory

Kuhn's arguments against the correspondence theory of truth have distinguished precedents: we can find something similar in Kant and also in James. However, particularly relevant in the present context are the logical positivists, chiefly Neurath and Carnap. In a 1935 article (his very first publication) Hempel described the progressive shift, in some of the major exponents of Logical Positivism, from a correspondence theory of truth to a (restrained) coherence theory: such a shift, that goes hand in hand with some shifts in their conceptions of the nature of perceptive knowledge and observation, presents a striking anticipation of Kuhn's reflection on these issues.

In his article, Hempel briefly referred to Wittgenstein's *Tractatus Logico-Philosophicus*, "the logical and historical starting point of the Vienna Circle's researches," characterized by a correspondence theory of truth: "a statement is to be called true if the fact or state of affairs expressed by it exists; otherwise the statement is to be called false" (Hempel 1935, 10). Wittgenstein's ideas concerning truth were rather generally adopted by the members of the early Vienna Circle. The first to raise doubts, which soon developed into a vigorous opposition, was Otto Neurath. And the first to recognize the importance of Neurath's ideas was Carnap, who joined some of Neurath's theses and gave them a more precise form. Hempel offered a "crude, but typical formulation" of Neurath's main theses:

[7] Most interestingly, in his comments on the typescript of *The Structure of Scientific Revolutions*, Feyerabend spots this point and highlights its root in Wittgenstein's philosophy: "[…] advance of knowledge, so I would have thought, has nothing to do with membership in communities (Wittgenstein notwithstanding)" Feyerabend (1995, 356).

> Science is a system of statements which are of one kind. Each statement may be combined or compared with each other statement (e.g. in order to draw conclusions from the combined statements or to see if they are compatible with each other or not). But statements are never compared with a "reality", with "facts". None of those who support a cleavage between statements and reality is able to give a precise account of how a comparison between statements and facts may be accomplished—nor how we may possible ascertain the structure of facts. Therefore, the cleavage is nothing but the result of a redoubling metaphysics, and all the problems connected with it are mere pseudoproblems. (Hempel 1935, 10–11)

As we can see, Neurath's doubts about the possibility of a correspondence between facts and propositions—a central theme of Wittgenstein's *Tractatus Logico-Philosophicus*—and access to reality, are the very same as Kuhn's, as read and understood by Hoyningen-Huene. Neurath's ideas involve a coherence theory of truth. As Hempel explained:

> Carnap developed, at first, a certain form of a suitable coherence theory, the basic idea of which may be elucidated by the following reflection: If it is possible to cut off the relation of sentences to 'facts' from Wittgenstein's theory and to characterize a certain class of statements as true atomic statements, one might perhaps maintain Wittgenstein's important ideas concerning statements and their connections without further depending upon the fatal confrontation of statements and facts – and upon all the embarrassing consequences connected with it. (Hempel 1935, 11)

Hempel took this to be the first step in the logical positivists' progressive abandonment of Wittgenstein's theory of truth towards that of Carnap and Neurath: by replacing the concept of atomic facts with that of protocol statements, the problematic correspondence with "external reality" is substituted by a comparison with the basic elements of experience.

The second step involved a change of view concerning the formal structure of the system of scientific statements. It consisted in loosening the verificationist conception of meaning typical of Wittgenstein's thought: in so doing universal statements, such as scientific hypotheses, can be regarded as meaningful even if they do not receive a logically conclusive verification by singular statements. Furthermore, Hempel remarked, also several propositions that appear to be singular in form possess a logical, hypothetical form. The singular statements we adopt depend upon which formal system we choose. Thus, also a second fundamental principle of the *Tractatus Logico-Philosophicus* must be abandoned; it is no longer possible to define the truth or falsehood of certain basic statements, whether or not they may be atomic statements or protocol statements, or other kinds of singular statements. "So," Hempel wrote, "the refined analysis of the formal structure of the systems of statements involves an essential loosening or softening of the concept of truth; […] In science a statement is adopted as true if it is sufficiently supported by protocol statements" (Hempel 1935, 13).

However, the principle of reducing the test of each statement to a certain kind of comparison between the statement in question and a certain class of basic statements which are allegedly deemed to be ultimate and admit no doubt, is still a leftover from Wittgenstein's view. The third and last phase of the step-by-step evolution from a correspondence theory into a restrained coherence theory of truth may be characterized, in Hempel's outline, as the process of eliminating even this characteristic. The idea was then to regard protocol statements not as absolutely reliable, but as akin to the other scientific statements for what concerns their revisability. Though we do appeal to protocol statements when a theory needs to be tested, protocol statements themselves can no longer be conceived as constituting an unalterable basis for the whole system of scientific statements. The chain of testing steps has no absolute last link, it depends upon our decision as to when to break off the testing process. Science is not a pyramid rising on a solid basis—rather, Neurath presented us with an image of science as a boat that must be constantly repaired at sea: there is no dry dock that allows for restoring it from the keel up.

Carnap and Neurath were no idealists, though: by no means did they intend to say that there are no facts, only propositions. What they actually meant to say, Hempel explained, is that each non-metaphysical consideration of philosophy belongs to the domain of the logic of science, unless it concerns an empirical question, and therefore is proper to empirical science. And it is possible to formulate each statement of the logic of science as an assertion concerning certain properties and relations to scientific propositions only. So the concept of truth may be characterized "as a sufficient agreement between the system of acknowledged protocol statements and the logical consequences which may be deduced from the statement and other statements which are already adopted" (Hempel 1935, 15).

Hempel's outline of the development of the logical positivists' coherence theory of truth leads to a position very close to Kuhn's own. Not only did Kuhn's philosophy statements describing their observations play the same role as protocol statements in the positivists' philosophy of science, as portrayed by Hempel, but the third step in the progressive dismissal of the early Wittgenstein's ideas, rejecting the foundational reliability of protocol statements, goes hand in hand with Kuhn's idea of the theory-ladenness of observations. However different their starting points may be, the resulting picture is nearly identical: although observation is the basis for scientific beliefs, not even it is free from revision in the light of theoretical change.

Once again, it is clear how Kuhn was not the anti-positivist thinker he is generally taken to be. Quite the contrary: the best way to understand his thought seems to be that of framing it within the tradition it in fact belongs to, that is, the Logical Positivism or Empiricism of Neurath and Carnap. Just like them, he rejected the characteristic assumptions of a certain kind of positivism, typical of the followers of Wittgenstein's early philosophy, such as Moritz Schlick. Schlick's reply to Carnap's and Neurath's progressive shift away from Wittgenstein was that their positions lead to relativism about truth: for, to the coherence theory of truth it may be objected that there might be several different and incompatible systems presenting a satisfactory internal coherence. A rejoinder may be to accept it and therefore make truth relative to the various coherent systems. This was Kuhn's move: if we regard the beliefs shared within the tradition of normal science as one of these coherent systems, then the relativized "truth" of Carnap and Neurath's coherence theory ends up coinciding with the idea of "truth" as relative to the various paradigms. And the coincidence becomes even more striking if we consider the close resemblance between Carnap's formal linguistic frameworks and Kuhn's lexicons, or structured vocabularies.

In "Truth and Confirmation" (1936) Carnap underlines that he prefers to speak of the confrontation between propositions and facts, rather than their comparison:

> There has been a good deal of dispute as to whether in the procedure of scientific testing *statements must be compared with facts* or as whether such comparison be unnecessary, if not impossible. If 'comparison of statement with fact' means the procedure which we called the first operation [that is, the confrontation of a statement with observation] then it must be admitted that this procedure is not only possible, but even indispensable for scientific testing. Yet it must be remarked that the formulation 'comparison' is not quite appropriate here. Two objects can be compared in regard to a property which may characterize them in various ways […]. We therefore prefer to speak of 'confrontation' rather than 'comparison'. Confrontation is understood to consist in finding out as to whether one object (the statement in this case) properly fits the other (the fact); i.e. as to whether the fact is such as it is described in the statement, or, to express it differently, as to whether the statement is true to fact. (Carnap 1936, 125)

"Furthermore," Carnap continues, "the formulation in terms of 'comparison,' in speaking of 'facts' or 'realities,' easily tempts one into the absolutistic view according to which we are said to search for an absolute reality whose nature is assumed as fixed independently of the language chosen for its description. The answer to a question concerning reality however depends not only upon that 'reality,' or upon the facts but also upon the structure (and the set of concepts) of the language used for that description" (Carnap 1936, 125–126).

A particularly telling parallel between Kuhn and the logical positivists becomes evident from the conclusion of the above-mentioned 1935 article by Hempel:

> [W]hat characteristics are there according to Carnap and Neurath's views, by which to distinguish the true protocol statements of our science from the false ones of a fairy tale? As Carnap and Neurath emphasize, there is indeed no formal, no logical difference between the two compared systems, but there is an *empirical* one. The system of protocol statements, which we call true and to which we refer in everyday life and science, may only be characterized by the historical fact that it is the system which is actually adopted by mankind, and especially by the scientists of our culture circle; and the 'true' statements in general may be characterized as those which are sufficiently supported by that system of actually adopted protocol statements. (Hempel 1935, 17–18)

But "How do we learn to produce 'true' protocol statements?" asked Hempel.

> Obviously by being conditioned. Just as we accustom a child to spit out cherry-stones by giving it a good example or by grasping its mouth, we condition it also to produce, under certain circumstances, definite spoken or written utterances (e.g. to say, 'I am hungry' or 'This is a red ball'). And we may say that young scientists are conditioned in the same way if they are taught in their university courses to produce, under certain conditions, such utterances as 'The pointer is now coinciding with scale-mark number 5' or 'This word is Old-High-German' or 'This historical document dates from the 17th century'. Perhaps the fact of the general and rather congruous conditioning of scientists may explain to a certain degree the fact of a unique system of science. (Hempel 1935, 18–19)

The logical positivists' departure from the correspondence theory of truth was grounded on the very same concerns that are at the basis of Kuhn's perplexities about the problematic correspondence of a theory with reality. Two decades after the "Postscript—1969" to the second edition of *The Structure of Scientific Revolutions* Kuhn wrote:

> [W]hat is fundamentally at stake is rather the correspondence theory of truth, the notion that the goal, when evaluating scientific laws or theories, is to determine whether or not they correspond to an external, mind-independent world. It is that notion, whether in an absolute or probabilistic form, that I'm persuaded must vanish together with foundationalism. What replaces it will still require a strong conception of truth, but not, except in the most trivial sense, correspondence truth. (Kuhn 1991, 95)

And he continued: "[W]e must learn to get along without anything at all like a correspondence theory of truth. But something like a redundancy theory of truth is badly needed to replace it" (Kuhn 1991, 99). Both for Kuhn and the logical positivists, the rejection of the correspondence theory goes hand in hand with their respective anti-realism.

Finally, it must be noted that Carnap subsequently abandoned coherence theory—both, presumably, for the inconveniences involved in that approach, and for the appeal of Tarski's correspondence theory of truth, developed in the early 1930s. The fact that Kuhn remained attached to that approach testifies that the roots of his reflection might plunge deep in the early phase of the neo-positivistic movement, rejecting one of its most radical developments.

Consolations for the Specialists

Kuhn's position is rooted both in justificationism and in a particular way of posing problems that William Bartley appropriately described as "The Wittgensteinian Problematic."[8] Taken together, these two closely interwoven aspects work together and reinforce one another, forcing the compartmentalization of knowledge and the limitation of rationality. One single problem lies at the roots of both of them: the problem of induction. For their development hinges on the assumption that the problem of induction has not been and cannot be resolved.[9] However, if we suppose it is possible to solve it and inquire what the consequences of its solution are, both from the methodological and the philosophical point of view, it will be possible to see things from an entirely different perspective.

From David Hume onwards, it has been asserted that there are two kinds of inference: deductive inference, which defines logic; and inductive inference, which defines the natural sciences: "Instead of being a faulty sort of deduction, induction is fundamental, defining science—just as deduction is fundamental,

[8] See Bartley (1990, chaps. 14–15).

[9] By no accident in the closing pages of *The Structure of Scientific Revolutions* Kuhn speaks of "dissolution" rather than "solution" of the problem of induction: see above footnote 1.

defining logic" (Bartley 1990, 219). Induction and deduction apply to different fields and must not be confused. In Hume's view the problem of induction is simply dissolved once we learn not to apply the standards of deductive logic to judge inductive inference; once we realize that the two principles cannot be unified, the task of the philosopher is simply that of describing and clarifying the standards of deductive and inductive reasoning. Most logical positivists, while maintaining the unity of the sciences, accepted this "methodological" division. Wittgenstein extended this approach: each discipline, or field, or "language game," or "form of life" is alleged to have its own standards, or principles, or "logic," which need not conform to or be reducible to any other standards or (external) principle and which, again, is the special task of the philosopher to describe and clarify, not in the least to judge, defend or criticize. There is no arguing or judging among disciplines: criticism, evaluation and explanation would no longer be proper philosophical aims. Knowledge is essentially divided, and description is all that remains to the philosopher. All he can do is describe the logics, grammars or first principles of the various kinds of discourse and the many sorts of language games and forms of life in which they are embedded. Philosophical critique is no longer of content, but of criteria application. As Paul Feyerabend put it, all that is left are "consolations for the specialists" (Feyerabend 1970).[10]

Kuhn's relativism gives rise to a sort of conservative defense of whatever belief system is construed as rational according to the established scientific community. Although revolutionary science is acknowledged, a critical attitude is systematically discouraged: instead, normal science is regarded as the essence of the scientific enterprise, and dogmatic commitment to a paradigm (or a lexicon) is upheld as a necessary prerequisite for rational knowledge and social harmony. What is worse, Kuhn's philosophy allows for and even invites the parochial policies of making outsiders of those who criticize the insiders too sharply, and of rejecting alternative theories as meaningless instead of critically engaging with them.

[10]From Lakatos' point of view, Wittgenstein is an intellectual defender of the *status quo*, and his followers set themselves the task to discourage every incursion from outside and attempt to overthrow from inside a "linguistic game" or "form of life": see Lakatos (1976). For Wittgenstein philosophy has no cognitive function—rather, it has a "therapeutic" function (see his 1953, Part I, §§ 109, 133 and 255). The descriptive task which characterizes philosophy concerns the rules governing the use of our language, that is, the grammar of the terms that constitute it: "description" refers to the description of language games, and it aims at showing the rules of those games and hence the structures which characterize them. Concerning rules, and not facts, description has an exemplary value.

Kuhn's Unfinished Historiographical Revolution

In the sixteenth century, Copernicus triggered a revolution the conclusion of which he would have been unable to recognize. But the Copernican revolution (the Scientific Revolution *par excellence*) came to an end only with Newton, well over one hundred years after Copernicus' death. The revolution against Logical Empiricism and Logical Positivism was not only well under way at the beginning of the 1980s, but started half a century earlier, even before Kuhn wrote *The Structure of Scientific Revolutions*—and yet Kuhn rejected it.[11] Furthermore, the Positivism Kuhn thought he was rejecting embraced rather more than these two claims: he was wrong to think that rejecting these two claims would amount to a root-and-branch rejection of Positivism (and, more generally, empiricism). It is certainly to Kuhn's (albeit, and quite significantly, not exclusively to his) merit that philosophy has repudiated some centuries-old tenets and has been able to reconcile itself with the lessons from the history of science. But, in fact, Kuhn's revolution is unfinished, for too many aspects of his thought contain a significant residue of that very Positivism he thought he was distancing himself from. Just like Copernicus who, while dealing the first fatal blow to the Aristotelian-Ptolemaic worldview, was also irrevocably steeped in that very same way of thinking, so Kuhn can be regarded the last exponent of the philosophical tradition he was determined to reject. He inaugurated the historical revolution in the philosophy of science—a revolution whose scope and significance goes much beyond what Kuhn himself was able to foresee.

Kuhn's contribution to the philosophy of science grows from his attempt to do *history of science from a theoretical point of view*. In so doing, he triggered a revolution. He said that revolutions are often started by outsiders, and his own career—that of "a physicist who became a historian for philosophical purposes" (Kuhn et al. 1997)—represents a particularly interesting case. However, as Kuhn himself stressed, revolutions are not often total revisions of the system of beliefs from which they originate. Again, Kuhn's case is an exemplary one: the revolution he triggered retained many aspects of the logical empiricist tradition against which he wished to react. In order to find a viable response to the crisis of foun-

[11] Lacking a proper philosophical training, he was not aware of the historical and dialectical provenance of the ideas he was dealing and working with. As Alexander Bird observed, "He was able to identify certain ideas as being characteristic of positivism or empiricism, such as the thesis that observation and perception are pre-theoretical. These he attacked and thereby helped to undermine positivism. But at the same time he was unaware that other (related) theses, which he happily adopted, were also central to positivism, such as the theoretical-context account of the meaning of theoretical terms, or the conviction that truth-as-correspondence is inaccessible. It is the partial rejection and partial retention of positivism that causes Kuhn to expound apparently radical theses such as the thesis of incommensurability" Bird (2002, 445).

dationalism of the twentieth century, we have to acknowledge Kuhn's results, realize the failure of his approach and move on, away from him.[12]

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[12] The argument here outlined is further developed in Gattei (2008, especially chapter 5).

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Chapter 9
Two Encounters
Fynn Ole Engler and Jürgen Renn

The Split of Rationality

By the middle of the twentieth century, the relation between philosophy and history of science may be characterized as amounting to a split of rationality. Whereas philosophy of science was dominated by a focus on the analysis of language and methodology, taking them as embodiments of an ahistorical scientific rationality, history of science paid attention to ideas, events and their more or less contingent circumstances without critically examining this normative rationality, let alone substituting it with its own form of historical rationality. Thomas Kuhn, with the publication of *The Structure of Scientific Revolutions* in 1962, is often seen as having closed this divide, in one sense or another, by integrating the different perspectives on science, the normative and the historical one, into one unifying framework.

By the mid-1930s, philosophers of science such as Rudolf Carnap, but also Moritz Schlick under the influence of Ludwig Wittgenstein, had retreated toward the logical analysis of language. The split of rationality represented by this retreat becomes particularly evident in the episode recounted in the following, brief written encounter between Schlick and the Vienna Circle's first important critic, the Polish bacteriologist, doctor and historian of science Ludwik Fleck.

From a modern perspective, this encounter strikingly anticipates the far-reaching conflicts between different perspectives on science, as they would come to determine the discourse throughout the following decades up until the present day. And yet, at the same time, what becomes evident in this encounter is the willingness to continue the dialogue and, in the situation of a political crisis rapidly coming to a head, the eagerness not to abandon the common battle for scientific rationality, even in the face of widely diverging criteria for science.

A Letter from Fleck

In March of 1934, shortly after the end of the winter semester at the University of Vienna, Schlick finally found the time to answer a lengthy letter from Fleck. In September 1933, Fleck had sent him an extensive manuscript under the title of "The Analysis of a Scientific Fact, Outline of a Comparative Epistemology," asking for an evaluation and perhaps even its submission for a prize offered by the *Soziologische Gesellschaft* in Vienna. Evidently Fleck, who had no connection to the German scientific community outside of his own area of expertise, as he himself states in this letter, also had hopes that the head of the *Vienna Circle* might assist him in the publication of the manuscript. It seems that Fleck considered Schlick to be especially open-minded among the German philosophers, even though the latter must have considered Fleck's approach to a "comparative epistemology" based on data from the histories of medicine and biology to be rather odd.

At the same time, however, Fleck sought to establish a dialogue with Schlick. He raised questions concerning the long-term processes of the transformation of knowledge, the connection between the established inventory of knowledge and the individual epistemic act, and concerning the dependence of the cultural evolution of knowledge on the social structures of thought collectives and complex systems of knowledge. Fleck was also not reticent about expressing his skepticism toward traditional epistemology, which was centered around the individual relation between subject and object, represented by the *Vienna Circle* and especially Schlick: "I could never shake the impression," Fleck wrote to Schlick, "that epistemology examines not knowledge as it actually occurs, but its own imagined ideal of knowledge, which lacks all its real properties."[1] And he continued his criticism:

> Already the choice of data, being almost exclusively physics, astronomy or chemistry, seems to me to be mostly misleading, since the origin of elementary insights into physics dates back so far that we can only investigate it under great difficulties—and the more recent insights are to such a degree, as it were, 'systematically biased', so greatly suggested to all of us through our educational background and scientific tradition, that I must find them inappropriate as well as a principal target for investigation. The statement that all knowledge originates in sensations is misleading—because the plurality

[1] "Ich konnte mich nie des Eindruckes erwehren, in der Erkenntnistheorie werde zumeist nicht die Erkenntnis, wie sie faktisch sich darbietet, untersucht, sondern ihr imaginiertes Idealbild, das der realen Eigenschaften entbehrt." Ludwik Fleck to Moritz Schlick, 5 September 1933, Moritz Schlick estate, Noord-Holland Archief, inv. no. 100/Fleck-1.

> of all human knowledge stems quite simply from textbooks. […] Finally, the historical development of knowledge shows some remarkable common aspects as well, such as for instance the particular stylistic closeness of the respective systems of knowledge, which demands an epistemological investigation.
>
> These considerations prompted me to treat a scientific fact from my area of expertise epistemologically, whereupon the aforementioned manuscript emerged.[2]

The Challenging Manuscript

In his manuscript, Fleck intended to give an introduction to the theory of thought styles and thought collectives. Fundamentally, he assumed that the development of knowledge was socially determined. This social determination was substantiated in the way a community processed and judged their perceptions, both conceptually and factually. By no means, however, was this meant to dismiss rationality! Rather, it was Fleck's intention to explore to what degree historically examined phenomena were amenable to a rational appraisal, despite their socio-cultural circumstances.

Against this background, Fleck had dealt with the origins and the long-term development of medical knowledge in his historically comparative study, focusing on a specific example. On the basis of the relationship between the concept of syphilis, which dates back to the end of the fifteenth century, and the so-called Wassermann reaction, which (through the cooperative work of a thought collective) for the first time in history gave an operationalizable identification of syphilitic blood, Fleck was thus able to demonstrate the dependence of one of the most well-established medical facts of his time on several social, psychological, and cultural factors. Thus it seemed that the origin and development of medical knowledge displayed a number of characteristics left unaccounted for by the epis-

[2] "Schon die Wahl des Materials fast ausschliesslich Physik, Astronomie oder Chemie scheint mir meist irreführend zu sein, denn das Entstehen der elementaren Erkenntnisse der Physik liegt so weit zurück, dass wir es nur schwer untersuchen können – und die neuern Erkenntnisse sind so sehr sozusagen »systembefangen«, so sehr durch die schulmässige Vorbildung und die wissenschaftliche Tradition uns allen suggeriert worden, dass ich sie als prinzipielles Untersuchungsmaterial ebenfalls für ungeeignet halten muss. Der Satz, alle Erkenntnis entspringe den Sinneseindrücken, ist irreführend, – denn die Mehrzahl der Kenntnisse aller Menschen stammt einfach aus den Lehrbüchern. […] Endlich finden sich auch in der historischen Entwicklung des Wissens einige merkwürdige allgemeine Erscheinungen, wie z.B. die besondere stilmässige Geschlossenheit jeweiliger Wissenssysteme, die eine erkenntnistheoretische Untersuchung fordern. Diese Betrachtungen veranlassten mich, eine wissenschaftliche Tatsache aus meinem Fachgebiet erkenntnistheoretisch zu bearbeiten, worauf das erwähnte Manuskript entstand." Ludwik Fleck to Moritz Schlick, 5 September 1933.

temology and philosophy of science of the *Vienna Circle* around Schlick, which focused on the analysis of language and methodology.

For the emergence and constitution of the fact in question, the ideas that informed the understanding of syphilis over centuries were as equally important as the socially transmitted familiarity with the material under investigation. The idea of syphilis, widespread through different social strata, as the "carnal scourge" and the "foul syphilitic blood," was indeed deeply ingrained in the collective memory. And the familiarity with the material under investigation was only achievable for the scientific practitioner after long years in the bacteriological laboratory as a member of a community steeped in tradition. Thus the cooperative nature of human knowledge was obvious to Fleck, as it presented itself particularly in the collectively arranged dissemination of theoretical and practical resources over generations of scientists, as well as in the transformations of knowledge during the cultural evolution of structured thought communities. This point of view was, first and foremost, the result of observations and reflections Fleck was able to make in his capacity as physician and head of the laboratory in the medical business of his hometown Lwów since the 1920s. He writes:

> Experience gained over several years of working in the venereal disease section of a large city hospital convinced me that it would never occur even to a modern research worker, equipped with a complete intellectual and material armory, to isolate all these multifarious aspects and sequelae of the disease form the totality of the cases he deals with or to segregate them from complications and lump them together. Only through organized cooperative research, supported by popular knowledge and continuing over several generations, might a unified picture emerge, for the development of the disease phenomena requires decades. Here, however, training, technical resources and the very nature of collaboration would repeatedly lead research workers back to the historical development of knowledge, since the bonds of history can never be cut.[3]

[3] "Infolge mehrjähriger Erfahrung in einer großstädtischen, venerischen Spitalsabteilung bin ich überzeugt, es könne auch ein mit allem Denk- und Sachrüstzeug bewaffneter, moderner Forscher nie darauf kommen, alle diese mannigfaltigen Krankheitsbilder und Krankheitsfolgen aus der Gesamtheit der vorkommenden Fälle auszuscheiden, abzusondern von Komplikationen und zu einer Einheit zu verbinden. Erst organisierte Forschungsgemeinschaft, unterstützt vom Volkswillen, und über einige Generationen dauernd, vermöchte das Ziel erreichen – schon deshalb, weil die Entwicklung der Krankheitsphänomene Jahrzehnte braucht. In diesem Falle aber würden Vorbildung, technische Mittel und die Art der Zusammenarbeit die Forscher immer wieder auf den alten Pfad der geschichtlichen Erkenntnisentwicklung leiten. Also ist Auflösung historischer Bindung keinesfalls möglich" Fleck (1979, 22).

The Missed Opportunity

Fleck's manuscript, written in a rich and elegant German, was the basis for his groundbreaking book entitled *Genesis and Development of a Scientific Fact*. Schlick had read this manuscript with great interest. In his answer to Fleck in March of 1934, he recognized it as "a first-rate scientific accomplishment."[4] Nevertheless, he could not agree with Fleck's views on the epistemology and philosophy of science. Moreover, without professional support from a medical authority well versed in the history of medicine, he did not feel that he was in a position to recommend the book to a publisher.

It was certainly not simply the case that the realm of bacteriology and serology was unfamiliar to Schlick, but the historicization of scientific knowledge, as it was inherent in Fleck's socio-cultural perspective, must have seemed to be a threat undermining the *Vienna Circle*'s view of the role of science as a model for rationality. Thus Schlick's normative rationality of methodologies and theories, based on the analysis of language, stood in opposition to Fleck's historical rationality of concrete thought collectives. And yet Schlick passed the manuscript on to *Springer-Verlag*, a publisher he had close ties to, before leaving for a longer sojourn on the Amalfi Coast at the end of March 1934.

We may assume that Schlick planned to have the manuscript printed in the series *Schriften zur wissenschaftlichen Weltauffassung*, which he published himself together with the physicist Philipp Frank, although nothing further is known about this decision. We are not in possession of the letter from Schlick to *Springer-Verlag*, which makes it impossible to ascertain conclusively if Schlick had planned to publish the book in his own series or whether he had other plans for it. Nor is it known whether the medical authority Schlick had called for had been consulted during the decision, or if indeed anyone else had been asked. In his letter to Fleck, Schlick had mentioned the sociologist and economist, Franz Oppenheimer as a possible consultant, who incidentally had practiced medicine for years in Berlin. He was also an acquaintance of Albert Einstein's, and was then working as a guest lecturer in Palestine.

In the end, *Springer-Verlag* decided not to publish the book, presumably for "external" reasons, that is, when viewed alongside the previous publications in the series, the publisher likely missed the austerity of form and the stringency of argumentation in Fleck's work. Instead, they recommended publishing the text in an abridged form in a journal, which might have been provoked by Fleck's rather literary style and his way of presentation which was, superficially speaking, almost like a collage—all of which was rather unusual for current "scientific

[4] "eine wissenschaftliche Leistung hohen Ranges." Moritz Schlick to Ludwik Fleck, 16 March 1934, Moritz Schlick estate, Noord-Holland Archief, inv. no. 100/Fleck-2.

series." In a letter from Otto Lange, founder and director of *Springer-Verlag* (Vienna), to Schlick, he comments:

> I have in the meantime had a look at the work by Dr. *Fleck*, analysis of a scientific fact, which you were kind enough to relay to me. It does not seem to me to be suitable for publication in book form. I would advise the author to perhaps publish it in a journal in abridged form.[5]

Even though Fleck's book was not published by *Springer* – it was printed in 1935 by the *Verlag Benno Schwabe & Co* in Basel, with a famously rather ineffective reception – Schlick, too, sought dialogue with Fleck. Clearly, Schlick and Fleck admired each other despite the differences of their views. Schlick, for instance, praised "the richness of ideas, the scholarship, the sagacity" of Fleck's arguments, "and the high intellectual standards of the whole thing." Fleck, on the other hand, likely viewed the author of the *General Theory of Knowledge*[6] as a partner for his historically comparative studies into scientific rationality.

However, both Schlick and Fleck also recognized the challenges that were inherent in the considerations of the other. At the same time, they must have also been painfully aware that the space available for a discussion was, due to the political circumstances, becoming smaller and smaller. In the end, due to external circumstances, Schlick and Fleck never got the chance to enter into a dialogue. In light of the existing divide between the philosophy of science and cultural studies, we may see this as a *missed opportunity*.

A Second Encounter

But from our point of view, this is not the end of the story. Is it, at least in principle, possible, from Schlick's perspective, to take on the questions that Fleck raised in his letter, *or* was this, after all, an encounter between mutually incompatible worlds? And what is the relation between Schlick's attempt to defend science's claim to objective knowledge and Fleck's emphasis on the socio-cultural and cultural-historical contexts of the long-term development of knowledge? Can both views be reconciled: one based on the reflective use of reason, intended to secure science's claim to validity, and the realization that science is through and

[5]"Die Arbeit Herrn Dr. *Flecks*, Analyse einer wissenschaftlichen Tatsache, die Sie mir freundlichst übermittelt haben, habe ich mir inzwischen angesehen. Sie scheint mir für die Ausgabe als Buch nicht in Betracht zu kommen. Ich würde dem Autor empfehlen, sie vielleicht in gekürzter Form in einer Zeitschrift zu veröffentlichen." Otto Lange (*Springer-Verlag*, Vienna) to Moritz Schlick, 14 April 1934.

[6]Reprinted in a second edition in 1925.

through part of our imperfect and ever-changing *Lebenswelt*? These questions are not merely relevant for a historiography of science (a counterfactual one at that!), but may even take on a certain urgency in view of the role of science for society that has grown in both importance and global extent ever since the exchange between Schlick and Fleck.

The resilience of these questions becomes evident from a second, almost symbolic encounter between an analytic and a historical perspective on science, between Rudolf Carnap and Thomas Kuhn. In contrast to Carnap's purely logico-linguistic considerations of science, Kuhn, in *Structure,* stresses its socio-cognitive and historical dimensions, and within this context specifically deals with the dynamics of theory changes. At first glance, this seems to connect him to Fleck's investigations of structured thought collectives and the transformations of knowledge systems. But, on closer inspection, Kuhn's perspective on the social dimension of science is narrower than that of Fleck. In fact, he primarily focuses on the "esoteric circles," constituted by scientific communities of highly specialized experts.

This perspective aligns him with Carnap and his view of linguistic frameworks as critical tools of science. They evidently shared an underlying conception of science as a world of its own, characterized primarily by struggles within the scientific community about the most appropriate scientific theory. This narrower focus on science and its practitioners perhaps also represents one of the reasons for Kuhn's ambivalent reaction to Fleck's book:

> I don't think I learned much from reading that book, I might have learned more if the Polish German hadn't been so very difficult. But I certainly got a lot of important reinforcement. There was somebody who was, in a number of respects, thinking about things the way I was, thinking about the historical material the way I was. I never felt at all comfortable and still don't with [Fleck's] »thought collective.« (Kuhn 2000, 283)

Kuhn and Carnap's agreement on what one might call the instrumental rationality of science becomes perhaps nowhere as obvious in a letter Carnap sent to Kuhn shortly after the completion of *Structure*, which was the last book to appear in the famous Vienna Circle series *International Encyclopedia of Unified Science*, created by Otto Neurath, Charles Morris and Carnap:

> Simultaneously I am returning your manuscript »The Structure of Scientific Revolution«. […] I am convinced that your ideas will be very stimulating for all those who are interested in the nature of scientific theories and especially the causes and forms of their changes.

> I found very illuminating the parallel you draw with Darwinian evolution: just as Darwin gave up the earlier idea that the evolution was directed towards a predetermined goal, men as the perfect organism, and saw it as a process of improvement by natural selection, you emphasize that the development of theories is not directed toward the perfect true theory, but is a process of improvement of an instrument.[7]

Looking back at this exchange 50 years after the publication of *Structure*, it is evident that this apparent reconciliation between historical and the philosophical points of view was premature, also in view of what Kuhn did not take over from Fleck. Moreover, Kuhn's image of science, to use Yehuda Elkana's term, does not take into account many dimensions of the scientific development that since have become central to historical and philosophical debates. Such as its embedding within a larger world of knowledge, its social construction, its material culture, its dependence on local contexts as well as on long-term processes, its implication in military and economic ventures, but also its role in generating values and its growing significance for human survival.

Even 50 years after the publication of *Structure*, the split of rationality has thus not been overcome. We are still confronted with the split between the view, if not the vision of science as the best model of rationality available to us, generalizable to other spheres of human activity as well, and the view of science as a deeply contingent, historically shaped human enterprise as any other, an enterprise that we can only practice, administer or describe.

Therefore, it is worthwhile to revisit the instances where this split became visible in the past, such as in the exchanges between Schlick and Fleck, or between Carnap and Kuhn. It then becomes evident that science's claims to rationality must remain speculative without consideration of the concrete socio-cultural and historical dimensions of this rationality, but also that any approach that reduces science to its purely instrumental character or that fails to take scientific rationality and its relevance to global human concerns seriously would ultimately become irrelevant and even cynical, since it would abandon, against better knowledge, our struggle for reason, and not only within science.

Acknowledgement

We would like to acknowledge Jendrik Stelling for contributing to the translation.

[7] Rudolf Carnap to Thomas Kuhn, 28 April 1962.

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Part 3: Kuhn's Legacy

Chapter 10
Thomas Kuhn
Jed Z. Buchwald

Figure 10.1: Taken by Buchwald at Tom and Jehane Kuhn's home on Memorial Drive in Cambridge in the spring of 1991.

In the fall of 1967 I entered Princeton as a Freshman intending to major in physics but interested as well in history. The catalog listed a course on the history of science, taught by a Professor Thomas Kuhn, with the assistance of Michael Mahoney and Theodore Brown, that seemed nicely to fit both interests. The course proved to be peculiarly intense for something about what was, after all, obsolete science as, each week, hundreds of pages of *arcana* from the distant past had to be absorbed. Professor Kuhn would pace back and forth in lecture, smoking intensely and talking rapidly to an elaborate outline drawn on the board at the beginning of each class. In tutorial, Mahoney (who passed away in 2009) developed Kuhn's points, forcing students to grapple with the meaning and significance of the many complicated texts that were assigned. Though the *Structure of Scientific*

Revolutions was assigned in that class, Kuhn never put much explicit emphasis on it; he lectured almost entirely about the historical materials we were reading. Everything he spoke about, from Ptolemaic eccentrics to stationary orbits in the Bohr atom, seemed to exemplify a way of thinking about science that was certainly unusual for the time. It seemed that he was continually trying to excavate a structure beneath a past science's apparent surface, something that could provide a key to understanding how it worked. He would often emphasize precisely what seemed to be the oddest, or the most irrelevant, passage or point in the reading. Furthermore, every story that he told took its shape and meaning not through explicit definition but rather through the examples that he developed, and through the ways he answered questions. Kuhn's novel view of science captured the interest of the class, though at the time its full outline remained somewhat fuzzy to many of us, a fact brought home rather strongly by his favorable but tough remarks on my essay for the course. We had the opportunity to discuss that and other issues over the next four years as I became his and Mahoney's research assistant.

During my time as Kuhn's assistant we would meet every week or two to talk about old physics. He would always emphasize the need to uncover what kinds of characteristic problems were at issue in the past, and about how these problems connected to mathematical and theoretical structures, though not much at the time about experiments proper. In the spring of 1971, Kuhn taught a graduate seminar on the history of thermodynamics. The readings—all of them primary sources—had been carefully prepared and put on reserve. Each week one of the students was responsible for taking the class through the texts. Kuhn did not want a simple summary of relevant issues. He expected you to have figured out precisely what made the text tick. He already had strong notions about the materials, and if you came up with something different from what he had in mind then you had to argue for it line by line, sometimes equation by equation (since most of the texts dealt with in that course were strongly mathematical).

Anyone who encounters Kuhn's *Structure* takes away at least the following claims: that scientists working in a given area together hold to a 'paradigm' that guides the way they think about their subject, from theory through the design, execution and interpretation of experiments, that group members use the paradigm to solve puzzles as they pursue 'normal' scientific work, that problems may eventually fracture the paradigm's coherence as 'anomalies' begin to show, usually from experiments originally undertaken in 'normal' research, but perhaps from internal problems affecting consistence, that these may lead some members of the group, or perhaps aspiring entrants, to question basic elements of the scheme, and that, often in a flash of new insight, a 'revolution' occurs that replaces the previous paradigm with a new one.

Kuhn's most detailed effort to work through a body of past physics—his *Black-body Theory and the Quantum Discontinuity*—appeared in 1978. He had been hard at work on it since 1971. To those who knew him well over the years, the book itself very nicely exemplifies Kuhn's special approach to the history of science as well as his particular views about scientific development. Like most things that he wrote, *Black-body Theory* generated controversy, some directed at its apparent failure to apply what he had himself laid out in *Structure*, some directed at his specific, technical claims. It seemed to many of us who knew him that Kuhn was not bothered much or even at all by the former critique, but he was very much concerned with technical criticisms. His need, even compulsion, to find *the*—not *a*—core of meaning that unites a disparate series of texts, to extract that largely-implicit structure and to display how it governed and connected to a set of canonical problems, powerfully directed his historical research. Technical criticism accordingly bothered him a great deal, precisely because it went to the core of what Kuhn took to be his central historical task, which was to uncover the hidden integrity of past science.

My own first book (*From Maxwell to Microphysics*, Chicago, 1985) concentrated on the structures by means of which a group of British physicists produced a purely continuum-based account of electrodynamics. The book aimed to uncover the practices of these investigators as they sought solutions to specific problems, both on paper and in the laboratory, and in that sense focused on what Kuhn termed "normal science." But, in addition, I sought to locate the points of divergence between that way of working and related areas of investigation in Germany, France and Italy. The book concluded with an account of the experimental work on magneto-optics in Germany and Holland that, I argued, produced an 'anomalistic' situation that led there to the first concerted introduction of microphysical reasoning and, in England, to abrogating the underpinnings of a continuum-based electrodynamics.

None of that dealt explicitly with Kuhn's *Structure*, but the approach taken was powerfully influenced by his way of treating past science. Much of that was learned directly from him, however, and not *pari passu* from the *Structure* itself. Which is perhaps not surprising, since on Kuhn's account it is only through exemplary situations, often learned directly in the apprentice-like training which students undergo, that one learns how to work a particular system. Neither did *From Maxwell* claim anything like a 'revolution' of the sort that, for example, might be thought to characterize the development of optics at the beginning of the nineteenth century, which was the subject of my second book (*The Rise of the Wave Theory of Light*, Chicago, 1989).

Perhaps the most important lesson that those of us who studied under Kuhn learned from him, and that does appear, if only implicitly, in the *Structure*, is that the deepest, most characteristic elements that constitute a field of scientific practice are precisely the ones that are the least obvious and that must be learned through the comparative assimilation of instantiating situations, or what Kuhn came to term 'exemplars.' *Wave Theory* sought explicitly to uncover those unspoken ways of working, and in so doing argued that what appeared on the surface to be the primary points at issue in the debates that eventuated in the theory's spread were not in fact the principal ones at all.

In the *Structure* Kuhn had cited the transition to wave optics as an example of crisis producing a revolution, that here we had "Thomas Young's first accounts of the wave theory of light [appearing] at a very early stage of a developing crisis in optics" (2012, p. 86). But was there a 'crisis' at the time, and, whether or not there was one, did it occur before the substantial evolution of a new system? In *Wave Theory* I argued, first, that there was no crisis at the time that Young evolved his novel scheme, that the issue of diffraction, which in retrospect seemed so important, had long been set to the side. But, second, that the system with which wave optics did come into direct conflict had evolved after Young's work and independently of it, upon the discovery of polarization phenomena by Etienne Louis Malus in France. And that system, as in fact optics since the time of Newton (and even before), did not depend at its basic level upon light being a stream of particles, though many did indeed think light to be something like that. Instead, the fundamental conceptual and mathematical differences between wave and non-wave optics, at the deepest level, concerned whether light consisted of individually countable, discrete entities (rays) or a surface evolving through space in time under the aegis of phase. What actually occurred was that the ray-based system evolved rapidly after Malus' discovery as new polarization phenomena were found, while at nearly the same time Augustin Fresnel developed the mathematical and experimental foundations of wave optics in ways that, for a manifold of reasons, Young had not and likely could not have done.

Here, then, we have something that is unKuhn-like in one sense—namely in not showing clear signs of anomaly and crisis among the originators of a novel scheme—but very Kuhn-like in another, for these events clearly do indicate that each system had evolved (and quite rapidly so) a striking internal coherence grounded on unstated but firmly held ways of treating problems, ways that showed themselves only through the examination of the exemplary problems that each sought to solve. Reading only prefatory words about the systems, words intended to persuade, almost never reveals the ways in which a system actually works; that can be found only by trying to understand, step by step, how practitioners went about solving problems. This is why Kuhn placed so much emphasis in *Structure* on back-of-the-chapter problems in physical science and mathematics texts, texts of a sort that first began to appear in the eighteenth century. Trained as a physicist himself, Kuhn was convinced that the only way to learn how to be successful (i.e. to be considered a proper member of the community) was to set up, articulate and solve problems in ways that the community accepted. Training and apprenticeship are consequently often, though hardly always (as, e.g., when a set of practitioners scarcely exists at all), critically important for someone fruitfully to enter an established field.

Kuhn's move to the Department of Philosophy at MIT in 1979 exemplifies his own sense that the issues with which he was most directly concerned were philosophical in nature, though he remained deeply committed to careful historical understanding, as he conceived it. In 1986 he wrote me a letter that contained the following remark: "I think of my primary talent as a hard-earned ability to read a text, find a way to make it make sense by discovering the conceptual structure that lies behind it. It's the experience of finding hidden structures that underlies *The Structure of Scientific Revolutions* and that I'm now back trying to analyze again." Those of us who studied under him, and many who knew him over the years, will recognize here his distinctive voice and point of view. Voice and view demanded and conveyed an uncompromising, rigorous attempt to push beneath the surface of technical work, to find out how it worked.

Which is why my third book (*The Creation of Scientific Effects*, Chicago, 1994) explored how Heinrich Hertz, apprenticed under Hermann von Helmholtz in late 1870s Berlin, came to create novel electrodynamic phenomena, including propagating electromagnetic waves. Hertz learned from Helmholtz a particular way to attack problems in electrodynamics, a way that was only marginally consistent with contemporary British field theory, that in fact differed from the latter at fundamental levels, including the most basic concept of electric charge. Hertz attacked problems assigned to him by Helmholtz, and so thoroughly had he absorbed the latter's way of thinking about physics interactions that he succeeded in solving a problem that Helmholtz—the very creator of the system—had initially stumbled over. And then, years later, when Hertz did succeed in generating and detecting electric waves in air, he initially thought that the type of waves he had produced conformed to Helmholtz's way of thinking and not to Maxwell's. When he eventually decided otherwise, and developed the fundamental mathematico-physical scheme for what became antenna theory years later, Hertz did not adopt British field theory, for he continued to think about electric charge in ways that the latter found inimical. Here, then, we have something that does look like an evolving Kuhnian crisis at the heart of what I termed Helmholtzian physics, one that emerged rapidly as a result of a discovery that at first seemed to be consistent with it but that even more rapidly proved anomalous. And the resolution of the crisis within several years did lead to the production of an electrodynamics based on what became a canonical set of four “Maxwell equations” soon coupled to the “Lorentz force” on electric particles.

In 1992 I became director of the Dibner Institute for the History of Science and Technology at MIT, where each week a Fellow would give a talk. Tom attended many of these, and once a month or so we would have lunch together. During these last years of his life he was trying hard to develop a lexical understanding of what it is about scientific work that produces difficulties of mutual comprehension between proponents of different systems that ostensibly cover the same phenomenal range. The problem, that is, of incommensurability. Although Kuhn had not lectured in any detail about the idea years ago in my first class with him at Princeton, the core of the notion was certainly there, if not explicitly developed, and those of us taught by him picked up by example what he had in mind.

Many of our talks in the ’90s ranged over examples of that sort of thing, taken not however from such wide-ranging schemes as Ptolemaic versus Copernican astronomy, but from much more limited structures, such as the arguments between proponents of an optics based on waves and those who thought in terms of rays. Or between British developers of electromagnetic fields and their German counterparts. Tom’s evolved understanding orbited about his conviction that the deepest differences between scientific schemes concern the ways in which they respectively divide their universes into kinds of entities. Incommensurability, he thought, was not a vague difference in views, but a specific violation by the one scheme of another’s affiliation among kinds—a violation of the principle that a given kind can be an immediate subset of at most one other. That, it seemed to him, was a general property of scientific systems which captures differences among them. This sounds rather abstract, and it is (partly because Kuhn never developed it into something tied to the roles of instrumentation), but it is nicely descriptive of what seems to be the case historically in a number of cases.

Our discussions in the early ’90s led me at his urging to write a paper explicitly applying the idea to the history of wave optics (Buchwald 1992). We corresponded and talked about the various issues as the paper took shape, and the diagram that it included resulted from our discussions. The dark lines represent the kinds of polarized and unpolarized light that were deployed by those who thought of light in terms of rays in the early 1800s, satisfying the one-immediate-ancestor criterion. The dotted lines show instead how practitioners of wave optics grouped kinds of light together in ways that violated the groupings of ray practitioners. These differences had instrumental consequences that appear quite directly in the literature of the period.

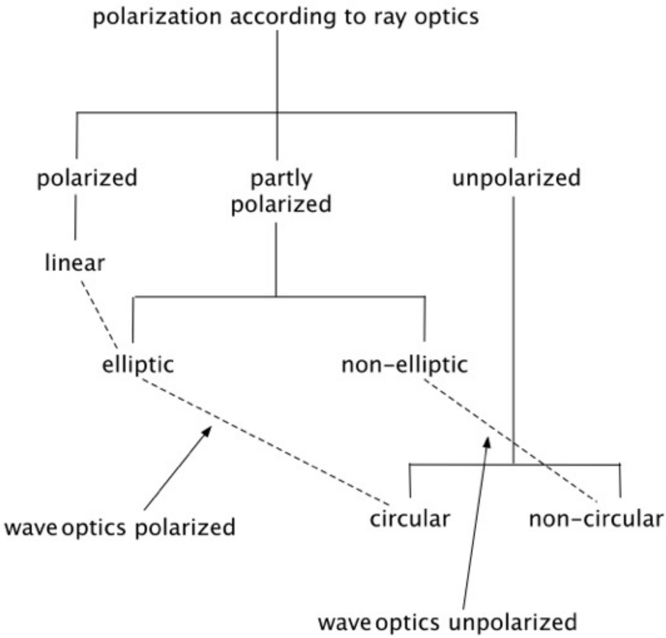

Figure 10.2: A tree of kinds for light in the early nineteenth century.

Such a system certainly does exhibit the signs of incommensurability, in that a kind term in the one scheme overlapped more than one such term in the other. Similarly, in the histories of electrodynamics that I had studied kind terms involving electric charge and fields or forces crossed disbarred boundaries when trying to apply a term from one scheme to another. And in all of these cases one could find examples in which a practitioner of one scheme, trying to argue against an alternative, or just to use an alternative's successful results, inevitably worked the alternative scheme in a way that violated the relationships among its entities. That is assuredly an indication of Kuhnian incommensurability, albeit locked down to specifics and avoiding a mushy, global sense of the term that has so often confused or even angered readers of the *Structure*—though a careful and sympathetic reader can find elements of the notion there as well. In our discussions Tom was interested for the most part in the categorical groupings, less so in their connections to measurement processes, though he did tell me that he intended to think through the latter in more detail in relation to kinds. He never found the time to do so.

Yet it seems to me that instrumentation is critical to understanding the sorting of objects or effects that this way of thinking demands. First, instruments are precisely what divides the elements of the tree from one another: sitting at the nodes or branch-points of the tree, experimental devices assign something to this or to that category. Second, devices may generate new kinds that can either be assimilated by, or that may disrupt, the existing structure. Moreover, experimental apparatus may have its own taxonomic structure that to a very large extent exists apart from that of trees with which it is in other respects associated—provided that experimental relations do not violate otherwise-accepted taxonomies, or at least that incommensurable taxonomies are not brought into contact with one another.

Devices on this account act at the nodes of the tree to assign objects to the appropriate categories. Absent the apparatus there would be no sorting, and the apparatus proper often constitutes an embodiment of the relevant kind-structure. One may very reasonably ask, therefore, whether (in)commensurability, and the doctrine of kinds discussed here, are highly limited in historical application, to, say, science after the late seventeenth century, or perhaps even to science post-1800. What, for example, do kinds have to say about the sort of astronomy practiced by Kepler, in which the apparatus can scarcely be thought of as embodying kinds in the way that, e.g., Fresnel's rhomb did in wave optics?

This is not an easy question to answer, and I am not certain that the doctrine of kinds can in fact embrace all forms of scientific behavior. It may just be that it is particularly well-adapted to some forms of apparatus-based science. If the doctrine of kinds must be linked to laboratory equipment then their history belongs also to it. I think, however, that a somewhat broader notion of apparatus may extend the utility of the doctrine beyond these boundaries.

'Apparatus' naturally suggests—and is so defined by the *Oxford English Dictionary*—material devices, machines, entities that make things happen to objects or that react to happenings. A signal characteristic of such devices is one's ability to change them in essential ways, and, in so doing, to make different things happen or to elicit different reactions to the same event. Keplerean astronomy used no such devices, because the telescope cannot work the (celestial) object that is being investigated, nor can it do more than one thing with the object's (optical) effects. Kepler, in working with the observations of Mars bequeathed to him by Tycho, might nevertheless be said to have worked with apparatus of a kind, though not apparatus that did anything to celestial objects or with their light. His 'apparatus' consisted of the rules and the mathematical methods that he was prepared to deploy in accommodating Tycho's observations. That apparatus—mathematical devices developed in antiquity—resisted application to some of the effects (the positions of light smudges on the celestial sphere) that Kepler brought it to bear on so long as those effects were also assimilated to Copernican motions. Changing the latter opened a new path, but it also generated a great deal of unresolved tension in the apparatus (antique mathematics). One might be inclined to say that this is just theory-work, rather than laboratory-work, and that writing in this context of 'apparatus' is otiose, but it seems to me that these two kinds of labour share at least one basic characteristic which links them to the doctrine of kinds: that of working on something to see what can be made to happen—either through paper 'apparatus,' or through material devices. Some scientific activity, such as astronomy or astrophysics, works only in the former way; laboratory science usually works in both ways. Learning standard problems is a kind of training in paper demonstration that is analogous to learning standard demonstration experiments; solving new paper problems bears a similar relation to performing new experiments.

From the standpoint of kinds, both forms of apparatus can act as sorters. A slice of crystal in a polarimeter does things to light that assign it to a particular category. One may know almost nothing at all about the crystal's likely behavior beforehand. Worked properly, the polarimeter produces novel information about the crystal. Theoretical devices can do something similar. Succeeding observations of the loci of a strange heavenly object can be subjected to astronomical theory, and it may as a result become possible to assign it to known categories, e.g. to comets. There is an evident difference between the two cases. The polarimeter acts on the object and sorts it. Astronomical theory acts on something other than the object, something that is itself produced by an instrument that engages an effect of the object. Whereas optical theory does not have to intervene in the polarimeter's sorting (once the device has been properly built and worked), astronomical theory itself does the sorting work.

Many historical situations exhibit both types. A slice of some transparent stuff may produce colored rings in a polarimeter, thereby assigning it to the class of ring-producing-things. But the rings may not look like ones previously seen, at which point theoretical technology, as it were, comes to bear, yielding in this case a novel class of objects in respect to their optical behavior, namely the class of biaxial crystals. This might even occur without the intervention of much theory through the construction of novel material devices that produce new sortings without violating old connections. If these material and paper attempts at sorting fail, then radical new technologies may be produced, or perhaps the effect may be relegated to the sidelines as something inconsequential. The point is that sorting 'technologies' do not have to be physical devices, and this may make it possible fruitfully to use the doctrine of kinds for pre-laboratory science.

The critical role of devices in configuring the taxonomic tree for laboratory science means that taxonomies may be distinguished from one another in two very important ways: first, as to their comparative freedom from device-induced category violations, and second, as to their robustness in respect to novel devices. This is, furthermore, not solely an abstract, philosophical point because scientists often do just that. They are continually using different types of existing apparatus to be certain they have properly understood something, and they generally try to produce new apparatus to get at a process in different ways. A taxonomy that is weak in the first respect and that is not robust in the second will almost certainly not gain adherents over time because it does not work well with or is not fruitful in producing (or both) scientific devices. To the extent that a premium is placed on building a world with apparatus, and on generating new apparatus from that world, such a taxonomy is objectively weak in comparison with one that fits well with existing devices and generates new ones. Nothing in this description requires invoking an absolute, eternal world of entities that apparatus-based science uncovers over time. It does require that, as a matter of fact, devices can be made to work and that new devices can be fabricated as scientific practice grafts, buds and restructures taxonomic trees.

I continue to think that Tom was substantially correct about the importance of incommensurability in scientific practice, and that the concept is best conceived in terms of a tree structure for kinds. Certainly his way of understanding cannot easily encompass the sort of thing that takes place when, say, someone trained as a physicist moves into biology, giving rise perhaps to new regimes with concomitant developments in social, cultural and institutional structures. Though Tom would occasionally talk about such things, he really had very little to say about them in later years since they do not map simply onto issues of incommensurability in the way that he had come to think about the latter. That notion occupied him to the end of his life and, he often told me, constituted his most important contribution to understanding the character of scientific work[1].

My fourth book, *The Zodiac of Paris* (co-authored with Diane Greco Josefowicz, Princeton, 2010) traverses rather different terrain, since here the issues range from archaeological expropriation during the Bourbon Restoration to censorship, religious revanchism, imagined pasts, and the question of who could control antiquity, calculating scientists or philological historians. Still, here too we find Kuhnian traces, since the communities in question usually talked past one another, and even among the computing scientists discord reigned as each group tried to forge its own version of antiquity by means of computations rejected by, and often not understood by, others. In a fifth book, *Newton and the Origin of*

[1] For more on Kuhn and the problems of incommensurability, include the issue raised by the continuity of evidence, see Buchwald and Smith (1998; 2002).

Civilization (co-authored with Mordechai Feingold, Princeton, 2012), we can find traces, if not of Kuhnian taxonomic incommensurability, nevertheless of the production of a novel way of treating evidence that escaped most contemporaries and that also clashed powerfully with standards grounded on traditions of textual, numismatic, and medallion-based argumentation. Here too much of what appeared in surface argumentation betrays, on deeper investigation, profound and mostly unvoiced differences concerning the very forms of persuasive argumentation.

Kuhn remains relevant today precisely because he insisted on probing beneath the surface of scientific discourse to reveal the unstated but powerfully operative practices and beliefs that characterize a group. That kind of probing analysis requires immersion in the details of often arcane computations and arguments. Few did it in the past, and few, Kuhn felt, do it today. More's the pity.

As the years went by Kuhn increasingly found historical research to be difficult. There seem to have been two reasons for his growing reluctance to read or to do history. He had trouble absorbing secondary work, in major part because he brought to histories the same intense commitment to the text's meaning that he brought to source materials. Vagueness bothered him no end, as did failure to produce the sort of analysis that he found most useful and interesting. But Kuhn was also not himself inclined to grapple with archival materials; he focused almost all of his own historical work on printed works. Yet, and he knew this to be so, the very structures that he so strongly wanted to uncover could often only be excavated from unprinted materials.

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Chapter 11
Thomas Kuhn and the Dialogue Between Historians and Philosophers of Science

William Shea

> I am, for example, acutely aware of the difficulties created by saying that when Aristotle and Galileo looked at swinging stones, the first saw constrained fall, the second a pendulum. The same difficulties are presented in an even more fundamental form by the opening sentences of this section: though the world does not change with a change of paradigm, the scientist afterwards works in a different world. Nevertheless, I am convinced that we must learn to make sense of statements that at least resemble these. (Kuhn 1970, 121)

Introduction: When Language Rebels

When Thomas Kuhn began to write *The Structure of Scientific Revolutions*, language model epistemology had just been smuggled out of departments of philosophy and linguistics and lobbed like grenades into unsuspecting departments of history of science. The traditional ties between language and reality external to language were threatened on the ground that language is the very structure of mental life and no meta-language can ever stand outside itself to observe reality external to itself. Thomas Kuhn thought the problem of translation from one language to another is mirrored in the problem of interpreting one scientific worldview in terms of a different scientific worldview. The difficulty is compounded by the fact that, whereas members of one linguistic community generally recognize that other communities may have their own, equally valid languages, the members of a given scientific tradition usually consider that theirs alone is genuinely scientific. Consider for instance, Sir Peter Medawar's scathing review of Teilhard de Chardin's *Phenomenon of Man*:

> Some reviewers hereabouts have called it the Book of the Year—one, the Book of the Century. Yet the greater part of it, I shall show, is nonsense, tricked out with a variety of tedious metaphysical conceits, and its author can be excused of dishonesty only on the grounds

> that before deceiving others he has taken great pains to deceive himself. The Phenomenon of Man cannot be read without a feeling of suffocation, a gasping and flailing around for sense. There's an argument in it, to be sure—a feeble argument, abominably expressed. (Medawar 1983, 242)

Medawar's intemperate outburst was the result of his deep conviction that there is one scientific method, that he knew what it was, and that no one should dare to suggest there could be another! Such statements, usually couched in a blander tone, were common in the heyday of logical positivism, the philosophy of science that dominated the scene from the eve of the Second World War to the early 1960s. Logical positivists recognized different languages but, like Medawar, they believed there was a clear demarcation between cognitively significant and cognitively meaningless expressions. But locating the demarcation line soon proved difficult, and historical studies revealed that, when found, it had a way of shifting regardless of the pronouncements of logically minded philosophers or philosophically aspiring scientists.

Structure dealt a blow to facile generalizations about the nature of science and ushered in a period of soul-searching that shows no sign of abating fifty years later. The first section of this paper pays tribute to the memory of Thomas Kuhn and discusses his stimulating ideas about the quirks of language; the second section examines how historians and philosophers of science have tried to interact.

The Challenge of Translation

The quest for the scientific method that underpins all scientific research has proved as elusive as the search for a universal grammar that underlies all languages. Kuhn never disavowed his belief that a scientific revolution marks a break between two incommensurable points of view, but after the publication of his work he relentlessly sought a way of moving from the perspective of one group to that of a different one. Whereas a gestalt switch was the analogy invoked in *Structure*, Kuhn came to favor a comparison with the acquisition of a foreign language by a culturally and socially sensitive anthropologist. Neither incommensurability nor untranslatability need debar us from the understanding of scientific texts if we have the required intelligence, determination (and modesty) to live with them and to learn from them. What Kuhn would not grant is that understanding implies total comprehension. The constellation of theoretical concepts, practical insights and mathematical techniques that cluster around the key notion in a given body of scientific knowledge cannot be fully evoked by even the best translation into a different system.

The case is analogous to that of poetry. A good French translation of *Intimations of Immortality* can capture most of Wordsworth's ideas. It may even recreate the atmosphere of the poem, but in order to do this it will have to forgo literal translation for literary creation. Kuhn stressed that we cannot translate an older scientific text simply by enriching the contemporary lexicon. A word alone, even a family of words, will not do. A scientific revolution is like a landslide: it moves whole layers of the lexicon to different places where they soon acquire their former deceptive naturalness and apparent permanence even though they no longer support the same superstructure. The delicate problem is the nature of the landslide, is it merely epistemological (i.e., a feature of our language about the world) or is it ontological (i.e., a feature of the structure of reality) as well? Kuhn sometimes wrote as though the structure of the world changes with each lexical shift, but he nonetheless maintained that we can use two different lexicons to describe the same phenomenal reality. It is difficult not to suspect that what Kuhn was groping for was an updated version of the Kantian noumenon / phenomenon dichotomy, although he framed his discussion in terms of access to manifold worlds of words.

Members of various linguistic communities organize the world in ways that need not be identical, and Kuhn even contemplated the abyss of saying that they need not overlap before withdrawing from an assertion that would preclude the possibility of the partial knowledge he wished to defend. Kuhn was no black-hole epistemologist. He did not believe that we are sealed in a linguistic house of mirrors even if he chose, at times, to use dazzling lights. What he conveyed to us is a vivid sense of the fact that the connection between the verbal signifier and the mental thing signified is more understandable and easier to describe than the connection of either with the world we revealingly qualify as "out there." Kuhn has sometimes been branded as an anti-realist, but it seems to me that he avoided this pitfall with the same kind of instinctive little lurch of faith that takes us out of bed every morning confident that the floor will be where we left it.

Observations are never made in a cognitive void and even the most apparently factual report comes to us tinged with anticipations and shrouded in some conceptual garb. This theory-ladenness, however, is neither as permanent nor as objectionable as it may sound. To say that I cannot get something without an instrument is not the same thing as stating that it cannot be reached. There is a kind of purity that is just another word for nakedness! Kuhn himself gave an excellent account of various ways in which such terms as force, mass and weight can be acquired. He offers a cautionary tale about the perils of trying "to straighten out the facts" before "getting the facts straight,"—in other words, of doing philosophy of science without history of science.

For Kuhn, the worlds of science, arts and philosophy are coterminous; several strands are intertwined and there is a constant interchange of information at the boundaries. Kuhn was aware of cross-fertilizations that may have been startling when they occurred or baffling to a later age but that make excellent sense when constructed with historical sensitivity. Consider, for instance, Emanuel Swedenborg's desire to explain the decrease of longevity since biblical times. This seems an unlikely stepping-stone to cosmological theories about the gradual slowing down of the axial rotation of the earth, yet it stimulated research in unsuspected ways. The rough outline of what was later called the Kant-Laplace Cosmogony was formulated by Kant in his *Allgemeine Naturgeschichte und Theorie des Himmels* in 1755, a work in which Kant devotes several pages to the inhabitants of the planets of the solar system whose "natures become more and more perfect and complete in proportion to the remoteness of their dwelling-place from the sun" (Kant 1960, 386).

Kant's interest in extraterrestrials was aroused by his reading of Swedenborg's *Arcana Celestia*, an eight-volume commentary on *Genesis* and *Exodus* that appeared between 1749 and 1756. The first volume of the commentary on *Exodus*, which was published in 1753, contains a description of Swedenborg's communications with the inhabitants of the Moon, Mercury, Venus and Mars. The second volume, published the following year, deals with the people on Jupiter and Saturn. The recent discovery that the period of rotation of Jupiter is ten hours compared to the Earth's twenty-four was submitted by Kant as an indication of the superior ability of the Jovians: in five hours of daylight they achieve as much as earthlings in twelve! Kant's science fiction blends the possible world of Swedenborg with the actual world of Newton in what for him, and many of his contemporaries, was a seamless robe. This may sound paradoxical but it suggests how historians and philosophers could get their act together.

History as a Safeguard Against Anachronisms

From the vantage point of any particular moment in the development of science, what happened before the discovery of the current method can easily be misunderstood. There is a natural tendency—conscious or unconscious—to mould great scientists of the past into the image of present-day scientists. Galileo is a particularly striking case of this kind of attempt. Let me borrow a couple of examples that Kuhn found interesting. The first comes from what was for a long time the standard English translation of Galileo's *Two New Sciences*. It has Galileo say that he "discovered *by experiment* some properties of motion that are worth knowing and which have not hitherto been observed or demonstrated" (Galilei 1914, 153). The words "by experiment" are absent from the original Italian version (Galilei 1890–1909(c), 190). The translators, Henry Crew and Alfonso de Salvio, obviously believed that by adding those two words they were merely making explicit what Galileo intended to convey. The result, of course, is to alter the very thrust of his argument, but the translators did not see this because they equated good science with experimentation, and they had no doubt that Galileo was a good scientist (I mean a scientist, 1914 vintage, when the translation appeared). The rapid development of the experimental sciences led to a distortion of Galileo's views in his own century. This can be seen in a passage from the first English translation of Galileo's *Dialogue on the Great World Systems* by Thomas Salisbury in 1661. The context is a discussion of the path that a stone would follow if it were released from the mast of a moving ship. The Aristotelian Simplicio claims that the stone will not strike the deck at the foot of the mast but some distance behind since the ship will have moved forward during the time the stone fell. Galileo's spokesman, Salviati, denies this and insists that it will strike the deck at the foot of the mast whether the ship is moving or at rest. When cross-examined, Salviati admits that he has not performed the experiment, and Simplicio asks why he should believe him rather than the reputable authors who held the opposite view. Salviati's rejoinder is translated as, "I am assured that the effect will ensue as I tell you; for it is necessary that it should" (Galilei 1661, 126). The original Italian reads: "Io senza esperienza son sicuro che l'effetto seguirà come vi dico" (Galilei 1890–1909(b), 171). Salusbury, perhaps unwittingly, left out the crucial *senza esperienza* ("without any experiment"). Writing at the time of the founding of the Royal Society, he saw Galileo as a scientist for whom only experiment counted. Two and a half centuries later, Crew and de Salvio, implicitly subscribing to the fashionable positivist interpretation of science, made Galileo think as they believed he must have.

What is interesting is not so much the attempt to foster an empiricist philosophy of science on Galileo as the fact, noted by Kuhn, that we are able to spot these occurrences, not because we went over earlier translations of Galileo with a fine comb but because we are familiar enough with Galileo's thought processes to spot incongruity when we come across it. Foreign languages can be learned; so can alien scientific methods. Just as a linguist who has mastered French recognizes a wrong gender, so a historian of science will pick out an anachronistic interpretation. He can never be sure that he has detected all the slips anymore than the linguist can be certain that he has identified all the grammatical mistakes, but both practitioners, in their different ways, can become sufficiently adept to rule out gross misinterpretations. In other words, they get it right most of the time, and this is as much as can be hoped for within the realm of human communications.

A deeper or, at least, a thornier problem is posed by the ambiguities that are almost always bound up with an early formulation of a new law. It is not only that there are many possible worlds, but that each world is open to several possible interpretations. Here again, the easy solution is the anachronistic one; the ascription to one man of the process that began long before him and was probably not completed until long after. A distinguished scientist and philosopher like Ernst Mach taught that Galileo, virtually single-handedly, founded the new science of mechanics, created the notion of force and discovered "the so-called law of inertia, according to which a body not under the influence of forces, i.e. of special circumstances that change motion, will retain forever its velocity (and direction)" (Mach 1960, 169).

Galilean scholarship has swung the other way since Mach, and we now believe that Galileo is better understood as bringing a long process that began in the Middle Ages to its culmination.[1] The realization that rectilinear motion is a state and not a process is a seventeenth-century achievement that cannot immediately be seen as having much in common with Aristotelian physics where motion in a straight line requires an external mover. But the principle of inertia did not spring Minerva-like from a single scientific head. Between Aristotelian mechanics and Newtonian dynamics we find a transitional phase in the theory developed by such thinkers as John Buridan and Nicole Oresme in the fourteenth century. Remaining within the tradition of Aristotelian physics inasmuch as it looked for a cause of motion, the impetus theory moved in the direction of the modern view by making impetus an impressed (i.e., internalized) and incorporeal force, and by considering the speed and the quantity of matter of a body as a measure of its strength. This theory encouraged a fresh approach to traditional problems by removing long-standing conceptual barriers. For instance, the Aristotelians had rejected outright the notion that the Earth could rotate on the grounds that a strong wind would be set up in the direction opposite to the Earth's motion. But if the air could receive an impetus and be carried around with the Earth, then the motion of the Earth itself became a distinct possibility in the real world of science and not merely in the world of science fiction. Likewise, by making the cause of motion an internal, impressed force, the impetus theory opened a new world to scientific speculation. Since air was no longer the cause of motion, as in the Platonic or the Aristotelian account, motion in a void was no longer ruled out, and it became possible to think of the idealized case of a body moving in a perfect void, i.e., in the complete absence of any impeding force. Furthermore by explaining all cases of motion in terms of one kind of cause, impetus, it removed the Aristotelian dichotomy between natural and constrained motion and provided the basis for a uniform interpretation of all motion, be it celestial or terrestrial. The fact that medieval scholars were able to question some of the fundamental tenets of the Aristotelian tradition in which they operated should be borne in mind. The

[1] In what follows I rely heavily on the excellent studies in Damerow et al. (2004).

Aristotelian-Scholastic cosmology was an intricate web of sophisticated concepts that discouraged thinking in certain other ways. The void, for instance, appeared self-contradictory, and the motion of the Earth physically impossible, but the reasons for setting up these limitations were clearly stated in Aristotle, and they were explicitly recognized and criticized by writers like Oresme and Buridan.

The word inertia in its technical sense was not introduced by Galileo, but rather by Kepler, who conceived of matter as characterized by "sluggishness," namely an inmate tendency to rest. Any lump or piece of matter comes to rest unless acted upon by some force. An important consequence of this view is that a body comes to rest not only *whenever* but *wherever* a force ceases to be applied to it. What moves the planets is the motive force emanating from the Sun. Were it to cease, the planets would come to a standstill. If we turn to Galileo we find statements that have a much more modern ring, such as, "Furthermore we may note that any degree of speed found in a moving body is, by its nature, indelibly impressed when the causes of acceleration or retardation are removed as is only the case on a horizontal plane" (Galilei 1890–1909(c), 243) or, "Consider a body projected along a horizontal plane from which all impediments have been removed; it is clear, from what has been more fully stated in the preceding pages, that this body will move along this plane with a motion that is uniform and perpetual, provided the plane extends to infinity" (Galilei 1890–1909(c), 268).

Mach took such statements to be identical with Newton's law of inertia. On closer inspection, however, we see that Galileo had not travelled that far. In Galileo's physics, all horizontal planes are small sections of the circumference of the Earth and the motion that endures is not rectilinear but circular. The Newtonian analysis of planetary motion as compounded of a linear inertial component and a descent towards the center is absent from Galileo's perspective because his belief that inertial motion is circular led him to claim that bodies on a rotating Earth would behave exactly like bodies on a stationary one. In his *Lectures on the Sunspots* of 1613, he wrote:

> If all external impediments are removed, a heavy body placed on a spherical surface, which is concentric with the Earth, will be indifferent to rest and to movement toward any part of the horizon. And it will maintain itself in that *state* in which it has once been placed […] Thus a ship, for instance, having once received some impetus through the tranquil sea, would move continually around our globe without ever stopping. (Galilei 1890–1909(a), 134–135)

Galileo did not fly in the face of tradition, but he restated the common belief in the perennial nature of uniform circular motion in such a way that it invited consideration of motion along horizontal planes and further investigation of the concept of state of motion. What may have been a passing remark in the text I have just quoted became a general principle in Descartes' *Principles of Philosophy*, where the "first law of nature" stipulates that uniform motion, like rest, is conserved because it is a state and not a process. A second and distinct "law of nature" adds that this motion is rectilinear, as becomes "the simplicity and immutability of the operation whereby God conserves motion in matter" (Descartes 1966–1974, 63).

Newton encountered the concept of a state of motion in Descartes and the continuing dialogue with his predecessor can be seen in the very title of his masterpiece, *Philosophiae Naturalis Principia Mathematica*, which repeats the title of Descartes' own work with two notable additions: the *Principles* are now said to be *mathematical*, and the philosophy *natural* (namely what we now call physics). The transformation of the title is but a sign of the profound change that the Cartesian law of inertia underwent in Newton's hands. Descartes' two laws are fused into one, and inertia is seen as resulting from the nature of matter rather than stemming directly from the metaphysical attribute of God.

Just as there is a continuous and intelligible path from Buridan, so there is one from Galileo to Newton, but we must be wary of ascribing to Galileo insights that were arrived at only by working out implications, which he himself did not contemplate, let alone analyze. Four changes were necessary to convert Galileo's concept into Newton's first law of motion. The notion of inertia had to be: (1) recognized as playing a fundamental role in motion, (2) seen as implying rectilinearity, (3) extended from terrestrial to celestial phenomena and (4) associated with quantity of matter or mass. The first three steps were taken by Descartes, the fourth awaited Newton. Of course, once the principle of inertia had been clearly formulated in the *Principia Mathematica* along with Newton's remark that Galileo had used it (Newton 1999, 424), no one could ever again turn to the *Two New Sciences* without reading into Galileo's words the correct Newtonian implications. But for Galileo himself these allegedly obvious consequences had not yet entered the realm of possibility.

In *The Equilibrium Controversy*, Jürgen Renn and Peter Damerow have recently enhanced our knowledge of Galileo's contribution by working out the implications of the law of the lever.[2] By means of the principle of virtual velocities, Galileo extended the law of the lever to the simple machines and even to problems of hydrostatics. In all instances, the governing principle is the equality of the product *mv* at one end of the lever to that at the other. The *momento* (moment) of the lever thus easily transforms itself into the *momento* (momentum) of the moving body. A possibility of serious ambiguity is built into the lever, and Galileo, together with the whole century following him, slips into it unaware.

[2] See Renn and Damerow (2012).

Since both ends of the lever move in identical time without acceleration, it is immaterial whether one uses the virtual velocities of the two weights or their virtual displacements. Velocities must be in the same proportion as displacements, and when Galileo states the general principle of the lever, he does so in terms of velocity, although he often uses the word displacement. Renn and Damerow show how it is all too easy to forget that the equivalence holds only for the lever and analogous instances in which a mechanical connection ensures that each body moves for the same time, and in which, because of equilibrium, the motion involved is virtual motion, not accelerated motion. The case of free fall is not, of course, identical to the conditions of equilibrium because the times involved are not identical and because two separate, accelerated motions take place.[3] If there is an equality of the product of weight *x* distance (that is, in our terms, work), there cannot be an equality of momentum (mv) but rather of kinetic energies ($1/2\ mv^2$). From the ambiguity of the lever springs the controversy between quantity of motion and *vis viva* in which the second half of the seventeenth century was to engage. This second phase has been studied by a number of distinguished historians of science, for instance, Richard S. Westfall in *Force in Newton's Physics*. What was lacking until the publication of *The Equilibrium Controversy* was a clear understanding of the historical and conceptual background to Galileo's endeavors.

The extensive research that led to *The Equilibrium Controversy* began in 2006 when the Max Planck Institute for the History of Science acquired a copy of Giovanni Benedetti's *Diversarum speculationum mathematicarum et physicarum liber* that appeared in 1585. This book comprises several treatises including one which contains a critique of a section of the Aristotelian *On Mechanics* that was much discussed at the time. While Benedetti's book is in itself an important source for understanding the struggles of early modern engineer-scientists with the ancient attitudes of mechanical knowledge, this specific copy is of special value because it contains handwritten marginal notes by Guidobaldo del Monte. Benedetti was influenced by earlier writers and more specifically by his master Tartaglia who had himself borrowed and modified material taken from the thirteenth-century Jordanus of Nemore whom he edited. The importance of Jordanus is illustrated by the fact that Guidobaldo del Monte not only read but annotated his copy of Jordanus. Renn and Damerow do not merely make the relevant material available; they offer a masterly survey of the development of mechanical knowledge from its origins in antiquity to the dawn of classical mechanics in the late Renaissance (Renn and Damerow 2012, 39–167). They stress that the development of technology owes much to *challenging objects* such as labor-saving machinery, ballistics, the stability of buildings and the performance of ships on the high seas. As a consequence a multiplicity of different pathways emerged.

[3] See Renn, Rieger, and Giulini (2000).

Renn and Damerow caution us against the danger of treating the results of these different approaches as if they were pieces of a puzzle that can be combined into a coherent whole. Strictly speaking, the solutions proposed in preclassical mechanics make use of alien concepts, such as natural and violent tendencies, which are incompatible with those of modern science.

A crucial problem was the exact relation between the key concepts of *center of gravity* and *positional heaviness*. Guidobaldo del Monte was proud to have reconciled the Archimedean theory of equilibrium, based on the concept of center of gravity, with the Aristotelian understanding of weight as tending to the center of the world. This reconciliation was embodied in what he saw as his greatest discovery: the realization that both an ideal balance and what he called a cosmological balance remain in indifferent equilibrium. Benedetti had claimed that, while such an indifferent equilibrium holds under terrestrial circumstances, it is impossible for a cosmological balance. This challenged Guidobaldo's synthesis, and while Benedetti's conclusion is in accordance with later classical physics, the controversy could not be settled with the arguments available at the time. In this sense, it was the equilibrium *controversy* more than its *resolution* that spurred the further developments of physics.

The Underlying Philosophical Stance

Philosophers of science clearly need historians of science if they are to avoid anachronisms, but historians of science can also learn from philosophers of science. I believe that Kuhn saw at least two ways in which philosophical considerations can prove useful to historians, namely (a) by elucidating the interpretive frameworks and the concepts employed, (b) by analyzing underlying methodological assumptions and (c) by clarifying the meaning of models and theories. I shall say a word about each aspect.

If history is to rise above a mere collection of anecdotes, it must be written from some point of view and with some unifying theme. It is here that the philosopher has a contribution to make by supplying some distinctive perspective, such as Kuhn's view about paradigms, normal science and revolutions. The two examples that were discussed above concerning the falsification of Galileo's text by well-meaning translators make it abundantly clear that no one can completely escape the climate of intellectual opinion prevalent in his own day. Unfortunately, historians only too often employ frameworks without thinking about them. They are left to operate as tacit assumptions, and are dangerous because they are not drawn out into the open and scrutinized for what they really are. The same can be said of key concepts, and this raises an important issue. Historians of science must immerse themselves in the writing of scientists of previous ages if they are to understand what they were actually up to. But it would be a futile exercise if their program of total immersion led them to lose their bearings in the world in which they actually live. Immersion is only profitable if it leads to eventual emergence into the contemporary setting with an enhanced ability to translate the past into terms that are meaningful for a present-day audience. The historian aims at recapturing the past not in order to live in the past, but in order to interpret it to those who cannot read its lessons first hand. Were the historian to divest himself of his twenty-first century frame of reference to the point of acquiring the full panoply of, say, Aristotelian thought, he would no longer be a historian but a living intellectual fossil.

Clarifying the Nature of the Argument

The philosopher of science can also cast light on the cogency of scientific reasoning. It is not enough to determine with historical accuracy what premises were employed to understand a scientific argument used in the past. To see the value of the argument one has to know whether the premises entail the conclusion or make it probable in the light of the evidence available at the time. The philosopher of science should be able, by virtue of his logical training, to examine the relations between the premises and the conclusions.

No one will deny that it is of intrinsic interest to discover whether an argument actually employed by a scientist of the past is cogent, but some might deny that this is history of science. The historian, it could be said, should ponder what the argument is, not whether it is any good. But this would be a narrow and ultimately stultifying approach. One of the most interesting questions in intellectual history is the determination of the value of arguments at the time when they were formulated. It is a task that requires the skills of both the philosopher and the historian of science, since we have to assess both the validity of the logical procedure and the nature of the evidence at hand. In this domain philosophical analysis can clearly compliment the historian's craft.

Any effort to reconstruct the past must be accompanied by a critical examination of what, in the light of hindsight, we know to have actually been the case. For instance, in investigating the models of Maxwell, Kelvin, FitzGerald, Helmholtz and others, it is important to recognize the nature and thrust of the methodological assumptions that guided nineteenth-century physicists.[4] In his paper on physical lines of force, published in 1861, Maxwell proposed a model of the electromagnetic field with the aid of certain assumptions, for example, that electromagnetic phenomena are due to the existence of matter under certain conditions of motion or pressure in every part of the magnetic field and not to action at a distance.

[4] See Bordoni (2008).

Likewise, he took for granted that there is inequality of pressure in the magnetic field that is produced by vortices. What he does not discuss is the ontological status of these assumptions, in plainer words, the reality that he ascribed to them. Was he saying that the electromagnetic field is really composed of the elements he described? Was he merely drawing an analogy with the mechanical system? Or, rather, was he showing what the electromagnetic field would be like if it operated on purely mechanical principles without claiming that this was necessarily the case?

The Role of Models

One need only raise these questions to realize that they are important if we are to understand what Maxwell was actually doing. The philosopher of science may be in a position to help the historian to ponder the various ways in which the term model is used, and I shall say a few words about three main kinds of models, which I take to be mechanical, theoretical and imaginary.

Mechanical models offer three-dimensional physical representations of objects such that, by considering them, we are able to know some facts about the original objects of study. The simplest kinds of these models are tinkertoy models of the molecule or of solar systems found in museums. They may be bigger or smaller than the original. They may also represent only those characteristics that a scientist is interested in. In this case, they may serve as an analog for the original as, for instance, when Maxwell represented the electric field by describing an imaginary incompressible fluid flowing through to a variable section. The analogous properties here are electrostatic force and that of the imaginary fluid, which both vary as the square of the distance from their sources, and the potential of the electric field and the pressure of the fluid, both inversely proportional to the distance. A model, in this sense, is an object distinct from the one that it represents. This is not the case of the next category.

Theoretical models like the billiard-ball model of a gas, Bohr's model of the atom, the corpuscular model of light or the shell model of the atomic nucleus, do not refer to a physical object that is distinct from the one of which it is a model but to a set of assumptions about the object that is itself under scrutiny.[5] For instance, the billiard-ball model is a set of assumptions according to which molecules in a gas exert only contact forces on one another, travel in straight lines except at the instant collision, are small in size compared to average molecule distances, and so on. These theoretical models can be further characterized. First, they describe an object or system by attributing to it an inner structure or a mechanism that is intended to account for certain features of the object or system. In the

[5] See Heilbron and Kuhn (1969).

case of the billiard-ball model, a molecular structure is ascribed to gases in order to explain observed relationships of pressure, volume, temperature, entropy, etc. Second, they are treated as useful approximations not exhaustive explanations. The billiard-ball model assumes that the only intramolecular forces are contact forces and thus ignores non-contact attractive and repulsive forces. This is useful in allowing a number of important relationships to be derived and in suggesting how the kinetic theory might be expanded. Thirdly, a theoretical model is set in the broader context of a more comprehensive theory. In the billiard-ball model, the behavior of the molecules always complies with Newton's laws.

The third group of models, imaginary ones, refers to a set of assumptions about a system that are supposed to show what the system could be like if it were to satisfy certain conditions but for which no factual claims are made. An example is Poincaré's model of a non-Euclidean world in which a number of assumptions are made such as that the temperature is greater at the center and gradually decreases as one moves towards the circumference where it is absolute zero, that bodies contract as they recede from the center and, as they move, achieve instant thermal equilibrium with their environment. This model satisfies the postulates of Lobachevskian geometry but Poincaré does not claim that such a physical world exists or that if a Lobachevskian world occurred it would necessarily be the one he describes. Such imaginary models serve the purpose of showing that certain assumptions, which may otherwise be thought self-contradictory, are at least consistent.

Armed with these distinctions, the historian can probe deeper into the status of Maxwell's mechanical assumptions. Until this is known it will be impossible to proceed to the analysis of Maxwell's argument. It is crucial to know whether Maxwell was actually ascribing the mechanical structure he described to the electromagnetic field or whether this was simply intended as a description of an analog or as a description of a possible mechanism if the field were purely mechanical. Unless we know what his model was intended to do, there is no way we can assess the validity of his reasoning. Nineteenth-century physicists did not explicitly distinguish three uses of the term model, but this does not mean that we cannot derive enlightenment from looking at their work with clearer concepts. If we consider the various models that were proposed in the nineteenth century, we readily see that it helps to bear these distinctions in mind. It is reasonably clear that in his 1861 paper, Maxwell was proposing an imaginary model of the electromagnetic field. He was saying what this field could be like if it were purely mechanical, but he was not claiming that it is actually like this or even that it is purely mechanical. In the case of Kelvin's celebrated mechanical contraptions what was being offered were representational models, while FitzGerald's proposition of 1899, according to which ether is a fluid, can be classified as a theoretical model.

Degrees of Likeness

There is much contemporary fuzzy-thinking about the meaning of theories. Although Kuhn was right in stressing that the framework of a given hypothesis determines to a large extent what questions can be raised and what views can be suggested about a particular problem, he did not manage to explain how different theories can be contrasted and appraised. On his view, one is practically driven to describe scientific change in revolutionary terms, to speak, for instance, of the "overthrow" of Aristotelian mechanics or the "victory" over phlogiston. As a result, theories seem "incommensurable" and their change can no longer be rendered intelligible in rational terms. This relativism is not, however, the outcome of an investigation of actual science and its history; it is merely a logical consequence of a narrow presupposition about the meaning of scientific terms. Positivists held that if the terms do not retain precisely the same meaning over the history of their incorporation into more general theories, then these theories cannot be compared, and the similarities they exhibit must be considered, at the best, as superficial and, at the worst, as deceptive and misleading. This claim rests on the assumption that two expressions or set of expressions must either have exactly the same meaning or must be completely different. The only possibility left open by this rigid dichotomy of meanings is that history of science, since it is not a simple process of development by accumulation, must be a completely noncumulative process of replacement.

The inherent weakness of this position turns out to be its retention of a positivistic concept of meaning. If anything the revolution is not radical enough! In spite of his spirited attack on the positivistic view that theories are parasitic on "observations," Kuhn nonetheless approached problems with that distinction in mind. He applied the old classification to a new purpose in a daring way by inverting the respective roles of the two members of the classical distinction: it was now the "theory" that determined the meaning and acceptability of the "observation" rather than the other way around. Observations were now so embedded in a particular theory that they lost any identity of their own, and ceased to be comparable. But this did not solve the problem of meaning: it simply replaced the theory of meaning invariance with the doctrine of incommensurable meanings. An alternative is to consider meanings as similar or analogous: comparable in some respects while differing in others. The difficulty in this interpretation lies in the concept of similarity or degrees of likeness of meanings. It is here that much more work needs to be done, and an indication of the urgency of the task is the proliferation of works on the use of metaphors, beginning with the book of George Lakoff and Mark Johnson (2003).

Globalization and the Quest for the Underlying Unity of History

An innovative thrust on how knowledge and history interact can be found in a book recently edited by Jürgen Renn (2012). The central theme is that there is only one history of human knowledge. There may have been many false starts, and there were probably many new and promising beginnings that were thwarted, wasted or simply forgotten, but there is a stream of cumulative discoveries that can be seen from a global perspective. Knowledge, whether scientific, technological or cultural, is now shared globally. But was this always the case? If we are tempted to say, "No," we may wish to pause after having been reminded of the rapid spread of the wheel in prehistory or of Roman law to such diverse areas as the Byzantine Empire and Ethiopia.

Globalization has been much discussed in relation to capital and labour, markets and finance, politics and military power, but it involves knowledge in many other significant ways, and the homogenization and universalization that are characteristics of globalization are fraught with dangers as well as opportunities. On the one hand, there is the threat of a standardization of mass culture that would result in a "dumbing down" of linguistic subtlety, political awareness and moral sensitivity. On the other hand, there is the opportunity of creating a richer network of social relations where diverse belief systems and political institutions would become complementary and could provide a stimulus for devising a more humane society on a worldwide scale.

Comprehensive globalization results from a number of factors such as the migration of populations, the spread of technologies, the dissemination of religious ideas and the emergence of multilingualism. These factors each have their own dynamics and history, and it is the study of their interconnection that enables us to see globalization at work. Historians of science have often focused on *who* made a discovery and *when* it occurred rather than on *how* it was rendered possible by the context in which it emerged. In other words, they privileged innovation over transmission and transformation. Renn redresses the balance by examining how knowledge is disseminated, enhanced and occasionally debased. For instance, the transfer of knowledge necessary for producing tools requires a framework of ideas that must be acquired. The late Peter Damerow, who was one of the driving forces behind the globalization project, was able to show how the powerful tools of writing and arithmetic were constructed and how they rendered possible the transmission of knowledge beyond the immediacy of verbal communication.

If systems of knowledge are essential to the organization of epistemic networks in a given social and cultural context, their subsequent restructuring is also of paramount importance. A particularly striking instance is the elaboration of Aristotelian natural philosophy, first in a theological milieu in the Middle Ages, and later in the wake of the scientific revolution in the seventeenth century. The outcome did not leave unaffected the intrinsic structure of Aristotelianism but created hybrids that changed the overall history of knowledge.

The relations between specifically scientific knowledge and socio-economic growth are clearly of importance. It was mainly in Europe that science and engineering became bedfellows and that a new class of scientists-engineers began to assimilate the know-how of craftsmen. This led them, in turn, to question the theories they had inherited. But we may well ask: Why is science reproducible and transportable? It can be argued that it is not because of any methodological principle, but because it focuses on means. The successful expansion of science within Europe created a model that was exported worldwide, including the replication of institutional settings and canons of what constitute knowledge. Science grew at an astonishing rate and travelled at an unprecedented pace. This was largely due to networks that introduced a connectivity that had once been assured by other bodies such as wealthy patrons, religious societies, universities and scientific academies. The rise of a new and highly mobile class of engineers was decisive. As their contribution to the solution of practical problems increased so did their personal prestige along with that of science. Local knowledge has generally been challenged, and frequently ousted by globalization, but there are several instances when they were preserved and served to shape the way new knowledge was perceived and integrated into different cultural traditions. Historians and philosophers of science must engage in a renewed dialogue over the significance of these changes. Thomas Kuhn would have considered them challenging, hence welcome. We should follow suit.

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Chapter 12
Constructive Controversy and the Growth of Knowledge

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I first met Tom Kuhn in 1962, shortly before his *Structure* was published, when I went to the International Congress of the History of Science, held that year at Cornell (and later in Philadelphia). I was then a scientist, and I was visiting the US primarily to do paleontological research at the US National Museum in Washington DC. But I already had strong historical interests. I therefore took the opportunity that the Congress offered, to meet and mix—for the first time—with historians of the sciences *en masse* (five years later I moved professionally into their field). On the first evening I happened to meet Kuhn and a few others, and all the talk was about his forthcoming book. I was immediately excited by what I heard, because Kuhn's ideas about the making of new scientific knowledge resonated with my own—albeit limited—first-hand experience of the practice of scientific research, far more than the abstract and idealized formulations of the philosophers whose work I had read. Kuhn's emphasis on the centrality of social interaction within groups of scientists, rather than the isolated individual minds presupposed by philosophical models, reminded me of Michael Polanyi's *Personal Knowledge* (1958)—which had earlier made a deep impression on me—with its insistence on the irreducibly personal, practical and often tacit character of the human processes of making knowledge, including scientific knowledge.

On the other hand, when later I read the published text of *Structure*, I was less persuaded by Kuhn's dichotomy between normal and revolutionary science, which seemed to be derived too narrowly from his own scientific training as a physicist and his early historical research on the Copernican "revolution." Practicing a very different kind of science, I felt the importance of taking into account the sheer diversity of the plural *sciences*, rather than treating physics as the ideal model for a monolithic "Science." (Many years later, when I was teaching in the Netherlands, one of my Dutch colleagues used to refer to the idea of a singular "Science" as the "*anglophone heresy*", in contrast to the mainstream Continental tradition of recognizing plural *Wissenschaften*, *wetenschappen*, *sciences*, *scienze* etc.) Reflecting on the then current state of my own science—paleozoology in the service of evolutionary biology—and on its earlier history, it seemed to me that "normal science" was often much more dynamic and innovative than Kuhn's model allowed, and "revolutionary science" often much less disruptive and not necessarily leading to incommensurabilities.

Kuhn's original and quite modest concept of paradigms—as concrete pieces of research that act as *exemplars* for fruitful further work—remains, I think, much more useful for understanding the making of scientific knowledge than his later concept of paradigms as alternatives that are radically incompatible and incommensurable. Fruitful exemplars have often emerged from a social process of controversy, entailing both conflict and collaboration, within a limited "*core-set*" of active researchers. When Harry Collins introduced this useful term, he emphasized the small size of core-sets, even within the Big Science of modern physics from which his examples were drawn. I suggested at the time that any core-set had, as its epistemic correlate, a similarly limited "*focal problem*" that had arisen within a wider scientific field. The successive and successful resolutions of specific focal problems by their respective core-sets might then help to describe how, in the history of the sciences, fields of relatively "normal" science have not always been static, constricted or eventually sterile, but often cumulatively fruitful and ultimately transformative, yet without any disruption by radically "revolutionary" change. I still think that this kind of "landscape" of scientific work fits the historical record of the sciences—at least the more observational and classificatory sciences, if not the highly experimental or rigorously mathematized ones—much better than the Kuhnian dichotomy allowed.

However, this claim can only be substantiated by assembling many relevant case studies of the dynamics of specific core-sets as they argue over and eventually resolve specific focal problems. This is where historical studies are indispensable, because problems in present-day sciences cannot show us how they may be resolved in the future. Yet current research by historians of the sciences is, in my opinion, giving too little attention to this issue. Any single case study may indeed be necessarily "*micro*" in character (and therefore currently unfashionable), yet cumulatively they ought to be contributing to issues that are as "*macro*" as any in our field.

By coincidence, around the time that I first met Kuhn I was given access to the previously unstudied manuscript papers of George Greenough, a prominent English geologist of the early nineteenth century and the first president of the Geological Society in London, which in turn was the world's first body of its kind. Among a mass of Greenough's otherwise unsorted papers I found a bundle of letters labeled by him as "Great Devonian Controversy." Reading these letters became my serendipitous entry point into an argument that had agitated the community of geologists in nineteenth-century Britain, and eventually much more widely, but which had almost been forgotten by their twentieth-century successors. I spent many years analyzing this highly controversial focal problem, trying to understand how it was eventually resolved—by a complex process of social dynamics within a quite small core-set of historical actors—into a consensus that has endured to the present day. When my book on *The Great Devonian Controversy* (Rudwick 1985) was published it got a lot of attention, some of it highly critical, from philosophers and sociologists as well as historians. But the detailed narrative that substantiated my analysis, and which was made possible by exceptionally rich primary sources, made it a very long book. Probably few readers read it from start to finish, and it has understandably faded from view. Yet, more than a quarter-century after it was published, I think it still has something to offer our current discussions of the making of scientific knowledge, if only as a case study that would be worth testing against others.

The Devonian controversy erupted in the 1830s among leading practitioners of the then quite new science of "*geology*", initially just in Britain but soon in the rest of Europe and eventually as far afield as Russia and North America. Superficially it was concerned simply with the classification and nomenclature of certain major *formations* or sets of strata in relation to others. But it was seen to challenge the dominant exemplar—embodied in the practice of *stratigraphy*—that formations could and should be identified, and hence *correlated* between one region and another, by finding the same fossils in them everywhere. They could then be arranged unambiguously in a unique structural order. Geologists agreed that this pile of rocks corresponded to the temporal order in which they had been deposited: they were a reliable record of the Earth's deep history, from which a reliable record of the history of life could be reconstructed (later, of course, this in turn became major evidence for evolutionary theories). The Devonian controversy arose when this well-established practice of stratigraphy was extended from the relatively easy cases of the younger formations to the more difficult cases of much older and more disturbed rocks. (The magnitude of the Earth's timescale was not at issue among nineteenth-century geologists, all of whom, whether religious or not, agreed that it was inconceivably vast although not yet quantifiable.)

The controversy was triggered when the highly respected English geologist Henry De la Beche reported finding fossils characteristic of the Coal formation, which was of supreme economic importance in the early Industrial Revolution, in the far older strata then recently named Cambrian. This anomaly was so radical and so unexpected—and its potential economic significance so important—that the factual reliability of the report was immediately questioned by other leading geologists, notably Charles Lyell and Roderick Murchison. There ensued some eight years of intense and sometimes acrimonious argument, recorded in field notebooks, in letters (often in turn recording private conversations), in reports of scientific meetings, and in published papers and books. A steadily expanding body of relevant evidence was deployed, with rhetorically effective argumentation on all sides, to support a growing array of diverse interpretations. The perceived balance of plausibility among these candidate solutions shifted repeatedly, as leading geologists changed their positions as a result of hearing persuasive new arguments or personally seeing persuasive new evidence in the field or in museums.

The primary sources—which in their rich density and completeness are possibly unmatched anywhere else in the history of the sciences—make it possible to track all these changes month by month, and at some points even day by day. It is possible to trace how, in real time, a period of bewilderingly diverse interpretations eventually converged into a consensus among the core-set (of about a dozen leading geologists), leaving only a couple of marginal figures holding out as dissidents and disagreeing with each other (see Fig. 12.2). This detailed historical evidence invalidates any claim that the resolution of the problem signaled the "triumph" of one side of the initial argument and the "defeat" of the other (as historical accounts of scientific controversies are usually framed, with the history often being written, of course, by the "winners"). Instead, it shows how the social process of controversy, with all sides deploying the changing empirical evidence to their best advantage, repeatedly forced the actors to modify their positions. Out of this social process a third and eventually successful alternative emerged, which *had not been foreseen by either side at the outset*: it incorporated elements derived from *both* the initial rivals, yet it was no mere compromise (see Fig 12.1). In other words, the consensual solution to the focal problem resulted in the production of genuinely *new* knowledge, which has been incorporated so successfully into the practice of the science that the Devonian controversy has been almost completely forgotten by modern geologists (its consensual product is the defining of a distinctive "Devonian period" in the history of the Earth, during which, for example, both plant and animal life made their first significant appearances on land). Was this new knowledge a social construction or a discovery about the real world? It was, of course, *both*.

During the half-century since Kuhn's *Structure* was published, the often acrimonious arguments about scientific knowledge *as social construction* could have been avoided, or at least ameliorated, if practitioners of "science studies" (historians, philosophers and sociologists) had considered other epistemic projects that are, like the natural sciences, *both* wholly human and social constructions *and* truth-bearing representations of natural realities. *Maps* and *mapping* were cited occasionally in this context, but I think they still have much to teach us. Anyone who uses maps extensively must be well aware of the sheer diversity of these representations of one-and-the-same reality, which adopt equally diverse sets of socially understood conventions. The world-famous map of London's "Underground" or metro system, for example, is utterly unlike a street map of the same city, or maps that show the major roads, aviation routes, weather conditions or underlying geology of the same region. Yet all these maps may be judged to be accurate (or at least corrigible) representations, which can be used equally successfully for their diverse respective purposes. Add the historical dimension, and maps may also be rightly judged to have been progressively *more* accurate and reliable representations; or, if they differ radically from their counterparts in other historical periods, it may be because their intended purposes were quite different (for example, the early *mappae mundi* centered on Jerusalem, compared to modern world maps). The analogy with the historical construction of scientific explanations should be obvious.

The past half-century has seen a welcome increase in historians' awareness of the value of *visual* sources of all kinds (including maps), compared with their earlier almost exclusive use of textual sources. I was acutely conscious of this when I moved in mid-career from a strongly visual science into historical teaching and research: visual images and diagrams were generally regarded by my new colleagues as optional decoration, not—as in my science—as an indispensable complement to any verbal exposition. A paper I published in *History of Science* in 1976, arguing for the importance of visual sources in historical work, was almost ignored by historians for several years (though it was welcomed by scientists with historical interests), before being cited retrospectively—to my bemusement—as a "pioneer" example of what has since become an active and fashionable field of historical research. Yet in contrast to this new appreciation of visual imagery in primary sources, historians still rarely use visual imagery of their own devising, to explicate their historical interpretations. Back in 1985, reviews of my Devonian book were sharply divided on this issue: the scientists found its interpretative diagrams helpful and illuminating, but most of the historians, sociologists and philosophers said they found them incomprehensible or even repellent. Nothing much has changed since that time: I think "science studies" scholars still deprive themselves of mental tools that many kinds of scientist find valuable or even indispensable (see Figs. 12.1, 12.2).

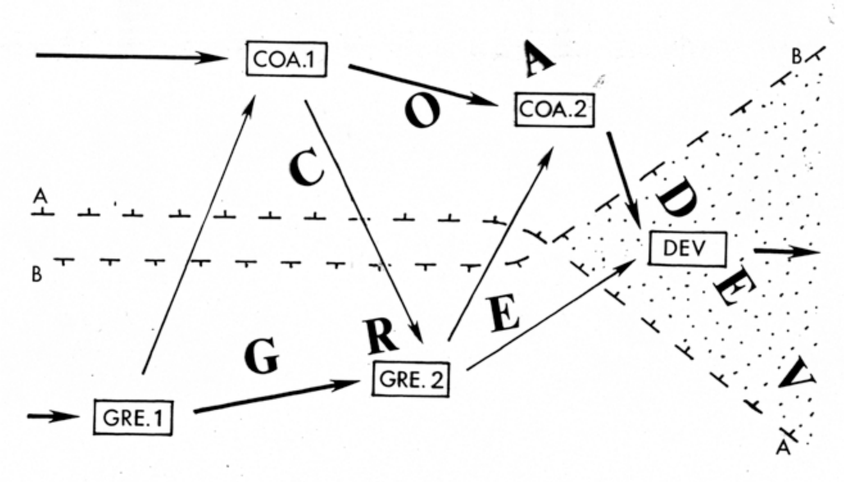

Figure 12.1: A schematic summary of the structure of the Devonian controversy. Historical time (in the 1830s and 1840s) flows from left to right. The vertical dimension represents the relative *theoretical distance* between five major interpretations of the developing body of empirical evidence. They are situated in three *interpretative domains*, classed as *GRE*, *COA* and *DEV*. Thick arrows are lines of *interpretative development*; thin arrows represent *interpretative pressure* from one interpretation on another. Thus when *COA. 1* challenged the pre-existing *GRE.1* it was maximally distant from it; but each later conceded, under pressure from the other, modification into *GRE.2* and *COA.2*, which reduced the distance between them. Later still, under further pressure from *GRE.2*, *COA.2* transmuted dramatically into *DEV*, a new class of interpretation unanticipated on either of the previous alternatives. The *GRE* and *COA* domains had been separated by non-negotiable and incompatible claims that formed the *interpretative boundaries* A and B; but the new interpretation *DEV* resolved their incompatibility ("the battle lines filtered silently through each other, until they faced outward, leaving at their rear a domain defended by them *both*"). The empirical success of *DEV* then expanded rapidly, as represented by the expansion of the stippled *DEV* domain and the consequent marginalizing of the earlier rival domains. This diagram (reproduced from (Rudwick 1985), Fig. 15.2) was an attempt to conceive the basic argumentative structure of the controversy, stripped—temporarily—of all the contingencies of the historical actors who proposed these interpretations.

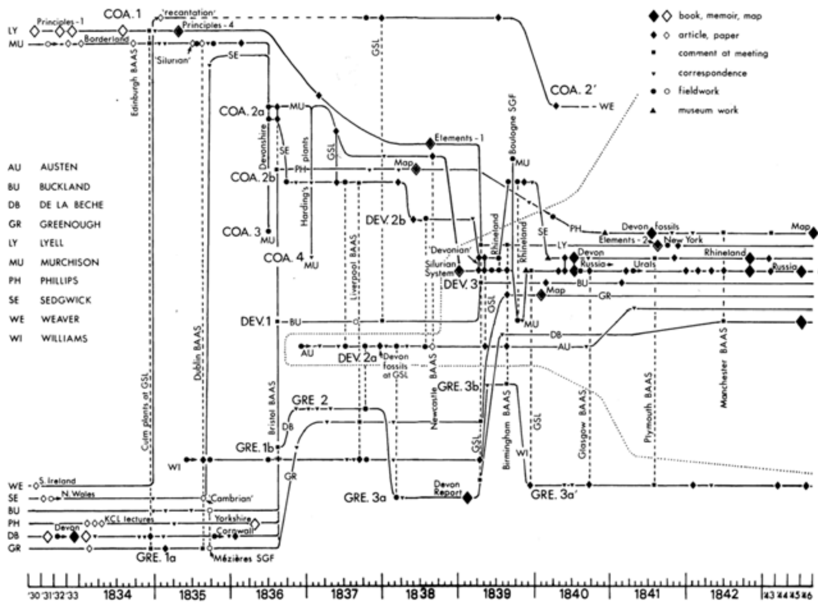

Figure 12.2: A schematic summary of the Devonian controversy, showing the theoretical trajectories of ten major historical actors, plotted against a quantified timescale of months and years from 1834 through 1842 (and, on a condensed scale, for some earlier and later years), marking the main points of documentary evidence for each trajectory and some of the scientific meetings at which the Devonian problem was discussed. The vertical dimension represents the interpretative distance separating the trajectories. They traverse many alternative interpretations within the main classes of *GRE*, *COA* and *DEV*; some variants were held only briefly, others were more stable through time. The diagram illustrates how an initial near-consensus around *GRE.1a*, with a dissident minority arguing for *COA. 1*, was succeeded by a middle period of great confusion, shifting commitments and rapid change, until the proposal of *DEV.3* (which had tentative forerunners from *DEV.1* onwards) led rapidly to a consensus, leaving only two dissidents out on the margins. This diagram (reproduced from (Rudwick 1985), Fig. 15.5) was an attempt to depict the dynamics of the controversy in terms of its core-set of leading historical actors.

This leads me to a more general reflection on our relationship to scientists. Scientists' historical views are not always limited to the level of "Let me tell you an anecdote." In some sciences—my own field of the Earth sciences is certainly one – there are many scientists doing serious historical work in an institutional structure parallel to ours, but largely separate from it. Back in 1994 the Geological Society of America sponsored a successful conference in San Diego, which was designed to lessen this divide by bringing together geologists interested in history and historians interested in geology. There was much talk of these groups as being, respectively, "insiders' and "outsiders": the geologists considered that they had "inside" knowledge of the science, whereas the historians could only observe it from the "outside." But I pointed out that these labels could equally well be reversed: it was the historians who knew the "feel" of past periods from the inside, thanks to the virtual time-travel made possible by lengthy immersion in the historical sources and their wider context, whereas the scientists often lacked that inside knowledge. (I felt able to make this point without offending either group, because I was one of the handful of participants who could claim to belong to both!) How then should we historians of the sciences interact with scientists? Some of us will continue to use scientists as valuable primary sources for the recent history of their sciences. But should we not also attend to their evaluation, as "insider" participants, of the dynamics of their own research, and all the other issues that engage us as "outsider" analysts? In my opinion the current trend among historians of the sciences, to seek ever-closer relations with "mainstream" historians, is not an unmixed benefit, if it leads us to neglect our links with working scientists.

On the positive side of this relationship, much excellent historical work, since *Structure* was published, has explored the role of new instrumentation and other material "tools" that have interacted with "ideas" in the making of scientific knowledge. However, to complement this, I think more attention needs to be given to the role of material *objects* such as natural specimens, and not only those such as *Drosophila* that have been used as materials for experiments. The conceptual dominance of physics in science studies of all kinds (including historical studies) has led, in my opinion, to an over-emphasis on what can be confirmed by replication in experiments. This needs to be balanced by recognizing the powerful role that natural objects—even *unique* objects—have played in sciences in which experimentation is subordinate or even negligible. For example, the chance discovery (by coincidence, shortly after Darwin's *Origin* was published) of a fossil *Archaeopteryx*, apparently intermediate in its anatomy between reptiles and birds, was used at the time as persuasive evidence for macroevolution. But this initially unique specimen (from Solnhofen in Bavaria) would have been immensely important in strengthening the case for an evolutionary history of life, even if it had never been "replicated"—as in fact it was later—by the discovery of further specimens of the same strange extinct organism.

Finally, the "experimental turn" in the historiography of the sciences, since *Structure* was published, has yielded valuable insights into the problematic nature of experimentation, not least as revealed by attempts to "*re-stage*" classic experiments. However, such studies of what has been done in laboratories need to be complemented by studies of the scientific practices located in two other major—but relatively neglected—sites of scientific knowledge-making, namely the *field* and the *museum*. In my own work on the history of the Earth sciences, I have found it immensely valuable to "*re-tread*" historic fieldwork, visiting classic sites and sights (specific quarries, mountains, volcanoes, etc.): not to discover what was "really" the case—as presentist-minded scientists might claim to be doing—but to try to see the historical actors' evidence "*through their eyes*" and thereby understand their reasoning and argumentation. In the same way I have studied in museums the *particular* specimens (of minerals, rocks, fossils, etc.) that historical actors described and argued about, again to try to understand how they handled the specific evidence that was available to them. In both these kinds of historical study, natural objects (large and small) are treated as primary sources. I think that much more work could be done along such lines, for all the natural-history sciences, provided that the "seeing" is as analytical (and not merely celebratory) as in our studies of conventional textual sources.

I have used my own experience of working in the history of the Earth sciences, in the half-century since Tom Kuhn's *Structure* was published (and since I first met him), as a small example of the immensely fruitful influence that his work has had on our human understanding of the making of new and reliable natural knowledge. I think it will be no surprise if that influence continues in some form through the next half-century.

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Chapter 13
The Structure of Scientific Revolutions and History and Philosophy of Science in Historical Perspective

Theodore Arabatzis

Introduction

My late teacher Gerry Geison used to say that *The Structure of Scientific Revolutions* is a book worth rereading once a year. With each new reading, one is bound to discover a new insight about science, and, I would add, one is also bound to raise new questions about the character of this revolutionary book. More than fifty years after its publication, *Structure* remains as intriguing and hard to categorize as it was when it first appeared. No less an authority on the book's character than its own author, even Kuhn himself had trouble classifying it: "Asked what field it [*Structure*] dealt with, I was often at a loss for response" (Kuhn 1993, xii). Recently, Ian Hacking again raised the question "is the book history or philosophy?" without addressing it directly (Hacking 2012, x). So, what kind of intellectual work is *Structure*, given that its ideas "are drawn from a variety of fields not normally treated together"?[1] Clarifying the book's interdisciplinary character may help us better understand and hopefully strengthen the troubled relationship between history and philosophy of science (HPS).

HPS as an integrated discipline goes back to the nineteenth century, when major philosophers and historians of science, from Comte and Whewell to Mach and Duhem, amalgamated historical study and philosophical reflection, imposing a "shape" on the scientific past.[2] During the first half of the twentieth century, however, as philosophy of history on a grand scale became suspect and philosophy of science focused on science as a static body of knowledge, issues about the pattern of scientific development receded into the margins of philosophical discussion. Kuhn's work brought those issues back to the forefront of philosophy of science, thereby reviving a nineteenth-century tradition of viewing science from a historical-cum-philosophical perspective. Since *Structure* offered a grand narra-

[1] The quotation is from Kuhn's application for a Guggenheim fellowship, dated 22 October 1953. See Hufbauer (2012, 459).

[2] I borrow the term from Graham (1997).

tive of scientific change in terms of long periods of normal science punctuated by scientific revolutions, it can be plausibly read as a contribution to the philosophy of history of science.[3]

Furthermore, *Structure*, more than any other recent work, opened up space for HPS as an integrated project, notwithstanding Kuhn's later claim that there is no such thing (Kuhn 1977, 4; 1980, 183). *Structure* raised novel questions (e.g., about the nature of scientific discovery or the character of scientific practice) that required an interdisciplinary approach. Neither historical research nor armchair philosophical reflection, by themselves, sufficed to address those questions. Rather, they could be tackled only through a combination of historical interpretation and philosophical analysis. Historical scholarship and philosophical argumentation had to be brought under the same roof.

In what follows, I will do four things. First, I will discuss some ways in which history and philosophy of science are intertwined in *Structure*. Second, I will briefly outline the history of HPS after *Structure*. Third, I will point out some possibilities for HPS opened up by *Structure* which, however, were not sufficiently explored in the subsequent career of HPS. Finally, I will reflect further upon one of those possibilities, namely philosophical history of science.

HPS in *Structure*

Structure was a rich blend of "something resembling philosophy" (Kuhn 1977, 8) and history. The relationship between philosophy and history of science in Kuhn's work has been extensively discussed and remains a controversial issue.[4] The focus of the discussion has been on whether Kuhn's extensive use of historical examples provides evidence for his philosophical claims or whether those claims were meant to stand on their own. While this is an important issue, my concern here is rather different; I plan namely to look at how Kuhn brought his philosophical acumen to bear on the historiography of science.

Before I discuss this, however, let me mention two uncontroversial points in the literature on Kuhn and HPS. First, it is widely agreed upon that Kuhn's philosophical reflections on scientific practice were elucidated and made plausible by discussions of historical cases, such as the Copernican Revolution or the discovery of oxygen. Conversely, on the basis of Kuhn's philosophical insights, such as the incommensurability of competing paradigms or the extended character of scientific discovery, those episodes were seen in a new light.

[3] Jardine (2009); Skinner (1990); Hollinger (1973, 370); Gordon (2012).

[4] See, e.g., Caneva (2000); Hoyningen-Huene (1992); Kindi (2005); Mladenović (2007); Sharrock and Read (2002).

Another uncontroversial point is that there was a direct link between Kuhn's experience as a historian and his philosophy of science. The notion of incommensurability, for instance, was motivated by Kuhn's difficulties in interpreting historical sources. "Incommensurability is a notion that for me emerged from attempts to understand apparently nonsensical passages encountered in old scientific texts. Ordinarily they had been taken as evidence of the author's confused or mistaken beliefs."[5]

Kuhn's realization that there was a conceptual gap between older modes of thought and contemporary science was in tune with the "new historiography of science," which ruled out anachronisms and retrospective evaluations of past scientific practice. These historiographical maxims were particularly prominent in the work of Alexandre Koyré, whom Kuhn deeply admired. *Structure* was put forward as an articulation of the image of science that was implicit in Koyré's innovative historiography.[6] Kuhn's account of scientific development provided, in turn, a powerful philosophical explication and defense of Koyré's non-presentist historiographical approach. In particular, the notion of incommensurability captured the conceptual and axiological distance between older paradigms and their contemporary descendants. Thus, it lent philosophical support on the resolve of historians to avoid contemporary concepts and values when interpreting past scientific beliefs and practices.

Thus, Kuhn's philosophical work has to be examined and appraised in close connection with his practice as a historian of science.[7] Despite his later ambivalence towards integrated HPS, there was an underlying unity in Kuhn's historical and philosophical work. He wore both hats (the historian's and the philosopher's) all the time.[8]

The new historiography of science was enriched further by Kuhn's philosophical vision. Philosophical theses, such as the theory-ladenness of observation and the importance of epistemic values in theory-choice, shed new light on previously puzzling features of scientific life, such as the existence of protracted disagreements among scientists. Kuhn did not just draw upon historical scholarship to score philosophical points. Rather, he employed the philosophical tools he had fashioned in order to illuminate key episodes from the history of science.[9]

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[5] Kuhn (2000, 91); cf. also Caneva (2000, 98).

[6] Kuhn (1970, 3). Kuhn's indebtedness to the "new internal historiography of science" has been emphasized by Paul Hoyningen-Huene (1993). Cf. also Larvor (2003).

[7] Cf. Sharrock and Read (2002, 2).

[8] Cf. Kuhn (2000, 85, 91); Marcum (2015, 109–111, 115–116).

[9] It should be noted that Kuhn stressed the philosophical, rather than historiographical, ambitions of *Structure* (see, e.g., Kuhn (2000, 276)). As I will suggest below, however, the significance of that book lies equally in its fruitful historiography.

For instance, on the basis of his philosophical analysis of scientific discovery, Kuhn developed a novel approach to the discovery of oxygen. In the older historiography of the chemical revolution, which dated back to the nineteenth century, the discovery of oxygen was attributed either to Joseph Priestley or to Antoine Lavoisier, depending on the national loyalties of the chemist-historian. In either case, those attributions presupposed that scientific discoveries are precisely datable events that can be credited to particular scientists. Kuhn argued that this presupposition blocked the historical understanding of the emergence and consolidation of the oxygen theory of combustion, by raising unanswerable and misleading questions about the date of the discovery of oxygen and the identity of its discoverer.

Kuhn suggested instead that scientific discovery is an extended process, involving the development of a novel theoretical framework, which inevitably spans a prolonged period and is a collective achievement. This explains why many scientific discoveries cannot be exactly dated or exclusively associated with individual scientists. Thus, Kuhn's meta-historical conception of discovery gave rise to a more sophisticated understanding of the discovery of oxygen. This could now be seen as the outcome of an extended and controversial process of experimentation and theorizing that involved the isolation of a constituent of atmospheric air and its conceptualization as a chemical element with distinct properties. Incidentally, the question of who discovered oxygen, Priestley or Lavoisier, now lost any appeal it might have originally held.

HPS after *Structure*

In post-*Structure* developments, we can discern two main strands of HPS: historical philosophy of science and philosophical history of science. The former addresses general epistemological and metaphysical issues about science in light of its historical development. The latter explores particular historical episodes while taking into account philosophical considerations about, e.g., the dynamics of scientific theories or the processes of conceptual change.

If we look at the history of HPS with this distinction in mind, we immediately realize that HPS has been dominated by the first strand, historical philosophy of science. To begin with, history of science has been used as a source of "data" for generating and evaluating philosophical accounts of scientific development. In *Structure*, Kuhn suggested that "theories about knowledge" should be subjected "to the same scrutiny regularly applied to theories in other fields" (Kuhn 1970, 9). In that spirit, he drew upon historical scholarship on the Copernican and the Chemical Revolutions to motivate and support his model of scientific development. Other philosophers of science, most notably Imre Lakatos and Larry Laudan, took up the challenge of developing alternative accounts of scientific change that could capture its rational and progressive character (purportedly undermined by *Structure*). In that "confrontation model" of the relationship between history and philosophy of science, history of science was seen as a repository of facts for testing theories of scientific change (Schickore 2011). This approach to historical philosophy of science is now passé, primarily because there are grave doubts that historical evidence can be sufficiently detached from philosophical theories so as to be used in their evaluation (Nickles 1986). Rather, it has been plausibly suggested that philosophy of science should be seen as a hermeneutic enterprise that interprets the historical record in terms of its analytic concepts, which in the process of interpretation may be refined or modified.[10]

Furthermore, history of science has been brought to bear on salient philosophical issues, such as rationality, relativism and realism. In the 1960s and 1970s, historical episodes of theory change (e.g., about the transition from ether-based electromagnetic theory to the special theory of relativity) were discussed in connection with the rationality of scientific development. The philosophers who contributed to that literature were for the most part interested in retrospectively justifying the outcome of past scientific episodes in light of philosophical accounts of scientific change, such as Lakatos' methodology of scientific research programs (Howson 1976).

Kuhn complained that philosophical case studies of that nature confused *ex post facto* philosophical justification with historical explanation (Kuhn 1980). It is no wonder that historians of science remained indifferent to HPS so conceived. They didn't see any added value in that enterprise and were repelled by its normative character. Thus, they stayed clear of the debates over HPS.[11]

In the 1980s, history of science entered forcefully into the realism debate. Historical cases of entities that have dropped out of the ontology of science (e.g., phlogiston and caloric) were used to throw doubt on "convergent realism," the view that science has been progressing towards the truth about nature (Laudan 1981). Ever since, history of science has occupied a central stage in philosophical discussions on scientific realism (Vickers 2013). In this area too, even though history and philosophy of science were brought closer together, all the action was on the philosophical side. Historians of science kept a safe distance from those debates, perhaps because they had already distanced themselves from the image of science associated with "convergent realism." The realist tendency to view older scientific theories as imperfect versions of contemporary ones was (and still is) anathema to most historians (Arabatzis 2001).

[10] See Schickore (2011); Nersessian (1995) made the same point earlier in connection with cognitive history of science.

[11] As can be glimpsed from Zammito's comprehensive survey (2004, chap. 4).

Proceeding to the second strand of HPS, philosophical history of science, we can see that it has been a relatively neglected endeavor. Whereas Kuhn's *Structure* made evident the philosophical stakes in the history of science, the historiographical relevance of philosophy of science has remained rather obscure. To many historians of science, philosophy of science still lacks "pragmatic value" (Buchwald 1992, 39).

Furthermore, Kuhn's grand narrative of scientific development was not well received by historians of science, who have been skeptical of his generalizations and have not adopted his terminology and conceptual apparatus (paradigm, normal science, crisis, revolutions, etc.) to describe and explain how the sciences have developed.[12] It is indicative of the historians' continuing indifference to Kuhn that only one major history of science journal, *Historical Studies in the Natural Sciences* (42:5, 2012), has devoted a special section on the 50th anniversary of *Structure*. Sociologists of science and intellectual historians, on the other hand, have been more receptive to Kuhn's message.[13]

Nevertheless, it would be fair to say that Kuhn's book has influenced substantially, if indirectly, historiographical practice.[14] Historians of science have learned from Kuhn, among other things, to appreciate the "losers" in scientific revolutions and see them as rational agents that resisted the new paradigm, often for good reasons. Furthermore, Kuhn's approach to science as a practice shaped by tradition, involving tacit knowledge and depending on rigid forms of training has stimulated historical research and has been substantiated by several historical and sociological studies.[15]

Structure and Philosophical History of Science

What morals about HPS can we draw from *Structure* and its early reception among historians and philosophers of science? Kuhn's classic work offers a spectrum of possibilities for integrating HPS, each possibility blending philosophical analysis and historical interpretation in a distinct manner.[16] On the philosophical side, there is little doubt that history of science can cultivate philosophical intuitions and function as a source of insights about the epistemology and the ontology of

[12]Cf. Hollinger (1973).

[13]See the special issues of *Social Studies of Science* (42:3, 2012) and *Modern Intellectual History* (9:1, 2012), respectively.

[14]Jan Golinski (2011) has plausibly argued that Kuhn's impact on history of science was mediated by the sociology of scientific knowledge and the Edinburgh School.

[15]See, e.g., Kaiser (2005). For a recent, rather critical assessment of Kuhn's impact on history of science, see Cohen (2012).

[16]Some possibilities for HPS, although not necessarily in a Kuhnian spirit, are suggested in Arabatzis and Schickore (2012).

science.[17] An engagement with history of science can also cultivate a sensibility to the complexity and variability of scientific practice, which have to be accommodated within an adequate philosophical account of science.[18] On the historiographical side, philosophy of science can stimulate and enrich historical work. In the subsequent history of HPS, only some of those possibilities have been explored in depth, mostly those related to historical philosophy of science. HPS has been, for the most part, a philosopher's game, where internal history of science is put in the service of philosophical theorizing. I think it's high time to redress this imbalance and further develop philosophical history of science by exploring how philosophy of science can be involved in historical interpretation.[19]

Philosophical history of science, as I conceive it, aims at understanding the scientific life in terms of philosophically articulated meta-scientific concepts, such as discovery, objects, models, epistemic values, the relationship between theory and experiment, etc. By actively drawing upon the philosophical literature on, say, scientific modeling, philosophically inclined historians of science may shed new light on familiar scientific episodes and in the process refine and modify the philosophical tools that they use.

I see Kuhn's *Structure* as the founding work for philosophical history of science in the above sense. In that respect, its importance did not lie in Kuhn's grand narrative of scientific development. As I already pointed out, although this narrative may fit some historical cases, it has not been taken seriously by historians of science, who, for the most part, have moved away from big pictures of scientific development and towards small-scale analyses of particular developments.[20]

Rather, the significance of *Structure* for philosophical history of science rests on some of Kuhn's insights into scientific practice, such as the role of epistemic values in theory-choice. Furthermore, his liberal conception of scientific rationality led to a more sympathetic understanding of the "losers" of scientific controversies, who can no longer be seen as irrational holdouts obstinately resisting scientific proof. Despite the fact that "there is no Kuhnian school of history" (Andersen, Barker, and Chen 2006, 1), several philosophically inclined historians of science have enlisted aspects of Kuhn's philosophy of science in the service of historical analysis and interpretation.

To begin with, incommensurability, a key Kuhnian notion, has been deployed to interpret various debates, from the wave theory of light (Buchwald 1992) to recent particle physics (Pickering 2001). Jed Buchwald, for instance, employed Kuhn's taxonomic approach to incommensurability to conceptualize

[17] Cf. McMullin (1974); Schindler (2013).
[18] Cf. Kindi (2005).
[19] For systematic reflections in this direction see Kuukkanen (2013).
[20] Cf. Golinski (2011, 25); Hollinger (1973, 370); Gordon (2012, 73).

the development of nineteenth century optics and electromagnetism, and argued that Kuhn's philosophical framework can deepen our understanding of the developments in those fields. More recently, Hasok Chang has also made use of incommensurability to interpret the late eighteenth century transition from a phlogiston-based to an oxygen-based chemistry. Chang argued that the proponents and the opponents of phlogiston had incommensurable methods, epistemic values and problems. One can thereby understand why the controversies around phlogiston were so difficult to resolve and why phlogiston chemistry persisted well into the nineteenth century. Thus, Chang's work shows the historiographical fruitfulness of incommensurability, a notion which Kuhn considered his main contribution to philosophy of science.[21] Conversely, Chang's engagement with late eighteenth and early nineteenth century chemistry has revealed some limitations of Kuhn's original understanding of incommensurability. In particular, Kuhn's overstated emphasis in *Structure* on conceptual incommensurability cannot accommodate the substantial continuity, at the level of chemical observations and manipulations, across the divide separating the phlogiston-based and the oxygen-based theories of combustion.[22]

Kuhn's approach to scientific discovery has also proved historiographically fruitful. As I pointed out above, Kuhn criticized the conception of scientific discoveries as temporally and spatially non-extended events and argued that it hindered historical understanding. Taking Kuhn's analysis as its point of departure, recent scholarship has further documented the complexity and extended character of particular scientific discoveries.[23] For instance, my own work on the discovery of the electron has been inspired by Kuhn's account of scientific discoveries as extended processes, involving the detection and most importantly, the gradual conceptualization of novel entities. This has led me to question the simple-minded attribution of the discovery of the electron exclusively to J. J. Thomson. As I have come to realize, Thomson's work must be situated within a complex landscape of converging developments, spanning from electrochemistry to spectroscopy, which collectively comprise the discovery of the electron.

To conclude, Kuhn's *Structure* got philosophical history of science off the ground by suggesting a rich repertoire of meta-scientific concepts for describing and interpreting the scientific past. Philosophically inclined historians of science have fruitfully framed their narratives and analyses of particular historical episodes in terms of those concepts. In my mind, this aspect of *Structure*'s legacy

[21] Chang (2012); cf. Collins, in this volume.
[22] According to Kuhn's later taxonomic approach to incommensurability, locally incommensurable "lexicons" may share a common observational ground.
[23] Arabatzis (2006); Caneva (2005); Dick (2013).

has outlasted Kuhn's famous grand narrative of scientific development and will continue to enrich the historiography of science for years to come.

Acknowledgements

I would like to thank Kostas Gavroglu, Gürol Irzik and Vasso Kindi for their helpful comments on an earlier version of this paper. This research has been co-financed by the European Union (European Social Fund – ESF) and Greek national funds through the Operational Program "Education and Lifelong Learning" of the National Strategic Reference Framework (NSRF) – Research Funding Program: THALIS – UOA.

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Chapter 14
On Reading Kuhn's *Black-Body Theory and the Quantum Discontinuity, 1894–1912*
Richard Staley

I am one of that small group of people for whom Thomas Kuhn's *Black-Body Theory* was much more important than *The Copernican Revolution* or *The Structure of Scientific Revolutions*. Although both his earlier and more widely-known books were set in my undergraduate courses in History and Philosophy of Science at the University of Melbourne in the 1980s, *Black-Body Theory* was clearly the most vital and engaging book amongst those I read while writing my doctoral dissertation at the University of Cambridge on the education and early career of the German physicist Max Born. It is very likely still the most dog eared and coffee stained book on my shelves, and I have often lent it out—since I have urged it on any serious students of the history of physics as the most fundamental and exciting study in the intellectual history of our field. Yet the book has been approached so differently over time that I can hope that a short and relatively informal note on just some of the ways it has been read will contribute to the broader aim of building a better understanding of the history of the history of science.[1]

With or Without *Structure*?

Black-Body Theory is one of those relatively small number of books that attracted such diverse responses that the controversy it aroused on its publication in 1979 helped bind the reviews it received to the history and understanding of the book itself (it shares this fate with Paul Forman's article on Weimar culture and Andrew Pickering's study of quarks, but not Gerald Holton's *Thematic Origins of Science* or Peter Galison's *Image and Logic*, all significant contributions).[2] As a result, quite a few of those who read Kuhn's book, particularly in the period through to 2000, would also have read several early responses—perhaps especially the reviews of Martin J. Klein, Abner Shimony and Trevor Pinch in *Isis*, and of Galison in the *British Journal for the Philosophy of Science* (Klein, Shimony, and

[1] See Staley (2013) for a study of the history and historiography of physics in the twentieth century.
[2] Forman (1971); Galison (1997); Hendry (1980); Holton (1973); Kuhn (1978); Pickering (1984).

Pinch 1979; Galison 1981). Even if they did not read any reviews, the article Kuhn published in 1984 and included as an afterword in the reprint of the volume in 1987 would have indicated some of the problems Kuhn faced convincing his contemporaries.[3]

What such reviews showed very clearly was that many were puzzled why a book that made such a major revisionist argument about the origins of quantum theory had so little to say about whether and how the conceptual apparatus that Kuhn had offered in *Structure* applied to his consideration of the work of Max Planck, Albert Einstein and others. The basic question of whether this was a revolution was answered in the affirmative, even as Kuhn's account threw into question its timing—in ways that he subtly illustrated in the first paragraph. This noted that Part One would describe the conception and gestation of the new quantum theory in Planck's work before 1906, while Part Two would offer an account of its birth and gestation in the work of others, and Part Three would consider Planck's response to their "apparently revolutionary reformulation" (Kuhn 1987, 3). That is surely a case of offering a back door entrance to what has normally played the starring role! Yet even while Kuhn so obviously urged a new chronology on the basis of his argument that Planck did not hold the concept of energy quantization usually attributed to him, Kuhn left unsaid what stood as the anomalies, crises and paradigms at issue, and whether these events illustrated incommensurability and gestalt switches. Kuhn's counter that the apparent "misfit" amongst his publications was in fact the best and most representative of his historical works would only have further puzzled many of those who raised questions about the issues that had been so central to Kuhn's philosophical approach to the history of science (Kuhn 1987, 349). Read in the light of *Structure*, then, the new book was a disappointment. Coupled with the fact that Kuhn had not been able to persuade Klein, the preeminent authority on Planck and the history of statistical mechanics, the controversy and discussion around its publication might even have given the impression that this book was destined for a short shelf life. Unlike the discussion surrounding Forman's article—which occasioned exchanges and debates on both the methodological questions around internal and external approaches to the history of science and on Forman's characterization of the period—many of those who responded to Kuhn's argument primarily urged Kuhn to clarify the implications of the book for his methodological stance towards scientific change. Rather than black-body radiation they were more interested in Kuhn's body of work.

[3] Kuhn (1987); the afterword had originally been published in Kuhn (1984).

Paradigms Lost and Found

As editor of *Isis*, Arnold Thackray found a pithy way of summing up this kind of impression by asking in the title of their review symposium whether this was "Paradigm Lost?" Yet when I read the book in the mid to late 1980s, it seemed immediately obvious that many of Kuhn's philosophical assumptions about the nature of revolutionary change were implicit in his approach (and especially his interest in relating technical details to changes in world views or paradigms writ large). I was hardly tempted to ask for a more detailed explication of the relations between *Structure* and *Black-Body Theory* either. One reason for this is surely the fact that by then both philosophically and sociologically oriented studies of the history of science were much less immediately engaged with Kuhn's *Structure* and distinctions between revolutionary change and normal science. Instead they debated realism and relativism around the work of Latour (especially) and Pickering's *Constructing Quarks* (in modern physics): it seemed everyone wanted to find their own way of saying that despite the underdetermination of theory by data, nature did constrain theoretical choice. But even more important than changing philosophical and sociological interests, I read Kuhn's *Black-Body Theory* against the background of other historical studies of turn of the century physics, and in that company it simply stood out.

As I began my dissertation on the early work of Max Born I naturally worked through earlier scholarship on the history of relativity and quantum theory. I can vividly remember being diverted by the engaging clarity of Klein's studies, reading some of them in the archive at the Staatsbibliothek Preussischer Kulturbesitz in Berlin when I should have been concentrating on Born's letters, for example. But it was Kuhn's *Black-Body Theory* that really stunned me. This was in part for the ambition of his claims and for the power of historical disclosure that he demonstrated, making apparent tensions that had previously simply been invisible to scientists and historians who had glossed over features of Planck's work that Kuhn showed demanded close attention. But it was also because he explained so much about the development and reception of quantum theory by a close investigation of the relations between interpretation and theoretical resources, as these had been developed through Planck's independent research, or by others such as Einstein, Ehrenfest and Jeans in the course of their education and research papers. For me, this was an explanatory intellectual sociology, and history of science at its most revelatory.

So *Black-Body Theory* was undoubtedly the most important work I read as a student, and I took it as an exemplar—a paradigm. The most important lesson I thought it conveyed was that individual variations in the approaches taken by different physicists could be rigorously related to their different educational backgrounds and fields of mathematical and physical expertise (a quite general lesson, applicable to many kinds of resources). Further, Kuhn showed that when physicists like Einstein, Ehrenfest and Lorentz brought their diverse expertise to bear on Planck's research, they not only interpreted and reinterpreted Planck's work—sometimes radically—they also abstracted it from its original context, thereby bringing it within the range of expertise of greater numbers of physicists (Staley 1992, chap. 6). Thus I saw Kuhn's account of Planck and responses to his work to have raised a similar question about authorship in quantum theory to those that were so evidently at issue in discussions of the relative contributions of Lorentz, Poincaré, Einstein, Minkowski and Hilbert in relativity (debating authorship was a major issue in the historiography of modern physics of the period). I thought Kuhn's thesis of the incommensurability of scientific paradigms had led historians to overemphasize the difficulties scientists exhibit in understanding different points of view in theoretical or experimental work. Yet whereas historians of relativity tended to relate acceptance of Einstein's contributions to proper or improper understanding, Kuhn related the character of understanding more thoroughly to training and research backgrounds and approached variation without the normative strictures customary in such approaches to relativity. In these respects his work was exemplary, methodologically valuable for its display of critical, investigative historical research. Had I looked back to *Structure*, I would have seen Kuhn's *Black-Body Theory* as a major contribution to the "historiographic revolution" he discerned in the study of science, but one that was most important as a practical paradigm.[4]

Later I saw particular limitations in Kuhn's approach. He had focused largely on theory and intellectual factors conveyed through technical education (formal or informal). But although he said so little about why black-body research was so important industrially, and we needed the work of David Cahan and Dieter Hoffman (taking Kangro's study further) to begin recognizing this, Kuhn did point very clearly to the importance of experimental work on the specific heat of solids, and Nernst's advocacy, for the propagation of Einstein's work. Thus, Kuhn indicated the significance of experiment and more materially focused histories, even if he had not written one himself (and one can see the same impetus in the diverse factors he saw to be important in his account of the "simultaneous discovery" of thermodynamics).[5] Similarly, the work of John Heilbron (drawing on Stanley Goldberg's studies of Planck) also integrated the closely focused technical history that Kuhn had offered with a more wide-ranging understanding of the cultural importance of absolutes in Planck's thinking (Goldberg 1976; Heilbron 1986). In this respect, too, Kuhn's work had offered an important point of origin

[4] For a brief description of this historiographical revolution, see Kuhn (1996, 3).

[5] Cahan (1989); Hoffmann (2001); Kangro (1976); Kuhn (1987, chap. 9), 1959).

for rigorous studies of the cultural significance of technical developments—even if with his focus on rewriting our understanding of "modern physics," Kuhn had been blind to just how novel and interesting it was to think of Planck's work as either a contribution to or departure from "classical physics." Thinking equally of industry, experiment and cultural studies, it would then be a mistake to focus too closely on Kuhn's own inclinations, and far better to see what his colleagues and students could make of his work. In this respect we should recognize that the work of one of the great theorists of the scientific community was as important for the concrete seeds it offered for quite different studies within our own community.

It did offer a stimulus to studies of quantum theory too, and as the publication of studies around the centenary of Planck's famous papers showed, over time Needell, Darrigol, Gearhart and later Badino have been able to offer a still more refined understanding of Planck's work than Kuhn had realized, often also taking pains to show why there had been such diverse responses to Kuhn's argument. Conceptually, these historians often found it important to note that Planck's caution towards the value of microphysical assumptions may also have led him to remain uncommitted about the nature of the energy elements that he invoked in following Boltzmann's work. Historiographically, their work has surely made reference to the original debate around Kuhn's study less necessary than it was previously, although appreciating its character will remain important to understanding the history of the history of science and several features of those earliest reviews have been confirmed by these later studies, as well as my own research.[6]

Along these lines I want to conclude by highlighting two points underlined by these early reviews. The first is the skepticism Pinch expressed about the central role of the dichotomy between quantum and classical theory in Kuhn's work, noting that Kuhn had assumed their incompatibility and perhaps incommensurability, without due historical analysis. The second is Galison's argument that Kuhn's search to exhibit a coherence in the work of pivotal scientists, both within their work and with prior paradigmatic problem solutions, might not be justified in the fluid situation characteristic of innovative work.[7] In my own research on the co-creation of classical and modern physics I used methods I could describe as Kuhnian to trace the earliest uses of concepts of classical in a careful chronological sweep through a wide variety of physical studies. Yet I found to my surprise that references to classical theory were absent from just those discussions of the

[6] Badino (2009); Darrigol (2001); Gearhart (2002); Needell (1980).

[7] Galison (1981, 83–84); Klein, Shimony and Pinch (1979, 438–439). Even when setting out the key elements of the historiographic revolution he saw in recent work in 1962, Kuhn had emphasized that analyzing earlier work from the viewpoint which gave it maximum internal coherence (and the closest fit to nature) was integral to the attempt to display the integrity of past science and trace "different, often less than cumulative, developmental lines for the sciences," Kuhn (1996, 3).

equipartition theorem in which both physics text books and the studies of historians like Klein and Kuhn would have led one to expect them to appear. As I worked chronologically forward from the 1890s through the 1900s and past the critical years following Einstein's work in 1905 and 1906—working systematically with research on mechanics, thermodynamics and other subjects—I began to suspect that the first occasion on which I would find our now customary use of the classical in conjunction with the equipartition theorem would occur in the papers and transcribed discussion of the Solvay Council of 1911. Had I reread Kuhn's *Black-Body Theory* at this point I would have had a further reason for betting on what turned out to be a good guess.

On Kuhn, Writing Backwards

In its highly autobiographical preface Kuhn tells us that he did not initially intend to undertake the project that led to *Black-Body Theory and the Quantum Discontinuity, 1894–1912*, in passages that also underline just how important his own experience of gestalt shifts and incommensurability was to the conduct of Kuhn's research. What Kuhn had wanted to write was a history of quantum conditions up to the inventions of Lande's vector model of the atom and Bohr's theory of the periodic table in 1922 and 1923 respectively, and the most unclear part of his plan was the appropriate date from which to begin. Looking for origins, Kuhn had then decided to reexamine Planck's work chronologically from 1895 in order to establish the first occasions on which physicists "asked about the nature of the restrictions placed by the quantum on the motion of systems more general than Planck's one-dimensional harmonic oscillator." That endeavor had ultimately given Kuhn the possibility of a radical rereading of Planck's key papers of 1900 and 1901; but I had not previously noted that Kuhn also tells us something more about his starting point. Revealingly, he found this search necessary because although he knew quantum conditions had been a major point of discussion at the first Solvay Congress in 1911, neither the proceedings of the congress nor the extant secondary literature provided clues to the origins of such questions (Kuhn 1987, vii). The comment indicates that even as Kuhn sought origins and embarked on a new account of Planck's introduction of energy elements, his reading of the Solvay Council proceedings had helped provide a concept of the classical that Kuhn unwittingly read back into that earlier period. While Kuhn warned us against a teleology that would seek the permanent contributions of an older science to our present advantage, in this instance he remained unaware just how deeply his own understanding of Planck's work had been shaped by Planck's innovative use of a concept of past, "classical" physics in 1911—one that melded previous, incompatible uses of the classical in the fields of mechanics and thermodynamics, and did so for present advantage.[8]

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[8] The point is argued in Staley (2008, chaps. 9 and 10).

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Chapter 15
Science, Politics, Economics and Kuhn's Paradigms

José M. Sánchez-Ron

Introduction

More than ever before, Thomas Kuhn's *The Structure of Scientific Revolutions* has opened the door of history of science to sociological considerations. Scientific revolutions, Kuhn taught us, do not start because normal science definitively fails—we can never be sure of that—but because a part of the scientific community becomes disillusioned with the dominant paradigm. In this sense, it is sociology, not logic, that explains the change of paradigm, sociology or the hope for better science in the future. Kuhn wrote:

> Paradigm debates are not really about relative problem-solving ability, though for good reasons they are usually couched in those terms. Instead, the issue is which paradigm should in the future guide research on problems many of which neither competitor can yet claim to resolve completely. A decision between alternate ways of practicing science is called for, and in the circumstances that decision must be based less on past achievements than on future promise. (Kuhn 2012, 156)

However, if we talk about "future promises," then we enter a world inhabited by more than experiments, data and theories; we enter in a world in which scientific expectations, as well as political decisions and how the public views science (that is, "cultural values"), affect the directions that scientific research will take in the future.[1]

[1] Prominent among those who have illuminated some of the external influences in the development of science is Paul Forman, especially in two works; in his classic "Weimar Culture and Quantum Mechanics," he argues that the crisis that permeated Germany after its defeat in World War I led a number of distinguished physicists and mathematicians to reject or limit the validity of causality in physics, and to incorporate acausality in the interpretation of quantum mechanics. Later on, in 1987, he showed that the military funding of research during the Cold War affected the direction of research carried out by physicists in the United States. Paul Forman, "Weimar Culture, Causality, and Quantum Theory, 1918–1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environ-

Kuhn, Politics and History as a Way to Act in the World

As Mary Jo Nye pointed out, "a political view is not explicit in Kuhn's writings. He did not set out on a political mission to become a public intellectual and tried to avoid political readings of his work" (Nye 2011, 250). Of course, we should not blame him, because political considerations were not present in his book; most of history of science is pursued along the same lines. However, such intentions are rather strange, for did Kuhn not teach us that we must also look further than mere science, and that social elements (perceptions, beliefs, hopes and so on) are in fact, very important? Did he not prioritize history over logic? When we ask ourselves such questions, we are led to think about the purpose of history—not only history of science—and the moral obligations, if any, of historians. "The responsibility, the obligation, of a historian is to tell the truth as he sees it, the whole truth and nothing but the truth. He should not allow himself to be a propagandist or to be used by propagandists. This is the great temptation and the great danger of history as a profession because history is, after all, the case that one makes for almost any political case"; so wrote Bernard Lewis, the reputed historian of the Middle East, in his memoirs. Of course, the "history" he refers to may be any history: political history, economic history, military history or history of science (Lewis and Churchill 2012, 140).

Yes, history can be used in perverse ways, but even so, there are other scenarios besides the purely intellectual one of reconstructing the past for its own sake. Almost immediately after writing the previous sentence, Lewis in fact stated: "By the study of history we can arrive at some better understanding of the nature of the human predicament in this universe; of what we can do and what we can't do; of where we are and, with luck, where we are going. History may serve us as a guide or as a teacher" (Lewis and Churchill 2012, 142).

Even I understand that history of science justifies itself independently of any practical considerations, I am also sympathetic to the well-known idea the Italian philosopher, critic and educator, Benedetto Croce (1866–1952), puts forward in his book, *La storia come pensiero e come azione* (1938). He wrote:

> Historical culture has for its object the keeping alive of the consciousness which human society has of its own past, that of its present, that is, of itself, and to furnish it with what is always required in the choice of the paths it is to follow, and to keep in readiness for it whatever may be useful in this way, in the future. (Croce 1949, 199)

ment," (1971), "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940–1960", (1987). For comments on Forman's work, see Carson, Kojevnikov and Trischler (2011); Schweber (2014).

In a similar vein, in his *Autobiographical Reflections*, John Stachel wrote:

> But one must not only continue to learn, to guard against all rigidity of belief, all dogmatism. One must continue to act in the world, not to be paralyzed by the knowledge that all opinions are fallible. We must act to change the world, our personal world, our social world, our intellectual world, guided by the best current beliefs, but always ready to change these in the face of new information. Our knowledge may fallible, but it is corrigible! (Stachel 2003, xiv)

In a different context but with a similar possible reading, Paul Forman wrote:

> [M]ore and more it is coming to be accepted that in social and humanistic studies, and particularly on history, the scholar's recognition of significance […] is inseparable from judgments of good and bad, desirable and undesirable. (Forman 1991b, 72)

And here Forman quotes Louis Galambos: "Moral judgments […] have always characterized the best historical scholarship" (Galambos 1983, 493).

I do not know if Galambos' dictum is true. Regardless of whether or not it is true that "moral judgments have always characterized the best historical scholarship," I believe that we, as historians of science, should consider intervening in the present as part of our profession, a profession that does not limit itself to looking at the past for its own sake. Scientists—and many professionals from other disciplines—claim, with evident reasons, that their profession is useful to society. There is no reason why historians of science should not try to show that they, too, are useful to society besides the obvious and of course important achievement of helping to understand the scientific past. Actually, such a claim has been put forward before: Forman noted that Hunter Dupree, the highly respected historian of American science, was not reluctant to offer history-based advice on science policy. In addition, Lewis Pyenson pointed out that when our discipline was founded, one of its major goals was supposed to be "clarity to act in the present on the basis of an understanding of the past."[2]

History of Science, Public Opinion and Newspapers

The question, or at least one of them, is how to intervene in the present. Here, I want to argue that one way historians of science can act in the present world is to participate in public discussions by writing in newspapers. Some scientists, especially physicists, have been doing this for a long time, even creating journals

[2] See Forman (1991a); Pyenson (1989). In this regard, see Brush (1995, 223).

(*Bulletin of the Atomic Scientists*, established in 1947, for example). Clearly, this is thought of as a way of influencing both public opinion and political decisions.[3]

For quite a number of years, I have been using history of science to write articles of opinion in what is considered to be Spain's (and Hispanic America's) main newspaper, *El País*. I have attempted to use specific episodes taken from the history of science in order to defend different points of view related to questions of present social relevance. Let me give some examples: On 19 February 2011, I published an article entitled "Juventud, maldito Tesoro" (Youth, Damned Treasure). Here I discussed the terrible present unemployment figures among Spaniards—between 40 and 50 percent, for youths and young adults. This implies that the best of them must go abroad to find work. I was interested particularly in the case of young scientists who especially suffer from the present situation. My argument was that young Spanish scientists, the best of them, should be given the opportunity to lead a great project. I mentioned in particular the creation of the new, well-endowed National Centre of Cardiological Illnesses, whose leadership was offered to an eminent, though rather old cardiologist who had carried out his career in the United States at Mount Sinai Hospital in New York. To defend my point, I explained that when in 1884 Cambridge University searched for a replacement for Lord Rayleigh as director of the Cavendish Laboratory, the position went to the young physicist, J. J. Thomson. Thomson was far from having the scientific credentials of the first two directors of the Cavendish Laboratory, James Clerk Maxwell and Lord Rayleigh, but over the course of his career, he would bring years of glory to the Institute, to Cambridge University and to England.[4] What is difficult for institutions, I argued, is to identify the genius when it is not yet fully manifested; to give young scientists the opportunities and facilities to put forward all their creative abilities, something that in general it is out of reach for older, more established scientists.

My second example is an article I published on 1 February 2009, the year of the 150th anniversary of the publication of Charles Darwin's *Origin of the Species*, under the title "El ejemplo y las lecciones de Darwin" (The Example and Lessons

[3] "Eugene Rabinowitch intended," wrote Patrick David Slaney, "*Bulletin of the Atomic Scientists* to be an institution of scientific internationalism in the early Cold War. He hoped that the Bulletin might serve, faute de mieux, as a site of international contact that would allow his vision of the scientific life to contribute to peace and stability in the shadow of the atomic bomb" Slaney (2012, 114). Scientists also used books as a way of defending their ideas and of influencing political decisions. A splendid example in this sense is Steven Weinberg's *Dreams of a Final Theory: The Search for the Fundamental Laws of Nature* (1993), which was clearly intended as a defense of the construction of the Superconducting Super Collider accelerator.

[4] "In December 1884, I was," wrote J. J. Thomson, "to my great surprise and I think to that of everyone else, chosen as [Rayleigh's] successor. I remember hearing at the time that a well-known college tutor had expressed the opinion that things had come to a pretty pass in the university when mere boys were made professors" Thomson (1936, 98).

of Darwin). My purpose was not just to remind *El País* readers of the anniversary and celebrations that were going on throughout the world in that year (although of course I took this opportunity to explain the importance of Darwin's book). I wanted to criticize the new presentation of creationism—the so-called "Intelligent Design"—as well as a declaration by Queen Sophia of Spain, who had said that: "Religion should be taught to children at schools, at least until a certain age: children need an explanation of the origin of world and of life." Other instances I have used as examples include Einstein's views on the Jewish problem to illustrate my views on the Israeli-Palestinian conflict, and the decline in Robert Oppenheimer's scientific production when he became an administrator and leader of scientific projects (Sanchez-Ron 2002, 2004).

My own experience is that these newspaper articles are well received by the public, which leads to another positive consequence: they serve to socially promote our discipline.[5] Emphasizing and using the history of science in such a way fits well with the goals Kostas Gavroglu and Jürgen Renn mention in the introduction to their volume in honor of Sam Schweber, *Positioning the History of Science*: "After more than a century, the history of science is still in search of a wider audience [...] In any case, the history of science today has turned out to be dramatically different from what its founding fathers imagined" (Gavroglu and Renn 2007, 3).

Kuhn, Political Revolutions and the Search for New Political Paradigms

As I pointed out earlier, while a political view is not explicit in Kuhn's writings, in *The Structure* he refers to the parallels between scientific and political revolutions:

> Political revolutions are inaugurated by a growing sense, often restricted to a segment of the political community, that existing institutions have ceased adequately to meet the problems posed by an environment that they have created. In much the same way, scientific revolutions are inaugurated by a growing sense, again often restricted to a narrow subdivision of the scientific community, that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself had previously led the way. In both political and scientific development the sense of malfunction that can lead to crisis is prerequisite to revolution [...]

[5] In the troubled and changing times we are living in, throughout the whole world, it might be a good idea to consider producing a collective monograph—this is another of my proposals here—whose chapters deal with some of the main problems that the world is currently facing, chapters which use some episodes taken from history of science.

> This genetic aspect of the parallel between political and scientific development should no longer be open to doubt. The parallel has, however, a second and more profound aspect upon which the significance of the first depends. Political revolutions aim to change political institutions in ways that those institutions themselves prohibit. Their success therefore necessitates the partial relinquishment of one set of institutions on favor of another, and in the interim, society is not fully governed by institutions at all. Initially it is crisis alone that attenuates the role of political institutions as we have already seen it attenuate to role of paradigms. In increasing numbers individuals become increasingly estranged from political life and behave more and more eccentrically within it. Then, as the crisis deepens, many of these individuals commit themselves to some concrete proposal for the reconstruction of society in a new institutional framework. At that point the society is divided into compelling camps or parties, one seeking to defend the old institutional constellation, the others seeking to institute some new one. And, once that polarization has occurred, *political recourse fails*. Because they differ about the institutional matrix within which political change is to be achieved and evaluated, because they acknowledge no supra-institutional framework for the adjudication of revolutionary difference, the parties to a revolutionary conflict must finally resort to the techniques of mass persuasion, often including force. Though revolutions have had a vital role in the evolution of political institutions, that role depends upon their being partially extrapolitical or extrainstitutional events. (Kuhn 2012, 92–94)

Suggestive as these ideas are, Kuhn did not try to develop such well-founded words about *political* revolutions. As is well known, *The Structure* is limited only to *scientific* revolutions; not even *technological* revolutions—which, by the way, may give rise to sociopolitical revolutions—were considered.[6] Nevertheless, five decades after the publication of *The Structure*, we find that the political situation in some parts of the world fit quite well with Kuhn's schema. I am referring to the protests that took place in the last few years in countries like Spain, Greece and Portugal, and even though they are not similar, those in Tunisia and Egypt. Especially in the case of the southern European countries, the masses that gathered asked for radical changes in the political systems that direct their countries. Reporting on the manifestations that took place in Spanish cities like

[6] "Kuhn is mainly silent on the matter of the pursuit of science for practical applications," Nye (2011, 250).

Madrid, Seville, Granada and Valencia, Elizabeth Flock of the *The Washington Post* reported on May 18, 2011 that "many demonstrators referred to the protests as a 'Spanish Revolution'." The protests were in fact not limited to southern Europe. The Spanish example, also referred to as the *15-M Movement* (*Movimiento 15-M*; *M* standing for "May"), or the *Indignants Movement* (*Movimiento de los Indignados*) crossed the Atlantic and arriving in the United States, first in New York (September 2011), with the denominated "Occupy Wall Street" movement, and later reaching Chicago, Los Angeles and Seattle. As if the time was ripe, in 2010 Stéphane Frédéric Hessel, a diplomat and writer, had published a booklet *Indignez-vous!*, which became a bestseller, selling 3.5 million copies worldwide and translated into many languages, from Swedish, Greek, Hebrew and Hungarian to English, Spanish, Italian, German, Portuguese and Japanese. *Indignez-vous!* provided, so to say, ideological support for the first "indignants," the Spanish *indignados*.

To these national difficulties and reactions, and of more far-reaching consequences, there are the changes taking place worldwide, changes related to the emergence of new world powers, (China above all), and to the technological revolution that has emerged from the digital world.[7] Europe is becoming aware that it must renounce the "Enlightenment spirit"—a spirit continued in what is called Welfare State, with health and educational services available to all its citizens—which seems to have guided Europe's history for the last two centuries. Confronted with the limitations they are increasingly experiencing via privatizations, European citizens are feeding the ranks of the *indignants*, who are searching for a new political order, new institutions and new systems of representation. To achieve this, they are refusing to participate in well-established institutions, such as parliaments or political parties. We can say that "old" and "new" politics are incommensurable. And at this point enters Kuhn and his paradigms.

In an article published in April 2012, the prestigious journalist Juan Luis Cebrián wrote: "Emphasis must be placed in that we are not only confronted with a crisis, but with a structural change, a new paradigm whose foundation is the loss of influence and prestige of Occident" (Cebrián 2012).[8] Indeed, the present social situation can be accommodated quite well to the schema Kuhn presented in *The Structure*. It seems that the political paradigm in which many Europeans lived throughout the last century or so is facing an increasing number of anomalies.

[7]Representative of the changes that the digital revolution are introducing is what Tina Brown, editor in chief of *Newsweek* wrote about in what was announced as the last print issue of this weekly journal (December 31, 2012): "This is not a conventional magazine, or a hidebound place. It is in that spirit that we're making our latest, momentous change, embracing a digital medium that all our competitors will one day need to embrace with the same fervor. We are ahead the curve."

[8]Cebrián was the first director of *El País*; at present he is the president of PRISA, an audio visual and publisher of the large international group to which *El País* belongs.

"The system is obsolete," is one of the slogans of those who protested in Madrid. In other words, the period of so-called "normal science" seems to be reaching its end. The problem, of course, is finding a new paradigm.

The question here is not which characteristics the new paradigm should have, especially the paradigm sought by new generations, nor is it a question of the global or local, for instance, that would enable Europe to face Asia's threats to its economic and political power. The question I want to put forward here is whether it would be worthwhile considering if the ideas that Kuhn presented in *The Structure* can be extended to the present socio-political world, and which, if any, changes should be introduced in order to achieve them.

Economics as a Niche for Kuhn's Paradigms: Keynes and Hayek

These considerations take me to the following reflection: if, as seems to be the case, paradigms and "normal science" have not proved—apart from the attraction of *The Structure*—to be very fruitful in the realm of history of *science*, an interesting academic task would be to explore other fields. Leaving aside the one I have just mentioned, an interesting case study would be the "clash that defined modern economics," as Nicholas Wapshott recently characterized the confrontation between John Maynard Keynes and Friedrich von Hayek (Wapshott 2012).Such confrontation offers characteristics that remind us of something that Kuhn said in *The Structure*. Keynes' emphasis on the intervention of the state through fiscal and monetary policies in fighting economic recessions and depressions, and Hayek's emphasis that the free market produces a spontaneous order, can be compared to two alternative and conflicting paradigms that influence governmental decisions. That these are alternative economic paradigms was clear from the very beginning. Thus, after reading Hayek's *The Road to Serfdom*, Keynes wrote to Hayek on 28 June 1944:[9] "I should therefore conclude your theme rather differently. I should say that what we want is not no planning, or even less planning, indeed I should say that we almost certainly want more."

However, if we consider the Keynes-Hayek confrontation in the framework of a confrontation of paradigms, several questions arise. The first is the mentioned fact that they are two paradigms that coexist, something that does not fit too well with Kuhn's scheme. This fact was pointed out many years ago by Imre Lakatos and led him to propose the idea of competing scientific research programs. (The coexistence of what we might call the Newtonian and the Cartesian programs in the eighteenth century is a clear example in this sense. It is in such a framework that one can understand the work of Euler, a Cartesian as far his philosophical

[9] Quoted in Harrod (1951, 436).

views extend, who nevertheless contributed to the development of Newton's dynamics). "What [Kuhn] calls 'normal science'," wrote Lakatos, "is nothing but a research program that has achieved complete monopoly."[10] He immediately added something that fits very well with considering Keynes and Hayek's economic ideas as two rival research programs: "But, as a matter of fact, research programs have achieved complete monopoly only rarely and then only for relatively short periods." Indeed, only cursory knowledge of the economic history of twentieth and twenty-first centuries is needed to realize that Keynes and Hayek's theories have alternated in favor of politicians and economists. "Arguments over the competing claims to virtue of the free market and government, now rage as fiercely as they did in the 1930s. So who was right, Keynes or Hayek? [This] is a question that has divided economists and politicians for eighty years [and that still] mark the great divide between the ideas of liberals and conservatives to this day," wrote Wapshott (2012, xiv).[11]

There are, however, characteristics of Keynes and Hayek's contributions that justify considering them in the framework of Kuhn's theory, especially in the case of Keynes. I am referring to the special role that certain books play in the establishment of a new paradigm; books like Aristotle's *Physica*, Ptolemy's *Almagest*, Newton's *Principia* and *Opticks*, Franklin's *Electricity*, Lavoisier's *Chemistry* and Lyell's *Geology*, all of which were mentioned in *Structure* (Kuhn 2012, 10). In the case of Keynes, we have *The General Theory of Employment, Interest and Money* (1936), a book which that was perceived as revolutionary: "With publication of *The General Theory* in February 1936, Keynes fired the starting pistol for what came to be known as the Keynesian Revolution" (Wapshott 2012, 154).

Conclusion

As Michael Gordin and Erika Lorraine Milam pointed out when introducing a series of essays commemorating the golden anniversary of the publication of *The Structure of Scientific Revolutions*, "Kuhn's *Structure* has stuck with us. There are few books that one can continue to chew over decades after first reading, and even fewer that could generate such a colorful arrays of responses" (Gordin and Milam 2012, 478). However, in spite of such permanence, its relevance to historical studies is far from being clear. As Mario Biagioli explained:

[10] See Lakatos (1970 [1965], 155).

[11] In fact, Keynes himself viewed his book as revolutionary. "To George Bernard Shaw he wrote in January 1, 1935 that he believed he was 'writing a book on economic theory which will largely revolutionize—not I suppose at once but in the course of the next ten years—the way the world thinks about economic problems'," Nasar (2011, 328).

> While *Structure*'s philosophical ambition (though not the methodology) is still found in some science studies literature and among those who pursue 'historical epistemology,' it has always seemed irrelevant to most rank-and-file historians of science. Perhaps perceived irrelevance was masking the field's opposition to all things theoretical or its difficulties in tackling them, but, be that as it may, it was not uncommon to hear that, when he engaged in 'serious historical work' in the later *Black-Body Theory and the Quantum Discontinuity*, even Kuhn no longer sounded too Kuhnian.[12]
> Institutional trends only hastened the eclipsing of *Structure*'s role in the discipline. Following the near-complete failure to institutionally integrate the history and philosophy of science and the nearly complete migration of the history of science into history departments, the field either stopped asking philosophical questions altogether or started to frame them through the methodological it borrowed from other disciplines—disciplines it had rarely interacted with before, such as European sociology, cultural anthropology, cultural history, gender studies, and so on." (Biagioli 2012, 480)

While it might be true that most—but not all—historians of science have stopped asking philosophical questions, the sort of analysis that Kuhn introduced in *The Structure* nevertheless has a wider range of possible applications than history of science, or other rather academic fields.[13] In this paper, I have tried to show that Kuhn's ideas, the nature and dynamics of paradigms in particular, can be completed and tested in a series of scenarios that are very relevant in today's world, such as in the fields of politics and economy. More importantly, while Kuhn's model is being completed and tested, it can perhaps provide a good framework for understanding the world and the society in which we live, and in doing so, contribute to making the present more rational. Moreover, history of science is not necessarily foreign to such economic or political scenarios, for have historians of science not made great efforts in the recent decades to integrate their historical reconstructions precisely with political and economic considerations?

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[12] I can testify to Kuhn's indifference, after writing *Structure*, to the paradigm's narrative. I was present, sometime in the last two months of 1978 in the lecture that Kuhn delivered at the New York Academy of Sciences, when he presented his then new book, *Black-Body Theory and the Quantum Discontinuity, 1894–1921* (1978). His first words were: "I am Tom Kuhn, and I am not going to mention at all the word 'paradigm'."

[13] "As Kuhn's respondents have demonstrated, the notion of a paradigm shift—which *could be applied to a variety of vocational or intellectual phenomena*—is historically visible at only certain scales and under unfairly controlled conditions;" Gibbs (2012, 512), italics added by the author.

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Chapter 16
Abgesang on Kuhn's "Revolutions"
Ursula Klein

No other theory has caused more turbulence in the history and philosophy of science as Thomas S. Kuhn's theory of scientific revolutions. What has become of this theory around half a century after its publication? What does recent historiography of science have to say about Kuhn's concept of scientific revolution? After a brief overview of Kuhn's theory, I discuss distinct aspects of it, including the concepts of "structure" and "revolution."

According to Kuhn, the development of the natural sciences does not follow a linear course. It is not a continuous, cumulative process in which new knowledge is simply added to the old, so that the stock of knowledge would permanently grow and become ever more validated and reliable. Rather, a look at the history of the sciences shows that long phases of cumulative knowledge production are followed by substantial restructuring processes, in which objects of inquiry previously believed to be especially important are called into question, scientific methods, values and ways of argumentation are partially discarded, and old theories are replaced by new ones. Kuhn calls these drastic changes "revolutions," drawing an explicit parallel between scientific and political or social revolutions (Kuhn 1970, 92–94). Scientific revolutions, accordingly, lead not only to profound breaks with existing scientific traditions, but also take place in a relatively short period of one or two generations, or more precisely, a span of no more than 20 to 40 years.

When Kuhn published his theory in 1962, he was met with vehement criticism from philosophers of science. As is well known, most Anglo-Saxon philosophers of science during this time took a normative, strongly idealized view of scientific rationality, which clashed with Kuhn's understanding of how scientists accept scientific innovations. Kuhn argued that the acceptance of revolutions always presupposes scientists' willingness to change their perspective. The willingness to accept a new theory along with new research objects, methods, ways of argumentation and standards of evaluation, he pointed out, is attained less through rational judgment than through familiarization with new views in the context of scientific education. As this argument challenged the philosophical ideal of rationality, it does not come as a surprise that analytical philosophers' counterreaction was correspondingly emphatic.

The historians of science of the 1960s and 1970s were considerably more welcoming to Kuhn's theory. The argument that the long history of the sciences included repeated revolutionary cataclysms was by no means a novelty for them. The episodes of scientific change linked with "great scientists" such as Copernicus, Galileo, Newton, Lavoisier, Darwin, Planck or Einstein had been designated as revolutions long before Kuhn. To name just a few examples: in 1773 the French chemist Antoine-Laurent Lavoisier claimed that his research would trigger a "revolution" in chemistry. Charles Darwin stated in 1859 that Charles Lyell had started a "revolution" in geology, and that he himself would cause a "revolution" in natural history.[1] For the leading historians of science of the 1950s, revolutionary breaks were among the topics of high interest. The French historian Alexandre Koyré wrote in 1943 that the conceptual changes in the sciences of the late sixteenth and seventeenth centuries represented "the most profound revolution achieved or suffered by the human mind" since antiquity. Several years later, the English historian Herbert Butterfield claimed that the Scientific Revolution of the seventeenth century "outshines everything since the rise of Christianity and reduces the Renaissance and Reformation to the rank of mere episodes [...] [It is] the real origin both of the modern world and of the modern mentality."[2] In 1961, the American historian of science Henry Guerlac described the Chemical Revolution in the final third of the eighteenth century in similar words. Lavoisier had "refashioned the materials, the concepts, and even the language of chemistry so radically," he claimed, that "the science as we know it today seems almost to have been born with him" (Guerlac 1961, XIV).

Kuhn adopted this perspective from professional historians of science and aimed to further develop it theoretically. The very title of his major book, *The Structure of Scientific Revolutions* (my emphasis), indicates that he aspired to more than just the affirmation of a known argument in the history of science. But why "structure"?

"Structure"

Kuhn did not only advance the thesis that radical change had taken place repeatedly in the history of the sciences—he also developed more precise ideas about the *what* and *how* of these processes. Concerning the latter, he argued that scientific developments always take place according to the same scheme

[1] See Cohen (1985, 4).
[2] Quoted after Shapin (1996, 1–2).

or pattern, in other words they exhibit a universal structure.[3] His model of the structure of the long-term development of the sciences is well known and strikingly simple. It can be summarized as follows:

Normal science A1 → anomaly → crisis → revolution → normal science A2.

According to Kuhn, "normal science" constitutes the longest phase of development in any particular science. During this phase, empirical knowledge is expanded, and theories, instruments and methods are elaborated and refined, yielding an accumulation of knowledge. An unexpected discovery, however, constitutes an "anomaly," which is typically followed by a "crisis," wherein scientists encounter serious obstacles in attempts to integrate the discovery into the existing system of knowledge. And a "crisis" generally leads to a "scientific revolution," which results in a new form of the normal science at stake.[4]

Clearly, with respect to the long-term development of a science, the meaning of "structure" is well defined here. Suffice to add that this concept implies a thoroughly internalist understanding of scientific change in history. While Kuhn conceded that social factors could exert a certain influence on the development of sciences, he believed that their impact was so marginal that it could be disregarded in his construction of a historical theory.[5] Less simple, however, is the question of what "structure" means with respect to the revolutionary event itself.

Social and political revolutions affect the power structure of a society and the institutions that protect and perpetuate it. Parallel to this, one might first ask what, according to Kuhn, is the central objective of a scientific revolution? In *Structure*, Kuhn answers this question with his concept of paradigm. In all scientific revolutions, a new paradigm replaces an already existing one. As has been repeatedly shown, Kuhn's concept of paradigm is not precisely defined. Its core element is a scientific theory, but Kuhn also argues that additional elements are included, some of which remain unarticulated and are learned only during the process of scientific socialization.[6] Scientists always orient their teaching and research on

[3] On this see also Hoyningen-Huene (1989, 34–37).

[4] In the first edition of his book, Kuhn observed: "all crises close with the emergence of a new candidate for paradigm"; Kuhn (1962, 84). In other words, he claimed that crises are always resolved by a revolution. In the second edition published in 1970, by contrast, he allows three possibilities for terminating a crisis: the normal science can ultimately find a way to integrate the anomaly; the anomaly can be declared irresolvable for the time being and its solution postponed; or a new paradigm can be introduced in the context of a revolution; Kuhn (Kuhn 1970, 84).

[5] Of the Copernican Revolution, for instance, he writes that it was also triggered by "the social pressure for calendar reform." Yet he immediately adds that issues of this kind were "out of bounds" for his essay, which can only mean that he felt justified in neglecting them in his theory; Kuhn (1970, 69).

[6] For further details see Hoyningen-Huene (1989, 133–143).

a set of rules, values, standards and know-how, which are difficult to disentangle and are taken as given within a scientific community. According to Kuhn, this orientation knowledge and set of rules is an important part of a paradigm, which is also affected in a scientific revolution.

Let us now address the *how* question along with the meaning of "structure" with respect to the revolutionary event itself. As Kuhn defined scientific theory as the core element of a paradigm, it would be consistent to argue that the major event in a scientific revolution is the introduction of a new scientific theory. Approaches to new theories, Kuhn observes, are already worked out during a "crisis" and subjected to controversial discussion, but it is not until the phase of the revolution that the decisive step is taken toward elaborating a new theory. How does this happen?

At this critical point of his theory, Kuhn turns to psychology. Answering the question of how a new theory is formulated, he points out, "demand[s] the competence of a psychologist even more than that of the historian" (Kuhn 1970, 86). This does not prevent him from seeking his own answer. Having discussed previous borrowings from Gestalt psychology (1970, 85), he first reminds his readers that the scientists themselves "often speak of the 'scales falling from the eyes' or of the 'lighting flash' that 'inundates' a previously obscure puzzle." "On other occasion," he continues, "the relevant illumination comes in sleep," to further state that it is "flashes of intuition through which a new paradigm is born" (1970, 122f.). The most revealing and astonishing formulation, however, is the following: Crises, Kuhn states, "are terminated, not by deliberation and interpretation, but by a relatively sudden and unstructured event like the gestalt switch." (1970, 122, my emphasis).

Was it not Kuhn's own intention to explain to us the "structure" of scientific revolutions? Alas, his theory ends with explaining the construction of a new theory as a mental event *sui generis*, which allows neither conceptual analysis nor displays structural features. With this approach, Kuhn comes dangerously close to both the analytical philosophy of science, of which he was otherwise so critical, and to the traditional historiography of science. Clearly, only individuals have "flashes of intuition." When it comes to explaining theoretical novelty in the history of sciences, what counts, according to Kuhn, are not explorative work by means of communal theoretical tools, but the individual intuitions of the great men of science.[7]

Let us now turn to some additional aspects of Kuhn's concept of scientific revolutions, beginning with the relation between continuity and discontinuity. What and how much, in Kuhn's view, remains preserved in a scientific revolution—and, regarded from a broader perspective, flows into a continuous trajec-

[7] On theoretical tools of scientific communities, see Kaiser (2005); Klein (2003).

tory of scientific change over time? And how much is discarded? There are many formulations in Kuhn's *Structure* that suggest he understood a scientific revolution to be a radical fissure in an existing scientific practice, or a break from an existing tradition. This is also indicated by his discussion of scientific revolutions as "changes of world view." "It is rather as if the professional community had been suddenly transported to another planet where familiar objects are seen in a different light and are joined by unfamiliar ones as well," Kuhn drastically states (1970, 111). On the other hand, his *Structure* also includes statements that allow the conclusion that in scientific revolutions large parts of knowledge and familiar practices remain intact. Kuhn never ventured so far as to argue that a break with a scientific tradition would affect the disciplinary boundaries themselves. For instance, he does not claim that Lavoisier's Chemical Revolution made all previous talk of "chemistry" obsolete, or that Einstein reinvented physics as a discipline.

In the final chapter of *Structure*, which bears the paradoxical title "Progress through Revolutions," Kuhn tackles a question that is intimately connected with the problem of continuity and discontinuity: what about our intuition about the progress of science? "Why is progress a perquisite reserved almost exclusively for the activities we call science?" Kuhn asks. "Why should the enterprise sketched above move steadily ahead in ways that, say, art, political theory, or philosophy do not?" (1970, 160). These are vexing questions for him that should no longer exist on the basis of the theory he outlined beforehand. Clearly, they served to fend off all-too radical consequences of his theory. Yet, their theoretical costs are just as unmistakable.

First, in the context of his considerations about progress in the history of science, Kuhn suddenly feels compelled to speak of a "continuing evolution" of the sciences (1970). Second, in the subsequent discussion about the issue, he comes to the general conclusion that scientific progress lies within a scientific community's capability to resolve problems across paradigm change. "The scientific community," he points out, "is a supremely efficient instrument for maximizing the number and precision of the problem solved through paradigm change." He further observes: "As a result, though new paradigms seldom ever possess all the capabilities of their predecessors, they usually preserve a great deal of the most concrete parts of scientific achievement and they always permit additional concrete problem-solutions besides" (1970, 169). This statement does not sound like the description of a revolutionary break from a tradition, or a sudden switch to a new world-view; rather, it highlights continuity. It raises another question—Had Kuhn not identified that what is recognized as a problem or an achievement as dependent on the paradigm of a scientific community? Kuhn fails to provide a compelling answer to this question.

In today's historiography of science, there is a broad consensus that scientific practice and the stocks of knowledge it produces are restructured again and again, and that such restructuring processes were, and still are, occasionally so profound that they yield new concepts, theories, methods, values, objects of research and sometimes even new research areas. Albert Einstein's theory of relativity revised the scope and truth claims of classical mechanics and electrodynamics, significantly shifting their importance for the overall discipline of physics; what was previously held to be absolutely true, basic knowledge now became special knowledge, valid only under specific framing conditions.[8] Darwin's theory of evolution fundamentally questioned the biological dogma of the constancy of species, and replaced it with a new view of the historical development of species. The chemistry of the eighteenth century used new concepts that were incompatible with alchemy, with the far-reaching consequence that many of the alchemists' questions were no longer considered to be legitimate objects of chemical research. Thus, from the perspective of history of science, Kuhn correctly pointed out that the historical change of the sciences entailed not only the accumulation of knowledge and the addition of new methods and standards to the existing ones, but that there are also processes of restructuring. However, apart from the problems discussed above, Kuhn's idea about the duration of scientific "revolutions" is highly questionable from the historians' perspective. As mentioned above, Kuhn's concept of scientific revolutions drew a parallel to social and political revolutions. Accordingly, scientific "revolutions" would take place within one or two generations, or in a maximum of twenty to forty years. On a timeline spanning several centuries, they would thus appear as punctuated events.

With regard to the so-called Copernican Revolution, historians of science have shown that there were doubts about Ptolemy's closed geocentric model of the cosmos long before Nicolaus Copernicus (1473–1543), and that it took another 100 years before Copernicus' heliocentric model was further developed by Johannes Kepler (1571–1630) into a modern model with elliptical planetary orbits, which further unified the different spheres of the universe.[9] Moreover, today many historians of science reject the more general assumption that a big Scientific Revolution took place in the seventeenth century. As numerous empirical studies have shown, shifts in the meanings of concepts, abstraction and mathematical representation and emphasis on experimentation had already begun in the late Middle Ages. Step by step, this created the prerequisites for the works of Copernicus, Galileo, Kepler and Newton. Upon in-depth historical research, what at first glance appears to be the exclusive revolutionary work of scientific titans like Galileo and Newton turns out to be the final, consequent step in a long

[8] See Renn (2006).
[9] See Boner (2013); Krafft (1997); Zinner 1988).

restructuring process, albeit a creative one that was certainly not taken by anyone.[10]

Likewise, the changes in chemistry in the final third of the eighteenth century, which went down in history as Lavoisier's Chemical Revolution, were the consequence of processes that lasted more than a century. The restructuring activities during the transition from pre-modern alchemy to early modern chemistry were so complex that there is still no agreement among historians of chemistry concerning the questions of which parts of alchemy/chemistry were involved and how to identify the beginning and end of these processes. The phenomenon of chemists shifting away from alchemistic philosophies of substances and transmutation, and turning towards the early modern conceptual system of chemical compound, composition, analysis and synthesis was a gradual process that had already begun in the final third of the seventeenth century. Around the mid-eighteenth century the majority of chemists were using the chemical concepts and analytical methods, which in Kuhn's day were attributed to Lavoisier. Their quantitative chemical analysis, for instance, presumed the conservation of mass and balanced the mass of the substance to be analyzed with the sum of the masses of its components. Yet for a long time these assumptions were considered a distinguishing feature of Lavoisier's chemistry.[11]

Similar to Galileo and Newton, Lavoisier, too, merely drew the decisive theoretical consequences from previous research results and existing problems. His replacement of phlogiston theory with the theory of oxygen and hydrogen, for instance, was doubtlessly a creative feat, yet it was based on numerous preparatory works by other chemists, and on the rigorous exploration of existing conceptual possibilities. Asking about the end of the restructuring processes in chemistry also raises difficulties. For instance, Lavoisier used a concept of chemical compounds that had already been introduced in the early eighteenth century and had long been used parallel to older conceptions about the generation and structure of substances. In this vein, "chemical affinity" was the main conceptual criterion for demarcating chemical compounds from mechanical mixtures of substances. The modern concept of a chemical compound, however, would also place the additional demand of constant proportions of the components. Yet this additional criterion was not introduced until decades later, around 1800, well after Lavoisier.

Similar considerations regarding the duration of restructuring processes are also valid for the so-called Darwinian revolution. Not only did Darwin build on the works of many botanists, zoologists and geologists, what is more, his theory was initially misunderstood as a teleological theory of development, according to which living beings constantly continue to perfect themselves and develop into

[10] See Damerow et al. (1992); Shapin (1996).
[11] See Klein (1994, 2015); Klein and Lefèvre (2007).

higher forms. The Darwinian theory of evolution as we know it was not accepted within the scientific community and thus could have hardly promoted a Darwinian revolution. Not until around 1930 was it perceived to be what it is, a theory that grants a constitutive role to accident, namely random mutations, in addition to selection by environmental factors, which determine the direction of evolution in tandem.[12]

All of these cases concern profound scientific changes, but these spanned considerably longer periods of time and involved significantly more scientists and generations of scientists than Kuhn postulates in his theory. The temporal boundaries of these processes, with the determination of a beginning and ending, always entail an arbitrary element, or something that is difficult to justify independent of the historians' interpretations and understanding. Should we opt, like Kuhn, to resort to analogies to social and political changes, the term "revolution" seems particularly unsuitable here. Political and social "revolutions" proceed swiftly, whereas most of the restructuring processes in the sciences proceed slowly and gradually, involving many generations of scientists.

What consequences do these considerations have for Kuhn's larger theory of scientific change in history, and for his concept of structure along with his phase model? Let us assume that Kuhn agreed with historians' objection to his concept of punctuated scientific revolutions. Assume he would accept that processes of restructuring in the sciences often span many generations or even centuries. His argument that the development of the sciences in history does not proceed only cumulatively and continuously, but also involves processes of restructuring and discarding, would then still be true. However, with this, the distinctive part of his theory, built around the concept of structure, would collapse. The assumption of gradual restructuring processes is incompatible with Kuhn's structural phase model, which clearly demarcates between normal science, anomaly, crisis and revolution. This is one reason why Kuhn's attempt to reveal a universal "structure" of scientific change in history has failed.

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[12] See Lefèvre (2009).

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Part 4: Reinterpreting Kuhn

Chapter 17
The Pendulum as a Social Institution: T. S. Kuhn and the Sociology of Science
David Bloor

Introduction

In the fullness of time, when the 'history of the history of science' is written, there are three themes which will surely find a place in the narrative covering the latter part of the twentieth century. The first is the impact on the history of science of Kuhn's *Structure of Scientific Revolutions* (Kuhn 1962, second, enlarged edition 1970). The second is the disputed relation between the history of science and two neighboring fields: the sociology of science and the philosophy of science. The third is the question of relativism and its relation to the history of science. The three themes are interconnected. Part of the problem of defining the impact of Kuhn's *Structure* derives from the question of whether he challenged the *status quo* by virtue of introducing new sociological insights, or because he introduced new philosophical assumptions, or new historiographical methods, e.g. methods that led to relativist conclusions about the status of knowledge.

A complicating factor that must be confronted by any future historian of the field is that the historical actors, by which I mean Kuhn and his contemporaries, have themselves offered answers to some of these questions. This raises the question of whether the self-awareness of the historical actors was accurate. Were the contested tendencies of the recent past correctly understood by those who enacted them and argued about them? For example, in some quarters Kuhn's book has been read as a work which forges a link between the history of science and the sociology of science, although Kuhn himself spoke out in the strongest terms against certain sociological accounts of science that were based on his work (Kuhn 1992, reprinted in Kuhn 2000, 105–120).

I shall come back to the question of relativism. For the moment I want to concentrate on the relation between historical and sociological approaches. To say that Kuhn's work provides a link between the history and sociology of science is to say very little until the character of the link is identified. Unfortunately there are superficial ways to make the connection as well as deeper and more penetrating links that might be discerned. The future historians of the history of science will be failing in their scholarly duty if they cannot sift the two and focus their analysis accordingly. This danger would arise if the future historians had a false stereotype of sociology in their minds (for example a stereotype in which a sociological explanation involved a denial of the role played by the material world). Such a stereotype would distort any answer then given to the question of whether Kuhn, as an historian who clearly respected science, was also engaged in a 'sociological' analysis of that enterprise.

In what follows I shall not try to anticipate the relevant parts of the future history of the history of science. I shall not try to write it in advance, but I shall offer some material that, I believe, should inform the thinking of those who, in future years, might chronicle today's activities. My argument will be that, despite his own protestations, Kuhn developed some profound sociological insights which were integral to his analysis of scientific knowledge. These insights, however, may not be immediately evident to an eye that has not been sensitized to pick them out.

In order to prepare the ground I need to lay out some basic ideas about the nature of social phenomena. This will make it easier to detect the sociological themes in Kuhn's thinking. In particular, I shall formulate a simple model of a social institution.[1] I shall then use this model to offer a reading of Kuhn's important 1961 paper "The Function of Measurement in the Modern Physical Sciences."[2] By making Kuhn's use of the idea of a social institution explicit I shall show how it is possible to appreciate more fully the coherence of the paper and, ultimately, the coherence of *The Structure of Scientific Revolutions*.

In setting out my argument in this way I shall be following the work of the Edinburgh sociologist Barry Barnes as he developed it in his 1983 book *T.S. Kuhn and the Social Sciences* (Barnes 1982, 1983; 2003). This is not a book about 'applying' Kuhn to the social sciences, in the sense of fitting sociology, or its history, into some pre-given 'Kuhnian' pattern. Rather, the book is about the sociological insights to be found in Kuhn's work itself, and the potential generality of those insights. Barnes' book is not as well known as it should be, but it would be a grave injustice if it were overlooked by the future writers of the history of the history of science. As well as seeking to offset this possibility I also want to

[1] An institution might be defined as a set of roles and statuses that are linked by conventions. The meaning of the words 'institution,' 'role,' 'status' and 'convention' differ from one another, but all of them designate aspects of social reality that exhibit an important, common feature. Furthermore, despite their differences, all of them describe a reality that is strangely intangible and elusive. For present purposes the differences between these terms is not of great importance. My aim is to explain the common feature they share and identify the source of the elusive quality of the social.

[2] Kuhn (1961), reprinted in: Kuhn (1977, 178–224). The page references are to the 1977 publication.

develop parts of Barnes' argument in more detail. I now move to a brief account of social institutions as a preliminary to the exposition of Kuhn.

Institutions

Sociologists, like anthropologists, use the word 'institution' in a broad sense. They talk of such things as the institution of marriage, the institution of law and the institution of money. The point of this usage is to draw attention to a range of important facts: for example that marriage involves, but goes beyond, the biological facts of co-habitation and child-birth; that criminality involves more than entering a person's house and exiting with the silver ware; and that money involves more than passing someone a metal disc when they pass you a loaf of bread. For simplicity let us stay with the latter example. Everyone knows that coins are more than metal discs. They are discs with a certain status and they operate within the broader institution of economic activity. But what is the added element? What makes a metal disc into a coin?

Stated in simplified terms the answer is that a disc becomes a coin by virtue of being regarded as a coin, treated as a coin, believed to be a coin, and being referred to as a coin. Let me summarize all these different orientations to the disc by saying that coins are coins because they are 'called' coins. The word 'called' can stand in for the rest. This analysis looks suspiciously circular but this is not really a defect; it will turn out to be a crucial feature of the model that is being developed.

In order to explore this circularity consider the question: When I refer to the disc as a coin, to what, exactly, am I referring? It will not be to the disc *qua* disc. Rather, I am referring to the status of the disc as a coin. But what is the status to which I am making reference? What sort of thing is a status? What reality corresponds to my words? We need an answer that articulates the informal idea that a coin is a coin if it is 'called' a coin. The answer is this: When I refer to a coin I am referring to other people's references to the disc as a coin. And what are these other people referring to? The answer must be that they too are referring to other people's references. This must culminate in a self-referential system. It follows that there is no reality independent of the discourse to which the discourse corresponds or refers. Each act of reference has an independent object, namely other people's acts of reference, but the system as a whole has no independent object. Rather, it has a self-referential character.

This analysis can be generalized to cover all other words that designate social reality. When a person follows a convention, what are they following? The answer is that they are following other peoples' following. They are doing this because other people do this, and they would not do it otherwise, but bringing into the story the motives and calculations that are at work does not alter the basis of the account. One can still detect the self-referential character of the motives and calculations that may be involved. No wonder that the basis of social ontology is difficult to grasp. There is nothing to grasp but the grasping. The concepts involved do not have an independent object of reference. They furnish their own object through self-reference.

It is important to appreciate that this theory does not reduce society to mere talk about talk. Recall, first, that the verbal emphasis on 'calling' something a coin was just a simplification. It stands duty for a range of responses that include non-verbal reactions to objects and people. Secondly, these verbal and non-verbal classifications have material consequences. Lack of anything that can be 'called' money doesn't just limit what can be said, it limits what can be eaten. The self-referential analysis is not, as it may at first appear, a thin and verbal account of social reality. Rather, it is an account which enables us to focus on the most basic processes that enter into the construction and maintenance of any form of social system.

To appreciate the potential of this analysis it may be of help if the process of self-reference is related to another mechanism which may be easier to grasp, namely, that of a self-fulfilling prophecy. The social reality associated with the use of what is called 'a coin' may be thought of as the product of a self-fulfilling prophecy. The existence of a currency is a reality brought about and maintained because, and in so far as, people believe in it. Similarly, to follow a convention is to add to the strength of the convention and increase the likelihood that others will conform.

The sociologist Robert Merton has drawn attention to the importance of self-fulfilling prophecies. He saw them at work in a range of phenomena such as racial, class and sexual prejudice. Belief in the inferiority of a sub-group will lead to its members being treated in hostile ways and this will result in further disadvantage and thus consolidate their inferior status. Coming closer to the example of the coin, Merton also illustrated the idea of self-fulfilling prophecies in the realm of finance. If a bank is believed to be unsound the depositors will withdraw their money and, whatever the initial state of affairs, the belief will be made true. The 'prophecy' of the bankruptcy is fulfilled (Merton 1957, 421–436).

Merton belonged to the school of American sociology called 'functionalism.' Functionalists try to explain social phenomena by identifying the 'functions' they perform. (Kuhn appears to have taken over their terminology in the title of some of his papers, including his paper on measurement.) Merton thought that the appeal to functions could explain the 'norms' and 'values' that, on this theory, move people to action. However, Merton disapproved of prejudice, and rumors that destroy banks, so he identified these phenomena as dysfunctional rather than functional. He thus confined the appeal to self-fulfilling prophecies to the explanation of social pathology. As a result, Merton overlooked the fact that these mechanisms can work in the other direction. A correct analysis of why a bank is sound will also contain an important element of self-fulfilling prophecy. His concentration on cases with undesirable outcomes led him to miss the generality of his own insight.[3]

If one gives the mechanism of the self-fulfilling prophecy the generality it deserves we can use it to see how the process of self-reference works over time. Appreciating the role of self-reinforcing processes adds motion to an idea that might otherwise seem static. It provides the dynamics of the system. The appeal to self-fulfilling prophecy thus helps to make the simplified model of self-reference a little less abstract and easier to relate to concrete historical material, for example, the sort of material that Kuhn assembles in his paper on "The Function of Measurement in Modern Physical Science."

The Criterion of Agreement

At the beginning of his paper on measurement Kuhn draws attention to the tables that often occur in scientific textbooks. These tables juxtapose a list of numerical predictions, derived from a scientific theory, with a list of measurements taken from experiments. Kuhn gives the (presumably hypothetical) example shown in table (17.1).

Theory	Experiment
1.414	1.418
1.732	1.725
2.236	2.237

Table 17.1: Kuhn's example of a table of results.

He asks the question: What are these tables for? What function do they perform? The answer might seem obvious. Surely, they are meant to show the reader that the theory is true or, at least, to show that the theory agrees with the facts. Kuhn, however, gives a subtly different answer. He says that the tables are there to show us what is meant, in this context, by 'agreement with the facts.' Everyone knows that agreement can never be absolute so, Kuhn concludes, the function of the table is best expressed by saying that it shows what counts as

[3] For a further discussion of this example, and further references, see Barnes (1983, 536–537).

'reasonable agreement.' The table, therefore, does not simply reflect a truth about nature; it also embodies, and speaks to, the scientific community's own response to nature. The table carries the message: '*This* is what the scientific community counts as reasonable agreement.'[4]

Kuhn's argument now takes a striking turn. Suppose we enquire into the *criterion* of 'reasonableness' that is in play. What is this criterion? Kuhn says that the answer can only be found in the tables themselves. We are forced to look at the tables themselves, says Kuhn, because there are no "consistently applicable external criteria." The criteria of agreement between fact and theory vary greatly from discipline to discipline and time to time. In addition, and crucially, these criteria are not external criteria, that is, external to the local practices of science itself. But, notes Kuhn, this has taken us around in a circle. As he puts it, "we have gone full circle" (p. 185). We look at the table and ask for the criterion that informed its construction, but to find that criterion we must refer back to the table itself. Kuhn's own formulation is as follows:

> I began by asking at least by implication, what characteristics the numbers of the table must exhibit if they are to be said to 'agree.' I now conclude that the only possible criterion is the mere fact that they appear, together with the theory from which they are derived, in a professionally accepted text. (p. 185)

Appearing as they do, in a table, he goes on, they cannot demonstrate anything but reasonable agreement.

> And even that they demonstrate only by tautology, since they alone provide the definition of 'reasonable agreement' that has been accepted by the profession. (p. 185)

Kuhn speaks of tautology and circularity but he could have expressed his insight by speaking of self-reference. What is being referred to when reference is made to the reasonable agreement of theory and experiment? The reference is to the phenomenon made visible on the pages of the book in which a certain relation

[4] Kuhn does not cite any actual instances of text-book tables (or graphs) though there can be no doubt that he was speaking on the basis of first-hand experience. His hypothetical example is sufficient for his argument, but it is easy to assemble a few, real-life examples, e.g.: Born (1923, 27), a table comparing calculated and observed wave-lengths of the Balmer series; Richtmyer, Kennard and Lauritsen (1955, 131), graphs comparing spectral-energy distributions predicted by the Rayleigh-Jeans formula, Planck's formula and Wien's formula with experimental data; Bleaney and Bleaney (1957, 521), a table comparing observed and calculated values of specific heats of some metals and (p. 536) a table comparing observed and calculated values of the Hall effect; Reid (1932, 206), a table of calculated and experimental values of the angle of zero lift for a range of wing-sections.

between fact and theory is presented as (i.e. is 'called') 'reasonable agreement.' The readers of the textbook correctly refer here to reasonable agreement because authoritative members of the scientific profession, represented through the text, refer to it as that. In referring to it as reasonable agreement the members of the scientific profession *make it* reasonable agreement. They have self-referentially created a vital piece of social reality, namely, the professional standard and criterion of agreement between fact and theory.

Measurement in Normal Science

I now want to follow Kuhn's argument a stage further. We have seen that he started his discussion with textbook science, but it is important not to miss the generality of the point he was making. The reader must not form the impression that the self-referential processes that he detected were a feature of textbooks alone—an artefact, perhaps, of the demands of pedagogy. For Kuhn, they are neither artefacts nor oddities: they are to be found in all the physical sciences that depend on refined measurement.

In his discussion of normal science Kuhn once again identifies the central role played by 'reasonable agreement.' The task of many, and perhaps most, scientists is that of refining and re-defining what may count as reasonable agreement. The objective of this work, he says:

> is, on the one hand, to improve the measure of 'reasonable agreement' characteristic of the theory in a given application and, on the other, to open up new areas of application and establish new measures of 'reasonable agreement' applicable to them. (p. 192)

Questions of reasonable agreement are therefore not confined to text-books or to science in its completed state; they are ubiquitous.[5]

There are, however, profound difficulties confronting scientists when they seek to make measurements that are both precise and meaningful. Refined measurements only make sense, and are only possible, says Kuhn, against a back-

[5] In their papers and reports scientists and technologists frequently provide a running commentary of evaluation regarding the degree of agreement between their predictions and their measurements. They will declare this result a good one and that result disappointing or puzzling. What analysis can be given to these streams of individual judgments? They can be thought of as 'performative utterances,' see Austin (1961). What Austin called the 'felicity' or the 'infelicity' of the utterance depends, in this case, on whether the judgments are deemed appropriate by the relevant community. This fact about their broader reception is, of course, known to those who are making the judgments. They are aware that, not just the theory under test, but also their own reputation, is on the line as they issue their personal responses to the results.

ground in which a stable, theoretical tradition has been achieved.[6] When such a tradition has been achieved, he argues, the general form of some of the basic laws will already have been guessed. Given these guesses, then refined measurements will start to make sense. This is because the guessed-at laws themselves are used in refining the instruments of measurement and in modifying the experimental techniques which generate the data. One again there is a sort of circularity at work.

Kuhn illustrates his claim by describing the difficulties encountered in finding the basic laws of chemical combination. Before Dalton, the empirical data about combining weights were complex and inconsistent. After Dalton's theory became known, instruments could be adjusted, and techniques pursued, with a sense of direction and purpose. This was possible because Dalton's theory was itself used to guide the adjustments and refinements. Kuhn sums up the situation and its resolution in an interesting way.

> Before Dalton's theory was announced, measurements did not give the same results. There are self-fulfilling prophesies in the physical as well as in the social sciences. (p. 196)

The mechanism of the self-fulfilling prophecy is at work in the normal science that creates the shared standards that eventually find their way into text-books.

Kuhn occasionally made reference to Merton's work so perhaps he invoked the idea of the self-fulfilling prophecy because he had read Merton on this subject. But whatever the origin of the remark, what Kuhn said about self-fulfilling prophecy is right. It is a pity he did not follow up his insight. He could have corrected Merton's one-sided tendency to see self-fulfilling mechanisms at work only when 'dysfunctional' social phenomena are to be explained. Kuhn certainly did not take himself to be describing anything 'dysfunctional.' He was invoking self-fulfilling prophecies to illuminate one of the great achievements of science. If Kuhn had made the point explicit he could have restored the symmetry that was missing from Merton's analysis, but central to his own approach.[7]

[6] Kuhn warns the reader against the assumption that science is a purely theory-driven or 'theory-first' enterprise. His claim is that theory is a precondition of measurement, not of experiment or experience in general.

[7] Kuhn refers to Merton in a footnote on p. xxi of *The Essential Tension*. In the footnote Kuhn seems to take exception to the criticisms of Merton's work put forward by sociologists of science, such as Barnes, who do not subscribe to the functionalist approach. See Barnes and Dolby (1970). In the light of his comments, I suspect that Kuhn did not have a clear apprehension of the difficulties of Merton's view or the extent to which his own work might provide the basis for criticizing the explanatory appeal to norms and values.

Revolutions that Change the World

In *The Structure of Scientific Revolutions* Kuhn argued that, in the course of a scientific revolution, the world does not change but, after a revolution, the scientist works in a different world. As it stands this is a logical contradiction, though I do not think it was a mere lapse. I assume it was a deliberate device to draw attention to the difficulty of conveying what he wanted to convey (Kuhn 1970, 121).

Should we wish to do so, it is easy to remove the contradiction. All that is required to restore consistency is to attach a different meaning to the word 'world' on the two occasions on which it occurs. Thus it might be said that, though the 'world-in-itself' does not change, the 'phenomenological world' of the scientist might change during a revolution (Hoyningen-Huene 1993, 32 ff.). All strategies for removing the contradiction must have this general form. They can be differentiated from one another by their capacity, or lack of capacity, to carry forward the deeper aspects of Kuhn's argument.

My suggestion is that the world that changes is the social world of the scientist and the world that does not change is the world of (non-social) nature. Recalling Kuhn's paper on the function of measurement, notice that the 'social world' of the scientist contains things such as the standards of reasonable agreement between theory and experiment. We are therefore not, primarily, dealing with phenomenological changes but with procedural and behavioral changes. The changes concern the standards that are sustained by the profession as social institutions. These are the new things that come into existence and, because they are social things rather than material things, they can be said, roughly, to exist because they are believed to exist. Here, perhaps, we have the cause of Kuhn's contradictions and hesitations. He was not equipped with the theory of social ontology that he needed to convey his meaning.[8]

Is this sociological reading consistent with the other things which Kuhn said about the changes that take place during a scientific revolution? For example, Kuhn described the change from Aristotle's mechanics to Galileo's mechanics by saying that, before this revolution, there were no pendulums—only swinging stones (Kuhn 1970, 121). How is this possible? On my sociological reading the change from a swinging stone to a pendulum must be understood as analogous to the change from a metal disc to a coin. A material object is accorded a certain status and is set in the context of an institutionalized pattern of activity. The swinging stone does not change but how the swinging stone is treated certainly does change. The change will be invisible if attention is focussed on the swinging stone itself. To see the change it is necessary to bring into the picture all the

[8]The sociological reading of 'world' does not rule out perceptual changes during a revolution, but I suspect that, if the phenomenology shifts, the shift will be a consequence of the prior social changes.

persons and activity around the swinging stone and attend to what these persons, collectively, do to, and with, the material object.

In the terms of the self-referential model of institutions it could be argued that, at the most basic level, a swinging stone becomes a pendulum because it is 'called' a pendulum. Can this be elaborated in a revealing way? It might be acceptable to call 'reasonable agreement' a social institution, but can it be acceptable to argue, by analogy, that a pendulum is an institution? The salient feature of the theory of social institutions that I have been using is that the discourse referring to social realities has a self-referring character. But references to pendulums are surely not self referential. They are examples of external reference, i.e. reference to an independent object. The analogy between the case of reasonable agreement and the pendulum, or between the coin and the pendulum, thus appears to break down. Is there any way to reconcile the pendulum as an institution and the pendulum as a material object? I think there is.

The Pendulum as an Institution

The place to begin is with the process of learning the meaning of the word 'pendulum.' It is plausible to assume that it is learned through 'ostensive definition,' that is, by a teacher pointing to examples and giving them their name. "*This* is what is called a pendulum." A small number of exposures to a limited range of cases will generally suffice to evoke a reasonable competence in recognizing the more familiar kinds of pendulum, for example, those in grandfather clocks. By starting my account with ostensive definition I hope to ensure that the subsequent analysis will in no way compromise the material reality of the pendulum. The starting point, then, is independent reference, not self-reference.[9]

There is, however, a question that must be confronted. Has enough been said to ensure that the relation between the word 'pendulum' and the independent object, the swinging stone, is really one of 'reference'? Given the story so far, do we have genuine reference, or merely some of the preconditions of reference? The answer is that we only have the preconditions. More is required. Genuine reference requires that it is possible to draw a distinction between correct and incorrect applications of a word and for the users of the word to be responsive to this distinction. This requirement may be expressed by saying that the 'normativity' of the concept must be explained. So far, in the sketch I have given, the pupils learning the word 'pendulum' have been shown examples and then (it seems) left to their own devices. Their future applications of the word would have

[9] Kuhn's own discussion of the priority of ostensive definition over verbally formulated definitions and statements of natural regularities is to be found in his "Second Thoughts on Paradigms," reprinted in *The Essential Tension*. The discussion occurs on pages 309–318.

to be guided by nothing but their subjective sense of similarity. Whatever seems to them to be a pendulum will be called a 'pendulum.' But 'subjective standards' are not really standards at all. Standards must be objective and external to the mind of the individual language user.

The answer to the problem of how to make provision for norms of correct usage, and hence make genuine reference possible, is implicit in the scenario of the teacher and pupil. The teacher provides the standards by correcting the pupil's subsequent attempts to apply the word. Of course, teachers here only stand in as the representatives of the society whose language they are transmitting. By precept and example, step-by-step, the teacher shapes the sense of similarity needed to confront new cases and to confront them in a way that co-ordinates usage with that of other persons. When the teachers have finished their work the sources of correction will come from other users in the course of subsequent interaction. But whether corrections come from teachers or other users there are limits to what can be conveyed. No two pendulums will be identical. All that teachers, or anyone else, can do is to sustain a shared sense of what counts as being reasonably similar to the 'paradigm cases' used when the word is taught.[10]

It will be clear that the present discussion is now proceeding along similar lines to the discussion of measurement in Kuhn's paper. Both deal with a form of 'reasonable agreement.' We can ask of the language teacher the question that Kuhn asked of the text-book writer: What is the criterion that is in play? By what criterion is the word 'pendulum' rightly applied? What are we speaking of when we speak of its rightness?

The answer that must be given is the one that Kuhn gave. There is no external criterion other than the authoritative practice of the community itself. The point can be conveyed by using the same words that Kuhn used, merely inserting 'pendulum' in the appropriate place. Thus: "I began by asking what characteristics the things called 'pendulums' must exhibit if they are to be said to 'agree' with the concept of pendulum. I now conclude that the only possible criterion is the mere fact that they appear, together with the word 'pendulum,' in a pro-

[10] I have used the expression 'paradigm cases.' This terminology was commonplace in philosophy before Kuhn introduced the word to refer to an exemplary scientific achievement. Oxford philosophers of the ordinary-language school developed what was called 'the paradigm-case argument.' The aim was to combat scepticism and relativism. If a sceptic doubted the existence of, say, free-will the response was to point to examples of behavior of the kind that could be used to define the concept of 'acting freely.' The claim was that the reality of free-will could not be doubted in any coherent way because these examples furnished the very meaning of the concept whose application was in question. Though not without interest, as an argument against relativism it is powerless. Different cultures can all employ the paradigm-case argument to suit their own ends, i.e. to introduce the concepts central to their own view of the world and (using the methods of Oxford philosophy) thereby 'prove' them to be unassailable. For criticisms of the paradigm-case argument see Ernest Gellner (1979, 52–59) and John Passmore (1961, 113ff.).

fessionally competent lesson for learning the language." Following Kuhn, the conclusion must be that "the examples alone provide the definition of 'reasonable similarity' that has been accepted by the community." Like Kuhn, when we try to locate the reality that embodies the crucial, normative component of discourse, we find "we have gone full circle." As before, I want to locate the reason for this circularity in the role played by self-reference. The thing to which we are referring, that is, the 'rightness' of any act of concept application, resides (like 'reasonable agreement') in the totality of other references, implicit and explicit, to this rightness.

It is now possible to reconcile the self-reference of the words that apply to social reality with the external-reference of words such as 'pendulum.' The link between the two is this: External reference requires that the meaning of a word incorporates a normative component. That normative component is a social component and, given its social nature, it must be sustained by, and consist in, processes of a self-referential kind. The apparent problem of 'reconciling' these two things was therefore an illusion. In reality, the full requirements of external reference depend on mechanisms of self-reference and could have no existence without them. But before I can go on to draw the conclusion I want to, a little more needs to be said about the normative apparatus surrounding the word 'pendulum.'[11]

The discriminations that differentiate right from wrong applications of 'pendulum' are not static. They do not reside solely in the guidance given by teachers to children on their early encounters with this class of worldly object. Historically the concept of the pendulum changed as these objects became the focus of scientific interest. The law of the pendulum was first guessed and then refined, and the behavior of the pendulum became the subject of increasingly precise measurement. Here again the story picks up the account given by Kuhn in his references to the history of mechanics. The norms governing the correct application

[11] I have argued that external reference depends on self-reference, but there is also dependence in the opposite direction, that is, self-reference depends on external-reference. If I call a disc a 'coin' because you call it a 'coin,' I must be able to respond to the physical phenomenon that constitutes the sound of the word 'coin.' But can both directions of dependency be real? Can this be possible without creating a problematic form of circularity? I think this apparent problem is no more than an artefact of the demands of exposition. It looks as if a temporal sequence is being identified where A must happen before B whilst also requiring that B must happen before A. In reality there is no such temporal sequence. It is not necessary to get certain self-referential processes up and running as a precondition for external reference while also demanding that external-reference is already in operation as a precondition of achieving the requisite degree of self-reference. The two forms of reference can and do arise alongside one another. Both are pre-figured in patterns of causal interaction and dependence i.e. the responsiveness of actors to the material environment and a simultaneous responsiveness of actors to one another. Both forms of responsiveness operate in unison and their interaction gives rise to the patterns that are later accorded the full, normatively informed, status of 'reference to' this or that aspect of the overall environment.

of the word 'pendulum' ceased to be confined to ones that could be followed on the basis of visual inspection alone. They come to include the accepted ways in which the object can be subject to mathematical analysis. Thus the correct application of 'pendulum' was now related to mechanical concepts such as 'force' and 'acceleration' as well as geometrical ideas such as 'circle' and 'cycloid' and, of course, engineering concepts such as 'escapement.' As Kuhn explains, the further elaboration of the concept also included an awareness of the all-important techniques of approximation that are applicable to it. There must be a shared understanding of the correct response to the ever-present discrepancies between the predicted behavior of an idealized, 'mathematical' pendulum, with its massless string, frictionless surroundings and constant temperature, and the real thing in the laboratory or the clock-smith's workshop.

I have now filled out the process of 'calling' something a pendulum and have arrived at an account that does more justice to the pendulum as an object of high scientific significance. I have also shown that the route to that status was exactly the one set out by Kuhn in his paper on the function of measurement. The pendulum is now an object that calls for immensely sophisticated behavior on the part of those who surround it, orient to it, observe it, refer to it, adjust it, modify it, measure it, experiment on it, complicate it, and use it as a resource for understanding other phenomena. Now we have 'the pendulum' as a veritable institution. And this designation in no way compromises the material reality of the object at the center of all this co-ordinated and conventionalized social activity.

The Vacuity of Absolute Truth

It would be possible to go further in teasing out the sociological thread that runs through Kuhn's argument in his paper about measurement. I have mainly concentrated on the theme of 'reasonable agreement' but further elements of self-reference can be identified which link what Kuhn said about reasonable agreement to: (i) the mechanisms which sustain the trade-offs that scientist must make between desirable but competing characteristics of a theory, for example simplicity and accuracy (p. 212), (ii) the patterns of relevance linking different phenomena, e.g. linking the inclined plane with free fall and trajectories (p. 115), (iii) the selective use of intellectual traditions, as shown by Galileo's use, at various times, of both Aristotle and Archimedes (p. 215). In all these cases the community of scientists has to make collective choices and then sustain the choice as a convention. Without the capacity to sustain conventions, or to institutionalize preferences, scientific practice would fragment. The cognitive order of science would be replaced by subjectivism, individualism and cognitive anarchy.

Kuhn spoke of reasonable agreement, but a theory that 'agrees' with the facts is a theory that 'corresponds' with the facts. The table in the textbook, showing the reasonable agreement of theoretical predictions and experimental measurements, exhibits their 'correspondence' to one another. Kuhn's analysis is therefore relevant to the famous correspondence theory of truth. In effect Kuhn posed the question of what 'corresponding with the facts' amounts to in the context of experimental science. It is illuminating to compare Kuhn's position with that of Karl Popper, a determined advocate of the correspondence theory of truth.

Popper places great emphasis on the Tarski-style formula that 'Snow is white' is true if and only if snow is white, or, more generally, 'P' is true if and only if P. Popper says that this formula provides a valuable clarification of what is meant by correspondence with the truth. He believes that it captures common-sense intuitions about truth and furnishes a justification for the confident appeal to 'correspondence' in discussions about the methodology of science. In particular, Popper thinks that this approach provides a weapon against relativism because the Tarski formula is said to embody an 'absolute' theory of truth. In his intellectual autobiography Popper (1976, 141–143) said:

> The correspondence theory of truth which Tarski rescued is a theory which regards truth as objective: as a property of theories, rather than an experience or belief or something subjective like that. It is also absolute, rather than relative to some set of assumptions (or beliefs) […] (Popper 1976, 143)

Suppose that, for purposes of comparing Popper and Kuhn, the Tarski-style correspondence formula 'P' is true if and only if P, is re-cast into the form of Kuhn's text-book table. The table, which might be called a Tarski table, would look like table 17.2.

Predicted Value	Measured Value
n_1	n_1
n_2	n_2
n_3	n_3
.	.
.	.
.	.

Table 17.2: A Tarski table.

Just as the symbol 'P' occurs twice in the Tarski formula, so each symbol 'n_i,' (i = 1, 2, 3, etc) occurs twice in the table, once as a predicted value and once as a measured value. Thus the predicted values of the quantity n_i and the measured values of n_i will be identical.

What are we to make of the Tarski table? For example, could it be used to perform the function that Kuhn identified when he reflected on the real tables found in real textbooks and research papers? The answer is: No. As Kuhn explained, the real table was of value because it conveyed information *both* about nature *and* about the scientific community. A Tarski table cannot be used in this way. In particular, it cannot be used to convey what the relevant scientific community count as reasonable agreement. The real table could be used in this way precisely because the numbers in the left and right-hand columns were *not* the same. Unlike the real tables, the Tarski table is doomed to be an idle cog-wheel in the machinery of science.

Popper sometimes speaks of truth as a 'regulative ideal.' Could it be said in defence of Popper's position that the Tarski table functions as a regulative ideal? Perhaps the table can be understood as giving the scientist a goal towards which to work. Kuhn would certainly agree that the column of predicted values, on the left-hand side of the table, constitutes a goal of some kind. He identified theoretical predictions playing such a role when he described how scientists used a theory to guide them as they refined their instruments and techniques. It was in this connection that he spoke of self-fulfilling prophecies. But to use the left-hand column as an ideal is not the same as using the table as such, that is, both columns, as some sort of goal. Indeed, Kuhn had some caustic comments to make about results that are too close to the predicted values, that is, tables of results which begin to look like the Tarski table. He noted that, "at least on a student lab report overly close agreement is usually taken as presumptive evidence of data manipulation" (p. 184). And the same applies to suspiciously good research results published in scientific journals. One could therefore invert Popper's argument, about absolute agreement as a regulative ideal, and present the Tarski table as a symbol of shameless and extravagant fraud.

Popper was equally off-target, in the remarks quoted above, when he said that Tarski's analysis lent support to the idea that truth was a property of theories and could be abstracted from the 'subjective' assumptions and beliefs of those who employ the theory. It is right to demand objectivity but, where Popper sought objectivity in the Tarski table, Kuhn sought it in the collective use of real tables. Kuhn knew that the theory and the table (the real table) must be linked—just as the examples in an ostensive definition must be linked to the agreed use of the term in a social group. In both cases those links embody and assert the normative dimension, a dimension that can only exist in the form of a social institution. As Kuhn put it:

> Without the tables, the theory would be essentially incomplete. With respect to measurement, it would not be so much untested as untestable. (p. 186)

The theory would be untestable because there would be no conventions about how close the experimental results had to be to the predictions in order to be deemed to 'correspond.' In the absence of these conventions there could be no objectivity in the assertion that theory and experiment 'correspond' or 'do not correspond' with one another. Nothing in reality will ever exemplify the absolute identity of the Tarski table and the table itself provides no guide to action in the real world outside the ideal table. Equipped with Kuhn's argument we thus reach a striking result. Despite all his emphasis on testability and objectivity, Popper failed to detect the real-world preconditions of testability and mistook their presence for mere 'subjectivity.'

Summary and Conclusion

I began with an account of social reality as something created through a process of self-reference. It will be evident that Kuhn's most famous analytical concept, the paradigm, has this character. An achievement becomes a paradigm in virtue of being regarded as a paradigm, treated as a paradigm and known as a paradigm (though the word 'paradigm,' as such, may not feature in the process). What may be less evident is that these forms of interaction are also at work on other levels of Kuhn's analysis. The significance of the paper on measurement is that the same self-referential and self-reinforcing processes that sustain a paradigm can be detected in the detailed construction of the standards of reasonable agreement between theory and experiment. Kuhn's message was that this vital relation cannot be understood without bringing in the scientific community and he traced the self-referential character of that process.

I have tried to make Kuhn's sociological insights explicit and use them to understand some of the puzzling things that he said about scientific revolutions, for example, that after a revolution scientists work in a different world and that new objects, such as the pendulum, appear to spring into existence. I have also tried to show that Kuhn's analysis of the self-reference involved in scientific measurement has important implications for the correspondence theory of truth. His account of the relation of 'reasonable agreement' between fact and theory exposes the embarrassing lack of *empirical* curiosity that so often disfigures the appeal to the correspondence theory of truth when it is used as a weapon against relativism.[12]

In regard to historiography, my aim has been to expose the intimate connection between the history of science, at its best, and the sociological analysis of knowledge. This intimacy is exemplified by Kuhn's work—but so is something else, namely, the difficulty of perceiving and acknowledging that bond. Kuhn succeeded in one of these respects and failed in the other. He displayed a strange mixture of awareness and lack of awareness. If an historian of science of Kuhn's calibre can make this mistake, one can only wonder what will happen when the history of the history of science comes to be written.[13]

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[12] Lack of empirical curiosity about the actual attribution of truth, in favor of formal specifications and definitions, is not confined to Popper and Tarski. These defects are to be detected in the work of many other philosophers. For example, John Searle also uses a form of correspondence theory to justify the rejection of relativism. Like Popper, Searle ignores the irreducible contingencies of human judgment that surround every act of concept application and treats the relation of 'satisfaction,' holding between a concept and the material world, as wholly dependent on the world. Once meanings have been 'fixed' by a definition, says Searle, there is nothing relative about it. Searle's theory is to be found in Searle (1995). For a criticism of Searle and a range of other anti-relativist positions see Bloor (2011, 444–446).

[13] Acknowledgement. I should like to thank the editors for their valuable comments on an earlier draft of this paper.

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Chapter 18
The Notion of Incommensurability

Harry Collins

In a paper published in 2012, I argued that Kuhn made the intellectual space for the creation of sociology of scientific knowledge and all that followed. I suggested, however, that pretty well all Kuhn's (e.g., 1962) ideas had been anticipated. At the request of the editors of this volume I will begin by repeating some of those arguments. Thus, as is now well known, many of the ideas in '*Structure*' were anticipated in Ludwik Fleck's (1935), *Genesis and Development of a Scientific Fact*. Less well known is that the idea of paradigm change, which is not found in Fleck, was anticipated. Consider the following passage:

> To illustrate what is meant by saying that the social relations between men and ideas which men's action embody are really the same thing considered from different points of view, I want now to consider the general nature of what happens when the ideas current in a society change: when new ideas come into the language and old ideas go out of it. In speaking of 'new ideas' I shall make a distinction. Imagine a biochemist making certain observations and experiments as a result of which he discovers a new germ which is responsible for a certain disease. In one sense we might say that the name he gives this new germ expresses a new idea, but I prefer to say in this context that he has made a discovery within the existing framework of ideas. I am assuming that the germ theory of disease is already well established in the scientific language he speaks. Now compare with this discovery the impact made by the first formulation of that theory, the first introduction of the concept of germ into the language of medicine. This was a much more radically new departure, involving not merely a new factual discovery within an existing way of looking at things, but a completely new way of looking at the whole problem of the causation of diseases, the adoption of new diagnostic techniques, the asking of new kinds of questions about illnesses, and so on. In short it involved the adoption of new ways of doing things by people involved, in one way or another, in medical practice. An

> account of the way in which social relations in the medical profession had been influenced by this new concept would conclude an account of what that concept was. Conversely, the concept itself is unintelligible apart from its relation to general medical practice. A doctor who (i) claimed to accept the germ theory of disease, (ii) claimed to aim at reducing the incidence of disease, and (iii) completely ignored the necessity of isolating infectious patients, would be behaving in a self-contradictory and unintelligible manner.

This passage can be found in a book published four years before '*Structure*…' written by the Wittgensteinian philosopher Peter Winch (1958, 121–122). For me, it was Winch who provided the set of ideas that led me to read Wittgenstein's (1953) *Philosophical Investigations* and provided the template for me to understand it. What this meant was that when I stumbled across a hardback copy of '*Structure*' in a bookshop and, intrigued by the title, took it home and read it, I saw it as the application of the Wittgensteinian idea of 'form-of-life' to science. And, of course, it is well known that David Bloor, who probably wrote the first paper (1973) that belongs to the sociology of scientific knowledge, spent a large proportion of his academic life trying to convince philosophers that Wittgenstein was as much a sociologist as a philosopher and that his ideas could be used as the backbone of the sociology of scientific knowledge (books published in 1976 and 1983). As far as British sociology of knowledge is concerned, I think it was Wittgenstein, and for me especially Winch, rather than Kuhn, who provided the intellectual meat.

This, I want to argue, does not reduce Kuhn's importance as much as it might because without his book we might well not have noticed what all those existing ideas were pointing to. For me personally, without Kuhn I might not have noticed what that passage in Winch, who I had been reading with great thoroughness, signified. I might not have noticed that Winch had already invented what amounted to normal and revolutionary science. Without Kuhn, no-one might have thought it worthwhile to translate Ludwik Fleck's book into English (it happened in 1979) because no-one could have noticed the mention of Fleck in the preface to '*Structure*' and no-one could have noticed the extent to which "*Structure's*' ideas had been anticipated. Kuhn, I argued then, and want to say again, made the space for the sociology of scientific knowledge, even if the ideas that were drawn on when it was put into practice came from somewhere else and that, furthermore, those other ideas were stronger because of the way they integrated concepts and practice (see the Winch quote), whereas Kuhn was later tempted to start disassembling paradigms into their component parts—a terrible mistake.

All that said, there is brilliance and originality in Kuhn and it is certainly found in the idea of incommensurability; incommensurability has the best claim of all the ideas to be his alone and it is his most important idea. We have almost forgotten that before Kuhn talked of incommensurability and paradigm revolution, the crystal of science seemed perfect and impenetrable. Before Kuhn, science was thought of as driven by its internal logic supported by a universal language and uniform practices. There could not be a sociology of scientific knowledge because science was an automaton with humans merely in attendance. The only possibility was, as Laudan (1983) insisted, a sociology of error—explanations of what caused Nature's attendants to do their job carelessly. The idea that different scientists could take the same logic and the same data and legitimately synthesize two different pictures of the world was itself a revolution in thought and it was that revolution that smashed the crystal into fragments and allowed the sociology of knowledge to reassemble them in many different ways.

I think that incommensurability has been shamefully neglected, perhaps because it has been thought to have been finessed by Galison's (e.g. 1997) trading zone idea. But the observation that groups on different sides of a conceptual divide can work together does not do away with the basic idea of incommensurability, nor its problems. The revelation was that different groups can quite reasonably see the world in different ways in spite of their common experimental and logical environment. The consequences are everywhere, from the repeated failures of interdisciplinary projects, to arXiv's tortured policing of its boundaries, to vaccine revolts and the row over global warming.

Incommensurability, then, is all around us. It is useful to invoke a fractal model. Incommensurability happens at a whole variety of levels each one of which reflects the structure of the one above and below. Kuhn had scientific revolutions in mind—the change from a Newtonian universe to an Einsteinian universe—but the same kind of thing happens at every level. Expressed in the way incommensurability impacts on practical life, the basic thing is this: we learn to see the world through socialization—mainly linguistic socialization (Collins and Evans 2007), and scientific socialization varies from place to place. It is sometimes impossible and always very hard to find a summary description of the differences and thus resolve them. This is because it is impossible, or very hard, to capture what comes to be understood through immersion in an oral culture without being immersed. What comes to be understood is tacit. Since oral cultures come in varieties of sizes embedded within one-another, so does incommensurability. Incommensurability is sometimes writ large and sometimes writ small.

I want to suggest that the logical version of incommensurability—analogous with the relation between the length of the side and diagonal of a square—as just the strongest and most colorful version of the idea. But sometimes tacit knowledge can be explicated. I classify tacit knowledge according to its degree and method of explicability, reserving only one class out of three to be inexplicable in the foreseeable future (Collins 2010). Sometimes it turns out to be possible to find a way of translating the vocabularies once everyone gets together long enough to discover the problems and put in enough work. But, and this is crucial, that these difficulties can sometimes be resolved does not mean that they are not part of the problem to which Kuhn drew attention. Prior to the point when the need for translation has been noticed and the painful process of translation is completed, there is effective incommensurability and in terms of its effects it might just as well be the real, quasi-logical, thing. The boundaries of practice-language groups (Collins 2011) remain the boundaries of knowledges, whether they are penetrable or not.

For example, when philosopher, Martin Kusch, and sociologist, Harry Collins, were writing *The Shape of Actions* (1998) we spent months using the word 'action' in different ways without realizing it—simply puzzled and frustrated by the fact that we could not sort out the foundation of the book. Luckily, once we spotted what was going on we could inter-translate the philosopher's meaning of action and the sociologist's. There was no side-and-diagonal-of-a-square incommensurability; it was more like the side and circumference. Nevertheless, until we realized what was going on we were in a situation of complete puzzlement that we would never have noticed, leave alone resolved, if we had not been pushed together for hours in front of the same whiteboard.

Scientists have a variety of practical means for resolving these problems. For genuine interdisciplinarity to come about, it is necessary for the different groups to spend years talking to each other, some of the time learning each other's non-translatable languages and some of the time spotting where common vocabularies mask diverse meanings. We now know that to talk meaningfully, even when translation is not possible, it is not necessary to master another's practices, but only acquire the interactional expertise necessary to talk meaningfully—that is, learn the practice-languages (Collins and Evans 2007; Collins 2011). Easier is the ambassadorial model where, not the whole group, but a member of a group learns the practice language of the others and can then act as an authentic representative of the other group. There is the boundary object, or trading zone, model, where different meanings may be invested in the same object but it can still be used as a medium of intellectual exchange, and there is the boundary language model, where new pidgins and creoles are invented to span a border (though I have never actually encountered such a thing in my practice or fieldwork). There is also the multi-disciplinary, as opposed to interdisciplinary, model, where a manager learns the various practice languages and co-ordinates the outcomes from what are otherwise self-contained communities, and there is the consultancy model, where one group simply commissions a piece of work from another, knowing nothing of the methods or concepts that go into producing it.

Non-logical versions of incommensurability are extraordinarily important. One variant, which is especially important to the relationship between experts and the public, is driven by the difference between the published and electronically promulgated literature, on the one hand, and the understanding of that literature in the relevant oral cultures on the other. There are members of the general public reading the primary source literature or the internet who have no idea that what they are reading is counted as worthless in the oral culture of mainstream science. It is quite impossible to judge these things from the appearance of the publications while the arguments on the Internet look utterly convincing (Collins and Evans 2007). It is the scientific argument on the Internet that was used by South African President, Thabo Mbeki to justify not distributing anti-retroviral drugs to his people, although the scientific arguments were long dead in the oral community. It is the Internet that drives other vaccine revolts, such as that over MMR, and not the mainstream literature. Even in science proper, there are scientists in field 'A,' basing their research on published results emerging from field 'B,' which everyone immersed in B's oral culture knows are wrong; that this happens is matter-of-fact knowledge among physical scientists. At the scale of whole sciences, the physics pre-print server arXiv grapples with incommensurability every day as it tries to find a way to police its boundaries, defining some scientists and their sciences in, and some out. All the protagonists are highly qualified and often highly published, though those outside the oral culture are often published only in fringe journals (Collins 2012).

Trevor Pinch and I wrote (1982) a Kuhn-inspired book on the incommensurable relationship between parapsychology and mainstream science showing how the very same set of detailed observations of child spoon-benders could be read two opposite ways. Today I can try to discuss parapsychology with more or less any senior physicist and will be met with a certainty that its practitioners are fools or charlatans or both. They are not, they just see the world a different way and Kuhn led Pinch and I to see this. As we put it, Kuhn invented a new way of not being able to do (or see, or speak of) two different things at the same time.

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Chapter 19
Kuhn, Meritocracy, and Excellence

Michael Segre

Two terms are frequently mentioned in relation to science and education: "meritocracy" and "excellence." They are often confused, although they express conflicting concepts—meritocracy normally aims at conforming to a framework, whereas excellence breaks away from it. I argue that Kuhn realistically describes science as a structure that seeks merit, rather than excellence, which is what science and education mostly require. This has implications, *inter alia*, on the history of science, which is also generally oriented towards meritocracy rather than excellence.

The term "meritocracy" was coined by the British sociologist Michael Young in 1958, four years before *The Structure of Scientific Revolutions* appeared (Kuhn 1996). The proximity of these events may not be coincidental. In a sarcastic, fairly prophetic book entitled *The Rise of the Meritocracy*, Young portrays and criticizes Britain under the future rule of a class favoring merit.

Yet, what is merit? The back cover of the Pelican edition of Young's book defines it as "intelligence + effort"—a definition that only appears to be clear-cut for there are multiple and distinct criteria of merit (and also of intelligence and effort), which are at times subjective. In fact, the book implies diverse qualities, such as competence, credentials, commitment and popularity—all steering towards what we generally call "success"—assessable in relation to a given framework. In perusing *Structure*, it is amazing how one finds all these terms in reference to normal science. Education, though requiring excellence, above all incentivizes merit as well.

No doubt as Young argues, meritocracy is better equipped than past social structures, such as those based on caste, gender, race, or, more recently, membership in a political party, in achieving certain goals. Meritocracy renders a state apparatus more efficient. It helps in winning a war. It has kept empires going. The ancient Chinese rulers, inspired by Confucius, had already understood the importance of meritocracy. Similarly, the British Empire was so successful thanks to a network of civil servants hired on the basis of competitive examinations. And Napoleon was initially successful in his campaigns thanks to a meritocracy educated in the newly founded French "Great Schools." Napoleon's imperial meritocracy breached the traditional class separation and included a curious mixture of revolutionaries, army officers and former aristocrats, among them illustrious scientists. (Hahn 1971, chaps. 9 and 10; Belhoste 2003, chap. 1) Today, institutions such as administrations, firms and universities all encourage meritocracy. Women have been emancipated thanks to meritocracy. In general, we can thank meritocracy for much of our well being and feeling of security.

The leading meritocrats of yesterday and today are admired and said to excel. Here lies the confusion: excellence conflicts with merit. To excel, as the Latin root suggests, means to break a framework, not to make the best of it. Meritocrats can be excellent individuals, yet their performances are normally judged in relation to specific frameworks and goals. As such, they conform; the excellent often do not. Young seems to have understood this, hence his reserve and sarcasm towards the current encouragement of meritocracy.

Kuhn, on the other hand, portrays science as giving more attention to conformism than excellence. He grants that excellence, or "genius" as he calls it, is required to "shift the vision" i.e. to create a new paradigm, incommensurable with the former one (Kuhn 1996, 115–122; this argument, by the way, is self-contradictory for one needs nonconformity to break a framework). He allows scientific leaders to be sufficiently nonconformist so as to break the framework occasionally, but normal scientists conform both in following the paradigm and in switching allegiance to a new one when told to do so (Kuhn 1996, 152–153). He considers normal scientific activity, rather than excellence, to be the main avenue to the "*gestalt* switch." (Kuhn 1996, 166) A new paradigm wins consensus within the scientific community that endorses the choice, either irrationally or through common sense. In Kuhn's words, a scientist is successful when his endeavor "is rewarded through recognition by other members of his professional group and by them alone" (Kuhn 1999, 21). Kuhn rightly uses Galileo as an example of excellence (Kuhn 1996, 119). Yet the secret of Galileo's success was precisely in his nonconformism and ability to break scientific, literary, social, institutional and other boundaries. No wonder he was punished by the gatekeepers of tradition and conformism.

Admittedly, research today—both "hard" and "soft"—closely follows Kuhn's meritocratic picture. Editors of scientific periodicals and books—the springboard for academic success—as well as their purposefully chosen peer reviewers, employ meritocratic criteria (Agassi 1990). Normal scientists become opinion leaders in the wake of their popularity, rather than their contributions to science. Opinion leaders are mentioned in scholarly meetings and publications as a matter of ritual, their arguments repeated, hailed and embellished with trendy expressions. Universities, especially those called "centers of excellence," select and praise faculty and students according to pre-established parameters (Readings 1996, chap. 2)—an evident contradiction causing confusion. Criteria for the evaluation of projects or exam questions are formulated accordingly. Students are said to "excel" when they manage to produce a flawless, up-to-date compliance with currently accepted views. The damage is vividly described by Karl Popper in his *The Open Society and Its Enemies*:

> Instead of encouraging the student to devote himself to his studies for the sake of studying, instead of encouraging in him a real love for his subject for inquiry, he is encouraged to study for the sake of his personal career; he is led to acquire only such knowledge as is serviceable in getting him over the hurdles which he must clear for the sake of his advancement. In other words, even in the field of science, our methods of selection are based upon an appeal to personal ambition of a somewhat crude form. (Popper 1966, vol. 1, 135)

Popper's follower, Joseph Agassi, labels students who are on the way to becoming normal scientists or academics, in Kuhnian terms, "super-normal."[1] For Popper, the resulting normal scientist "is a person one ought to be sorry for [because he] has been taught badly" (Popper 1970, 52).

The reasons for sticking to meritocratic criteria are easy to comprehend: a meritocratic option is safer than a violation of the framework; excellence is harder to recognize since it often takes time to become established. Yet, the establishment excludes nonconformists at the risk of thereby excluding excellence as well.

Yet today more than ever, the need for excellence is great, both inside and outside science. Kuhn and Young wrote half a century ago, in a period in which meritocracy was still needed and triumphant. Today, empires no longer die slowly and peacefully, as the British Empire did, but instead crash like the Soviet Union. That crash, incidentally, was to a great degree due to the development of science and technology, as Mikhail Gorbachev openly admitted: it came in the wake of the Chernobyl disaster (Gorbachev 2006). And many economists who caused the ongoing global economic crisis came from so-called "centers of excellence." To be sure, this was and still remains a complex crisis, but "centers of excellence" are considered such because they manage to create an aura of leadership and become a reference for better or worse.[2]

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[1] Stated repeatedly in conversations with the author.

[2] Not being an economist, I am not in the position to judge who is responsible for the recent economic crash. Let me however refer to Gary Stiglitz's article (2010), which holds Alan Greenspan, Robert Rubin and Larry Summers accountable. From *Wikipedia* (accessed July 19, 2014), I learned that Greenspan received an M.A. in economics at Columbia University; Rubin graduated summa cum laude in economics from Harvard University, and later attended the London School of Economics

It is not that these centers should be closed, or that we should encourage anarchy or altogether abandon a meritocratic approach for which we still have no substitute. Yet, it is wise to be aware of the need for excellence and how it differs from merit, in order to avoid confusing the two and to better engage with opinions that are not quite in line with the received paradigm. To reduce risk, changes of frameworks can be controlled and made gradual.[3] In any case, when requesting merit, it is advisable to clearly specify and debate the criteria for merit. If this is not done, one falls into the generally accepted, confused meaning of the concept.

To conclude, the history of science has gone through a few paradigm shifts, including that of Pierre Duhem, who a century ago freed the field from the yoke of positivism and inductivism, as well as that of Alexandre Koyré and Bernard Cohen, who both studied the significance of mistakes and failures in research (e. g. Koyré 1978, 65–66). And yet, titles of recent science history publications, as well as titles of meetings and their invited participants, suggest the adherence to the general trend of conformism, rather than excellence. Leading historians of science create a confused conformism to a confused paradigm, which is at times hailed as brilliance.

The history of science can lead the way, since it can pinpoint past cases of real excellence that are not always easily spotted, and foster their repetition. This implies the particular responsibility of calling to attention the fact that merit does not always mean excellence.

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and received an LL.B. from Yale Law School; Summers studied at MIT and Harvard, receiving a Ph.D. from the latter. Moreover, in 1983, at age 28, Summers became one of the youngest tenured professors in Harvard's history.

[3] By means of what Popper calls "piecemeal [social] engineering," which he considers as "the only rational one," Popper (1966, vol. 1, 157).

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Chapter 20
From Structures and Tensions in Science to Configurational Histories of the Practices of Knowledge

John Pickstone

Introduction

I begin this paper with notes in praise of Kuhn, but with a sense of regret that some of his best in insights have barely been followed up—as yet. The Kuhn I praise is not primarily the author of paradigms. When I was a student in London around 1970, they were a part of the Popper-Kuhn-Lakatos-Feyerabend arguments that were then central to history and philosophy of science; but paradigms were only one way of historicizing science, and other guides were becoming available, especially for those of us who came to focus on the history of biology and medicine—notably Canguilhem, Foucault, Toulmin, Collingwood and the German historians of medicine who had emigrated to Baltimore in the 1930s. The Kuhn of the history of quantum physics has remained beyond me; but from its appearance, I valued Kuhn's essay on the tension between mathematical and experimental traditions in the history of the physical sciences (Kuhn 1976). The article seems well cited, but to my knowledge no historians of physical sciences have persistently built upon its suggestive arguments and promising design. Perhaps the closest is Peter Galison's *Image and Logic* which highlights the tensions between experimental and theoretical physics (Galison 1997), and aspects of the work of Andrew Warwick, for example his demonstration of the different traditions into which Maxwell's work was taken up (Warwick 2003).

I appreciate Kuhn's approach because I have tried to work in a similar way, though I did not in fact follow Kuhn. Mostly I took my methods and directions from Foucault and from the history of medicine, but I used Kuhn's article, and my conversations with colleagues, to assure myself that my arguments also applied to the history of physical sciences. To my mind, that article demonstrated some of the key methodological moves which our subject needed—and still does.

What then are the strengths of that article, as a sketch for a general history of science—at least. First and crucially, the demonstration that science is not unitary; that the history of natural knowledge is better modeled through changing relations between contrasting traditions. This simple move provides the historian with a dynamic structure, in tension, over time. Each of the component traditions had its methods and characteristic forms of development; the tensions provide part of the dynamism. This historiographic tactic could have made room for systematic methodological pluralism in science and historiography; its openness should have defused jejune fears of ‘grand narratives’. It can provide a strong periodization, but with plenty of room for uneven development; it makes sense of the second scientific revolution, which in my experience is commonly accepted and rarely analyzed. And it opens nice questions both about national styles and the history of mathematics. Thus the costs of its neglect seem to me considerable.

Much of the excellent historiography since the 1970s has presumed a certain unity of science, at least by failing to establish any systematic distinctions between different kinds of science. This seems as true of sociology of science or material cultures studies as it is of Latourian accounts. Studies ‘by site’ have brought us laboratory, field and museum sciences, but they tend to depict a common set of methods modified in response to by different environments. The literature on field science, for one, seems to show no particular methods, only shared constraints (Kuklick and Kohler 1996), and much the same is true of colonial science (Palladino and Worboys 1993). I once wrote a paper on museological sciences c. 1800 (Pickstone 1994), but the term museological is liable to mislead if intended as a single type, since it is clear that Natural History Museums supported both the natural history for which most of them were founded and the comparative anatomy which some of them developed from the early nineteenth century. Indeed museums were the sites in which several kinds of analytical sciences were built out of various kinds of natural history, and where those interactions and compoundings continued. For clarity, in my view, we do best as historical analysts to give priority to the projects or tradition, not to the sites (Pickstone 2012), and so we come back to Kuhn’s paper. As far as I know, Kuhn never explored the physical-science sites in which mathematical and Baconian traditions interacted and produced new kinds of work, but he could have; and as we shall see below, there is now lots of historical work which can help answer the relevant questions.

Note that this museological example follows the same pattern as Kuhn’s article in presenting two traditions as interacting and compounding. This is its advantage over another of the recent mechanisms for systematically breaking up the unitary history of science without reifying scientific disciplines over time or descending to the specificities of actors’ labels at any particular time. The idea of styles in science was introduced at length by Alistair Crombie (1994) and has been subtly discussed by Ian Hacking; it has much to recommend it, and Kuhn’s two traditions appear in Crombie’s and Hacking’s lists of styles (Hacking 1992, 1996). What is less obvious in their accounts, and in a more recent version (Kwa

2011), is the tensions, the compoundings of styles, and the subtle imbrications which make up much of scientific and technical practice, and to which Kuhn pointed.

This compounding and structural development is allowed for in principle, it is true, but it is not a prominent feature of style histories. It is noteworthy that when Hacking wrote on Foucault and shifts in knowledge c 1800, he connected Foucault's account of the eighteenth-century episteme to Kuhn's notion of immature science and to the Baconian tradition, but he did not discuss the parallel tradition of mathematical analysis. Nor did he follow either Foucault or Kuhn in trying to account for the structures of the sciences and the technologies emergent from about 1800—in medicine, zoology, economics and philology (Hacking 2002). I have tried to draw on both Foucault and Kuhn and to connect them constructively, as I will show below, but first I note other important approaches which seem to me to be limited in similar ways to style analysis.

A recent collection of excellent historical essays on observation shows how seventeenth-century understandings of observation developed, in part, through the interplay of two traditions of collecting: of serial *observationes* on the weather, medicine, planetary positions etc.; and of *experientia* related to crafts. (Daston and Lunbeck 2011). But once the focus moves beyond the seventeenth century these interplays seem absent from the narratives about observation, or at least from the summaries. It is as if observation had become an established practice to be found at various sites and times and in various disciplines; as if it did not thereafter matter how the observations were related to analysis or theory, or to contemporary questions of precision; as if observation was one kind of work, threaded through science, rather than interacting and being compounded with others in complicated, dynamic projects. Much the same might perhaps be said about 'objectivity' when it is analyzed primarily through shifts in moral economies, with little attention to the changing configurations of the complex intellectual and professional projects of which measurements, readings and interpretations were parts (Daston and Galison 2007, and see Porter 2008). In other places, I have tried to show how both these approaches might be enhanced by giving more attention to the changing structures of scientific projects, by histories of tensions, interactions and compoundings (Pickstone 2007, 2012). How then might we use this Kuhnian tactic, not just for the history of science as it existed in 1976, but for the much wider range of topics and concerns which our community of historians now addresses?

In this essay, I first consider the spectrum of present historiography and the expansion of the 'range' since Kuhn. I then show how models of working knowledges in tensions and in compounds may help us analyze the history of science, technology and medicine, as if one complex assemblage, from the early modern formations through to the mid nineteenth century. I further broaden the essay to consider how such models may also be useful for social and cultural sciences and their associated 'real world' practices, such as social action or painting. I conclude with some tentative notes on Romanticism and the reconfigurations of Art / arts and of 'science'. These later sections should to indicate how important understandings of the history of art can fit with and extend my model of the history of STM, especially for its main crux—the deep and complex shifts in the late eighteenth and early nineteenth centuries.

HPS and HSTM

Historians today want much more than Kuhn attempted to provide. We speak to a different present, in a different historiographical context, and we might reasonably ask for analyses that extend across all the sciences and technologies, including social sciences and technologies (or even, perhaps, all the other subjects that German speakers can call *Wissenschaft*). In as much as we are concerned with wide audiences for science (and history), we may also wish to include vernacular or mundane understandings and practices. That breadth would cover a variety well beyond anything Kuhn envisaged when he argued against the unity of science (Pickstone and Worboys 2011).

The Kuhn conference at Berlin helped make evident the great variety of ways in which the history of science has developed since Kuhn wrote. Simplifying, perhaps unfairly and certainly incompletely, I there contrasted two patterns. One was represented by the history and philosophy of science (HPS) I found in London c 1970, which is roughly that from which Kuhn produced his article. The other contains the history of medicine which I began to see in North America in 1971–73, and especially the history of science and technology which I found in Manchester and the Northern Seminar from 1974. It is in this second tradition, of History of Science, Technology and Medicine (HSTM) that I have since worked—hopefully without losing sight of the first.

In brief, HPS centered on philosophical issues, as exemplified in the Popper-Kuhn-Lakatos-Feyerabend sequence; it was 'sociologized' in Edinburgh and Bath, and 'anthropologized' in Paris. The resultant style has been practiced with distinction in Harvard, Cambridge UK and Berlin, and many other places. Which is not to say these places only included that tradition, but that work in these places has generally shared a focus on history of science, rather than technology or medicine, and has retained a strong interest in the first scientific revolution, without neglecting later periods.

The second formation I think of as rooted in the North of England, first in Leeds (especially under Jerry Ravetz) and then in Manchester (led by Donald

Cardwell), not forgetting aspects of Lancaster (led by Robert Fox) and the exemplary work at Bradford of Jack Morrell (see Ravetz 1971; Cardwell 1994; Fox 1995; Morrell 1972). The North American base was Philadelphia, where through Arnold Thackray the Northern tradition cross-fertilized with American history of technology (Tom Hughes) and the social history of medicine (Charles Rosenberg), including policy issues (Rosemary Stevens). In these places, the main stress was on knowledge at work, taking science and technology (and medicine) together, often for particular localities (not just sites). The chronological center of gravity was the nineteenth and then the twentieth centuries, and the social relations in question were not restricted to those of scientists and patrons; they included professional associations, hospitals, governments and wider publics. This HSTM tradition was important in the development of history of medicine in Britain, including the work at Oxford of Charles Webster who had been associated with Leeds. It was also linked with work that was initially marginal at Cambridge—notably that of Bob Young on history of biology and medicine, and of Martin Rudwick on history of geology (both of whom were formative influences for Roy Porter). As an approach it is now widespread—as the popularity of the acronym HSTM indicates.

That gross simplification may serve as a very rough guide to the social history of the subject in Britain, if we also note that while most of the entrants to these studies in the 1960s and 1970s had been trained in the sciences, entry from history graduates has substantially increased since, perhaps especially into history of medicine. That is one of the reasons for the loosening of the connection with philosophy of science and the tendency of historians of STM to define their work by place and time as well as field—for example, renaissance Italian medicine or British physics in the Cold War—seeing more connections with the wider history of the period than with issues in the theory of science or of STM. This tendency has been accentuated by developments in the sub-discipline of philosophy of science which seem abstruse and unhistorical to historians. A further complication is the growth of 'science studies' in ways which either make little connection with history, or which connect only with the history of late twentieth-century science, producing a 'deep present' rather than a history informed by contrasts over time.

Historians of knowledge know that academic geographies of the kind I have sketched in the last paragraph are always very complex. Happily, our present problems of delineation are ones of riches rather than poverty; but maybe we can hope to benefit by asking about the range of our shared theories of science or of STM. Can we still remain with the traditional HPS issues which have tended to assume a unity of science, to think of technology as the application of science, and to locate the crucial revolution in the seventeenth century? That form of history of science has tended to focus on experiment, and usually on physics; it 'worked outwards' to include chemistry and biology, along with observations and collections etc. But the more we widen out, the more that frame gives out, and the more we may be left with histories which—for all their subtlety to place, time and politics—lack generality. The resultant case histories remain a key part of what we need to speak to the present, but they may lose 'the past in the present' by having no common account of the development of STM.

By my work on Ways of Knowing I have tried to bridge the gap between the HPS where I began in London and the social history of science, technology and medicine we have tried to develop in Manchester—along with colleagues in many other places. To bridge across the sciences, technologies and medicine in a Centre for HSTM, I developed a model—not from the history of physics but from the social history of medicine; not from relatively simple case of knowledge creation in physics laboratories, but from instances of medical practice with defined social relations, including patients; not by thinking of knowledge as esoteric and then 'applied,' but seeing it included and embodied in mundane practices (Pickstone 2000). I have tried to treat ways of working symmetrically with ways of knowing, and have sometimes conjoined them as 'working knowledges' (Pickstone 2007).

I have argued for four such couples, four kinds of working knowledges, both practical and 'theoretical':

1. Understanding philosophical or symbolic 'meanings' and their use in communication;
2. 'Sorting' natural kinds and their use in crafts;
3. Reduction to elements, either mathematical or substantive, and their use in rationalization; and
4. Synthesis from elements, either mathematical or substantive.

Technically speaking, this is a scale of forms (Collingwood 1933). Later forms subsume the earlier, but the earlier also continue, and the relationships are often contested in ways which are context dependent. The compounding ensures that the method is analytical rather than simply taxonomical, a subtlety which seems to have escaped some recent commentators. In any given scientific project which involves more than symbolic meanings, there are relationships to be considered.

The key developments *between* the different classes of working knowledges correspond, as one would hope, to key 'moves' within the history of STM: naturalization, analytical reductions and the move from analysis to synthesis. But all three shifts are seen a reversible, indeed as continuing dynamic relations; hence the complex structures which historians and anthropologists have noted (but rarely analyzed), for example in modern medicine (Mol 2002).

Mapping these kinds and developments over time produces a narrative frame which highlights crucial changes around 1800, from when many forms of substantive analysis were invented. But this is not a model of 'the origin of science'; it is about perpetual shifts in the configurations of working knowledges—and in principle, as I have increasingly realized since the Berlin conference, it seems to work across the whole range of formalized Western knowledges, including social and cultural knowledges and practices.

There is room in this new story for Kuhn's paradigms (most of which seem to be new programs of analysis—mathematical or substantive), but paradigms are not primary. There is a much stronger place for Kuhn's essential tensions, for seeing different 'projects' in tension, and their various compoundings. In the rest of this paper, I want to show how Kuhn's insights can be related to the larger picture which I have tried to develop, for STM and then for wider fields. Maybe this will help explain my work to historians who know Kuhn better than they know history of medicine or biology. I begin with two key absences in Kuhn's essay; one set is easily explained, the other more suggestive.

Working Knowledges: The Early Modern Triad

For the early modern period, Kuhn pointed to the mathematical tradition extending back to the Greeks, and to an empirical or experimental tradition which was more recent, less structured, less esoteric, and closer to crafts and other everyday practices. This pairing now seems well established as two of the three main seventeenth-century traditions involved in the Scientific Revolution—the other being natural philosophy. The mathematical story was foundational for accounts of that revolution; what we may call the Baconian tradition has received much more attention of late, as the history of natural history and then by reference to the wider renaissance category of *historia* (Pomata and Siraisi 2005).

Kuhn discusses natural history only for Baconian 'experimental histories,' but the extension to the rest of what we might call 'extended natural history,' or even 'information sciences' seems relatively unproblematic. In Kuhn's experimental tradition, observations which one might term passive or active (that is, manipulative or experimental) were being collected, examined and sorted, more or less critically, in much the same ways as specimens. Whether the observations concerned minerals or electrical effects, birds or the stars, here makes little difference. Additionally, it is now clear the collecting and indexing of many kinds of text can be thought of in the same way, specifically as *historia literaria*. More generally, the 'sorting' of manuscripts and books might well be seen as the renaissance basis of all the other forms of *historia*; the humanities, here and elsewhere, preceded the natural sciences (Grafton 2011; Blair 2010).

The larger absence is natural philosophy, which Kuhn's essay barely discusses. It certainly could have, since one of the key points in Kuhn's own historiographical journey had been the realization that Aristotle was doing philosophy, not 'science'. In my terms Aristotle was understanding and explaining from a set of first principles which applied to human activities as well as the natural world. The best of recent historiography for or against the scientific revolution does indeed include natural philosophy as the third components (Schuster 1990). This is also very clear in the masterly survey by Floris Cohen, and in his own work which models the interactions of the Archimedean tradition (mathematical reductions), the Baconian (natural histories) and various philosophical traditions—through into the eighteenth century (Cohen 2007, 723).

This triadic model of early-modern knowledges and practices seems indeed to neatly summarize much of the best recent work in HSTM. We are concerned with the relations and interactions of three ways of knowing: the 'natural' branches, as it were, of philosophy, of mathematics and of *historia*. That triadic formulation can take us from physics to medicine and beyond because it relates to three forms of knowledge that seem to be fundamental and persistent: reasoning from first principles, the reduction of phenomena to mathematics, and all the descriptive and classificatory activities which make for 'information'—what Bacon called the sciences of memory. Naturalization and mathematization have long been seen as key aspects of the seventeenth-century scientific revolution; if we see them as continuing and unstable relationships, we can then see how they work in later periods of STM, and how they help comprise subsequent knowledge structures, at many levels. We may also note, here without argument, that each of the early modern genres had its characteristic form of utility—say, rhetoric and self-development, practical mathematics and craft. Thus we can also bridge across from practices *for* knowledge to practices *for* material advantage or improvement. This relation is *not* the application of science (as pictured in the nineteenth century), it is the knowledge *in* the liberal arts, broadly understood, from rhetoric to medicine, from astrology to architecture.

If these few sentences may indicate how Kuhn's suggestions could have been generalized across the range now required for the early modern period, we can then add a key point about the interactions. In the formal hierarchies of knowledge, philosophy once stood above mathematics and *historia*. The latter two were meant to be preparative components of natural philosophy, but mathematicians and naturalists came increasingly to challenge philosophy—through mathematical philosophies or by doctors turning from philosophical to practical knowledges. That much seems common ground among writers on seventeenth-century science, so too the important transformations in the mathematical tradition and natural philosophy, and the powerful extensions of the mathematical and Baconian traditions through the eighteenth century. Less clear is the importance of *interactions* between mathematical and Baconian traditions in the seventeenth. Kuhn downplayed them, arguing that most experiments which impacted on mathematical studies were but 'thought experiments'; only Newton was allowed as a strong bridge, and his legacy remained two-fold—a mathematical tradition based on the *Principia* and an experimental tradition based on the *Optics*. For Kuhn, modern physics was a later creation: the required bridges were partly a matter of internal developments, partly due to new institutions; and they were created with difficulty. Much was achieved in France between circa 1780 and 1830; but even by the later nineteenth century, compounding was a work in progress (Kuhn 1976).

Other historians have been more positive about the achievements of the seventeenth century in compounding mathematical and Baconian traditions, while recognizing the strong contrary tendencies throughout much of the eighteenth (Cohen 2007; Chalmers 2012). I am not competent to properly judge this issue, but perhaps our learning more about the interaction circa 1800 may help us assess the extent to which those compoundings may have been realized, albeit insecurely, in the seventeenth century.

What Happened circa 1800 in STM?

Kuhn depicted modern physical disciplines as formed by the convergence of mathematical and experimental traditions. The former typically calculated the movements of matter, drawing for confirmation on common observations and concepts. The latter dealt with the (usually qualitative) interactions of things that had secondary qualities, so to speak, like colors or heat or static electricity. The art of producing new sciences was to find something that could be quantified, so the interactions could be mathematized (Heilbron 1980, 1993). One then had a science of light, say, which was more than 'geometrical,' or a science of heat. The physicists of the mid-nineteenth century then found ways of reducing these different sciences to a smaller number of wider ones. Chemistry was said to fit this model because chemical reactions, if not very mathematical, could at least be quantified.

Kuhn did not stop to worry about how one might characterize these new physical sciences beyond the linkage of mathematics and experiments. Maybe they were too familiar. Chemists took their elements for granted; historians of physics knew that heat and light were once elements—but that seemed a temporary glitch; and no one was asking how these new physical sciences were related to the biological and social, or even to technologies. Because Kuhn's history carried us through to classical physics, we did not have to continue with historical characterizations. That may be a general problem with stories that have familiar endings: the strange precursors are studied but the familiar is taken for granted, and the connection with our present is assumed.

But step back now and look at the whole range of knowledge and practices around 1800, including the contemporary revolutions in industry, medicine, education, philosophy and the arts. Look at crystallography, stratigraphy, histology, comparative anatomy, comparative engineering, philology or political economy—and ask again what Kuhn's model can tell us. Return if you will, to the question of *one* or *many* sciences, and wonder when and why you could vote for one. Maybe because eighteenth-century natural philosophers often saw mathematical physics as *the* model, whilst natural history was only fit for refined play, 'merely professional' information—or imperial expeditions. Maybe because the Gentlemen of English Science c 1830 still saw the traditional mathematical sciences as outranking the novelties of chemistry and kindred recent formations (Morrell and Thackray 1981). Maybe because later nineteenth-century physics had common theories (for example, energy) even more extensive than the aspirations of the old rational mechanics. But Kuhn rejected all these unities, insisting on the crudities of experimental philosophy, and the independent births of different physical sciences. The more widely one looks, the more compelling is his model. In as much as science had unity, it was constructed from the disparate. Unity may have been a directing principle for some investigators, intellectually or politically, but it was not constitutive in the creation of new physical sciences, let alone the rest of the sciences and technologies.

So what, if anything, did these disparate sciences have in common? Mathematics, or even quantitation, will not stretch to stratigraphy, nor does it have much purchase for early studies of affinities or for organic chemistry; and it hardly appeared in the new biological, medical and social sciences. So maybe physics is not a good guide here; in some ways it was a residual subject—covering mathematical physics and what was left of experimental physical sciences when chemistry attained a new definition? One notes moreover the different histories of light, heat, electrostatics, magnetism and current electricity.

My own search began at the opposite end of the scale—with the distinguishing features of the new Paris medicine, especially the analysis of the human body into tissues. I knew from the work of Randall Albury that these had been seen as analogous with chemical elements, and that Cuvier's zoology and Magendie's experimental physiology also worked in terms of interactions between elementary organs or tissues (Albury 1972). From Foucault's accounts of the new sciences—of philology and political economy, as well as zoology—it was clear that they too involved new ideas of systems, with interactive parts, and perhaps developing over time (Foucault 1970). Comparative anatomy often proved a useful reference for these sciences, and for studies of machines or architecture.

Chemistry, however, with its pragmatic definition of elements and its compositional understanding of all other 'chemicals,' was undoubtedly the key reference for many other sciences. Their 'elements' too were no longer prescribed by natural philosophies but instead were unearthed pragmatically; so each new science was constituted by its elements—be they single or multiple, passive or active, related by structures or interactive, etc. Mathematization, quantitation, or manipulative experiments might be desiderata, but they were not always possible: stratigraphy was about structures, and so were comparative anatomy, histology and crystallography; in these subjects you could dissect to observe, but you could not interrogate interactions (Pickstone 2000, 2007).

So should one say that in these underprivileged fields one was left with mere observations? I think not. The distinctions between qualitative and quantitative, or between observation and experimental, do not exhaust the issues; indeed, in my view, their application may do grave damage to the historiography of this period, and others. Consider observations of nebulae and of planetary positions. The former, through the eighteenth century, may be classed as natural history; the second might also be so, for example if you were looking for particular astrological conjunctions. But if you are testing a mathematical analysis of planetary motions, or a synthetic model you have created therefrom, then you have reasons for particular kinds of accuracy. You are relating theories, however humble, to observations, and vice versa; that diadic relation *is* the project (Pickstone 2009).

If Cuvier had taught you how to do comparative anatomy, then you knew that parts of the body were related to each other by functions, and that different kinds of animal could be recognized by their different fundamental plans. In this scheme you could even make predictions about bits yet to be found (Rudwick 2005). You could radically alter classifications because you had understood deeper levels. Observation, thereby, was no longer mere description or classification, and perhaps objectivity was a moral demand in a new structural way. Perhaps it arose, at least in part, from the new need to test deeper understandings by means of surfaces, and *vice versa*.

John Herschel was clear about analysis in the different early nineteenth-century sciences: dissect phenomena to the limit, to their elements, and you have a new science (Herschel 1830, 93). William Whewell was even clearer: you have, in some sense, to know the 'idea' of the science to use it—and to clarify the idea. J. S. Mill was less clear and much less historical in assuming the terms to be obvious; but Comte knew that each of the sciences had its own basis, even though they were related to each other. Laudan (1968) remains a useful guide to these debates. Whewell still hankered for the old primacy of mathematical sciences (Yeo

2003); Comte did not; but they both knew that they were dealing with sciences that were new creations, from the mid-eighteenth century at the earliest.

I have called these new sciences analytical because, like chemistry, they dealt in elements peculiar to each particular science. The older and more general knowledges of natural history, mixed mathematics and natural philosophy continued alongside, and in some ways within, the new disciplines. One should not underestimate the number of new materials or phenomena discovered in the nineteenth century, or the continuing importance of detailed description and classification; natural history thrived as never before. But in the eyes of the new professionals, classifying was now to be based on subject-specific elements, which also helped explain structures and processes, and which might, or not, be subject to calculation. This move might also displace the general natural philosophies, for to outline the elements and understand their relations was to explore the 'philosophy of chemistry,' or whatever. Each of the new analytical sciences had its 'philosophy,' but they no longer needed to add up to one natural philosophy.

In this radically new structure of knowledge, more and more sciences were created, in parallel or through hybridizations. Different sciences could be pulled together by finding deeper elements; and substantive analysis could give rise to substantive synthesis, as had previously been the case in mathematical subjects. The combinatorial possibilities here are enormous, and so is the historiographical power: with simple working knowledges as elements, we can analyze very complicated situations (Pickstone 2007, 2011). The historiographical keys, as Kuhn knew, are disunity, differences, tensions, interactions, compoundings and configurations. But we can now push the argument much further than he did.

We might, for example, consider knowledge and action from the side of action, beginning with crafts where demystification of the process may be the equivalent of naturalization, and similarly unstable. Now as then, if actions are truly important then so is the self-preparation of the actors, and the energy that maybe needed for the requisite 'distancing'. As Collingwood outlined rather well, we have our own forms of magic—even for constructive technologies, to say nothing of the selling of the products (Collingwood 1936). As for analysis in technical practices, the best historians of technology know that much of industrialization involved the articulation of learned crafts with the working out of technical relationships, for example, about the duty of engines—rather than the appliance of science (Wegenroth 2003). Such relationships may prove to be contributory to sciences as Cardwell argued for doctrines of energy in the industrial revolution (Cardwell 1989). The exponents of analytical sciences liked to present technologies as products of their sciences; better, I think, to see analytical work as related to empirical specificities *and* to wider meanings—both in natural knowledge and in technical action.

But why should these kinds of reciprocal relations between knowledge and action be peculiar to work on the worlds of nature, or material technologies? Perhaps models of this kind are useful not just for nineteenth-century histories of natural sciences and technics, but also for social technics and social sciences, and indeed for the practices and analyses of the humanities and culture. A few notes may make clear the historiographical possibilities.

Looking Wider: Social and Human Sciences

Following Foucault, and without going much beyond the *historia*: analysis relations already outlined, we can note that several new social and human sciences not only variously extended traditions of natural history but followed the new biological sciences in their forms of analysis. I will take three examples here: history of art, mainly in France; social science and related practices, mainly in England; and philological analyses and practices, mainly in Germany. At the end of this essay, I will return to the complex question of how best to understand the practices which were called Art, rather than merely arts.

Historical and theoretical studies of Art were based on collections, often newly used for teaching. Describing and cataloguing were key activities, often with an imperial dimension. Discoveries in Egypt, or the extension of the Art canon to include gothic painting were empirical excitements that we too easily take for granted. But there were also new forms of analysis which paralleled biological approaches. Viollet le Duc's morphology of Gothic buildings in France and Hippolyte Taine's environmentalist explanations of art by social environments both owed much to the earlier work of Geoffroy Saint-Hilaire and Georges Cuvier at the Paris Museum of Natural History—which both these art-historians knew well. Intriguingly, their work on art history was also connected with attempts to destabilize the old genre structure in the Paris Ecole des Beaux Arts, and hence with new understandings of contemporary art (Walsh 2002).

As a second test, we can check the structure of the 'social sciences' in the mid nineteenth-century. The relation of French sociology to biological analysis was explicit and pervasive, at least in the form pioneered by Comte and extended by Durkheim. That is well known because histories of social sciences have often been disciplinary, in the academic sense, and have focused on sociological analysis. But if we want to grasp the scope and variety of nineteenth-century social action and knowledge, then Lawrence Goldman's account of the Social Science Association in Britain is a fuller guide. The Association's *Proceedings* certainly included much which could be seen as the social equivalents of natural history, and some of the studies extended to quantitative data which could be analyzed mathematically (using the new statistics). Lots of the material was 'practical,' for much of the public interest was aroused by reformers and philanthropists pursuing what might well have been called social arts, rather than social sciences. At the level of 'theory,' some British 'social scientists' were influenced by the reductionist analyses of the Ricardian economists, while others preferred the more Germanic, historical and inductive work of Whewell and his friends. Though relatively few, excepting the Positivists, were interested in *creating* systematic analyses of society, many referred to Herbert Spencer's analytical account of human society over time. This was heavily based on the idea of 'division of labour,' first developed in eighteenth-century political economy, and then in nineteenth-century comparative anatomy and physiology (Goldman 2005).

Comparative anatomy was also a reference point for the new philology (Amsterdamska 1987; Leerson 2012; Karstens 2012). Here the new developments, though based on British and French discoveries, were institutionalized mainly in German universities, where the pattern of development similar to that of the natural science, and comparable in range and volume. Indeed it can be argued that, just as Renaissance *historia* of texts seems to have led to the *historia* of nature, so the collections and University seminars of the German analytical philologists preceded the laboratories and research schools of the chemists, anatomo-physiologists and experimental physicists.

But in all these sciences—natural, social and human—the context for both 'historia' and analyses in German universities was not just collections: it was the will to research, and in some cases the placing of new subjects in the philosophical faculties rather than the professional faculties. Germany showed how new disciplines could flourish, not in the professional schools which had helped create them in France, but in universities with a research ethic—a new system of intellectual production which bears comparison with industrial capitalism. As academic histories tell us—for chemistry, physiology and also for fields such as philology—subjects were to be built as new disciplines, not as preparatory to professional training; all achieved 'autonomy,' more or less.

So runs the usual history of science, but again we need to be cautious, and look for continuing traditions including natural history and crafts, especially in relation with the liberal professions. Schools of medicine and law, and the teaching of languages had practical, normative goals; they were still in large measure vehicles of what had been called liberal arts—practices laced with knowledge and developed reflexively. They depended on histories of cases and professional crafts. To reduce these relations and practices to the building and subsequent application of analytical disciplines is bad history and was probably bad pedagogy—for humanities as for medicine or engineering.

Romanticism and the Creation of Art and Science

Kuhn's account of what happened to knowledge circa 1800, and mine too, rely heavily on France. If we stick with the natural sciences and technics, or even if we include the social sciences and the humanities closest to biology, then we might, to a first approximation, exclude much that was specifically German and focus on natural histories and crafts, analytical disciplines and practices—in a narrative which runs easily to our present. But if we want to account for the specificities of German forms of analysis, for new philosophies, and especially for the creation of the modern relationship between Science and Art, then we are forced to focus, albeit here briefly, on German developments. These may now lie outside the ken of most historians of STM, but they are central to histories of Art and of those parts of the humanities which depend on 'understanding'.

To begin to explain these features we must look, of course, to the formative roles of the new German philosophy—the Kantian Revolution, Idealism and Romanticism. Three points, at least, are critical in relation to the working knowledges model. Firstly the importance of new type of analysis which relied not on reduction to different elements but on tracing complex structures back to their basic or early forms—a method that was crucial for various kinds of biology as well as many kinds of historical studies (Cunningham and Jardine 1990). Secondly, the explorations of what came to be called 'subjectivity' and the possibility of analytical introspective psychology—in addition to extensions of experimental physiology. This connected with a new account of how texts (and maybe practices) were to be analyzed as systems of meaning—an account that was eventually called *verstehen* and made to distinguish *Geisteswissenschaften* from *Naturwissenschaften* (Smith 1997). Thirdly, the creation of a new view of fine art—not as a *techne* of objective representation which generalized according to classical rules, but as a result of inspiration through which artists recorded their individual and subjective responses to particularities (Shiner 2001; Abrams 1953). These three innovations were closely connected: a pervasive interest in 'ways of seeing' bound romantic art with German natural sciences and with new practices in the humanities.

The contrast between German approaches and French (and most British) analysis is very striking; indeed, for most of the century 1760–1860, Western Europe constituted a remarkable 'natural experiment' in the historical sociology of knowledge. While decompositional analysis came to dominate French science and French natural philosophy tended to be marginalized, German created a system of intellectual productivity around a novel reinvention of natural philosophy, based on formative ideas rather than the association of sensations. In many ways that experiment proved temporary for natural sciences, but for the humanities and

Art it remains foundational. The texts by J. T. Merz, through a century old, remain a key starting point for these developments in science, and in philosophy, not least for his account of nineteenth-century scientific analyses (Merz 1904–1912, esp. Volumes 1 and 2 on Scientific Thought).

We have come a long way from Kuhn and physics, and the end of a long article is not the place for deep histories of art-science relations. But if we are asking what happened to structures of knowledge circa 1800 we must at least note that the creation of Science in its modern English language sense seems to have been importantly co-constitutive with the contemporary creation of Art. Within the old triad of natural knowledges and practices, a range of parallel analytical sciences took prominence; 'science' and 'scientists' were invented to reify that new formation and assert its pre-eminence over such older arts as were said to require knowledge rather than inspiration. Over about the same period, as Kristeller argued around 1950 (Kristeller 1990) and Shiner has shown though social history, the various fine arts became reified as Art, underpinned by aesthetics and divorced from the other old arts—which came to be seen as 'applied art' (Shiner 2001). Thus the old range of arts were variously subjected to Science and / or Art, which came to be seen as parallel terms.

Attention shifted from the work of art to the artist, from the making of an object to the responsiveness of the creator, and to mental capacities which seemed to precede the making. In such ways the artist became equivalent to the new scientist rather than the craftsman. The new view of Art as primarily about the expression of personal response now stood opposed to a vision of Science as a federation of analytical disciplines and thus the sum of objective knowledge. As Abrams (1953) showed collaterally, as it were, poetry's significant 'other' in 1750 had been natural history; by the early nineteenth century, it was chemistry.

These new emphases were associated with the increasing self-consciousness and self-promotions of *scientists* (a new word for a new role) and *artists* (a new newly delimited class). Their parallel elevations came at the expense of the old arts and of natural history. Some of these were divorced from and subordinated to fine art, as lacking prestige and intellectual interest. But they were also undermined by analytical division of labour and mechanization—and by the supposition that the knowledge they contained was but the application of general principles understood by scientists.

But to write thus, of course, is to overwrite continuing traditions and essential tensions. The views of new scientist and artists were contested, and not just between the new twins, as it were. Naturalists and craftspeople, physicians, engineers, architects, radical art critics, social reformers and teachers all continued to stress knowledge from cases and from practice. They still do; and historians need better ways of saying so—by focusing on the complexities of working knowledges.

Conclusion

The history of working knowledges is not one of successions, but of tensions and contested cumulations. That is true within the traditions of natural sciences and technology, including the arts and Art. Few methods disappear from either kind of work, and technics always involve much more than is contained in formal analytical accounts. If that holds for natural sciences and technologies, it is probably true also for the knowledges and practices we call social or cultural.

We know, partly from Kuhn, that tensions between traditions are crucial motors for change. We know from countless historical case studies that the patterns of tensions change over time; that is the very stuff of our histories, and it is obvious that similar tensions occur in many different fields. I have tried to suggest an analytical and historical framework which will allow us to work across the whole range of western disciplines and practices and learn from comparisons. That move will not remove the need for detailed histories of particular cases, any more than they are removed from medicine by new forms of biomedical analysis. But it may make the work of historians less repetitive, more challenging and more collectively creative. The wider the span, the more likely these outcomes; which is why I have here extended the discussion to include social and cultural sciences, and Art as well as arts.

But as always, we must note that by widening the frame we bring new issues to the fore, and that these issues should inform the whole picture, not just the new bits. In suggesting that models of working knowledges developed for natural sciences and technologies may be useful for other fields, I want also to suggest that such long-standing goals of the humanities as moral reflexion and self-development should be part of the wider discussion—including our studies of natural sciences and technologies.

Kuhn's *Structure* was an academic and publishing success in part because it allowed social scientists to focus on their specific paradigms, thus winning scientificity at the expense of wider considerations. Many approaches to history now offer similar deals. But part of the greater challenge to historians, I would say, is to put back the wider considerations and to show them at work in and through our wider histories of knowledge.

Acknowledgements

My thanks to the Max Planck conference organizers, especially Jürgen Renn. I am most grateful to the Descartes Centre at Utrecht University, my generous and stimulating hosts in autumn 2012, and especially to Rens Bod and Floris Cohen for discussions about the history of the humanities and the scientific revolution. Thanks also to Jonathan Harwood, fellow Emeritus in CHSTM, Manchester, for his encouragingly youthful interest.

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Part 5: Beyond Kuhn

Chapter 21
Kuhnian and Post-Kuhnian Views on How Science Evolves

Mary Jo Nye

If I had happened to glance at Thomas Kuhn's new book *Structure of Scientific Revolutions* while browsing in a bookstore in 1962 as a college freshman, I likely would have seen nothing surprising in the title. I probably would have thought that the book had to do with scientific methodology and the way in which proper scientific method ensures the scientist's rejection of wrong ideas and the discovery of revolutionary new phenomena. Of course, I would have been wrong.

In fact Kuhn's title registered two bold assertions on the basis of case histories in the physical sciences. First, scientific history is a history of distinct ruptures, as in political history, and new ways of seeing the physical world are incommensurable with the systems they have destroyed. Secondly, there is a repetitive and predictable structure in scientific change which, in Kuhn's pithy terminology, is one of "normal science" under a dominant "paradigm," followed by accumulation of "anomaly," then "crisis" and "revolution." The outcome of the process is the result not just of empirical logic, but also of the psychology of conversion and the sociology of community. The Copernican Revolution, the Chemical Revolution and the Quantum Revolution are among his exemplars (Kuhn 1962).

A few years before the publication of *Structure*, Kuhn outlined some of his main themes at a conference devoted to the identification of scientific talent (Kuhn 1959, 1977). The Soviet launching of Sputnik had just triggered a panicked infusion of federal money into science education in the United States, and the 1959 conference at the University of Utah was one of many efforts to define and promote scientific creativity and achievement. Kuhn presented a paper titled "The Essential Tension: Tradition and Innovation in Scientific Research." He likely startled participants who were expressing the usual view that the creative scientist eliminates all prejudice from the mind and cultivates "divergent thinking" from accepted opinion. This point of view coincided with the already popular critical empiricist philosophy of science of Karl Popper (1959). It also corresponded with the well-known adage of Claude Bernard, a century earlier, that the scientist must leave his imagination in the coatroom when entering the laboratory and put it on again after recording experimental results (Bernard 1957).

Kuhn's different view denied the heroic stereotype of objective scientific method in search of new and revolutionary discoveries. In contrast, said Kuhn, "almost none of the research undertaken by even the greatest scientist is designed to be revolutionary" but, on the contrary, "normal research" is a "highly convergent activity based firmly upon a settled consensus acquired from scientific education and reinforced by subsequent life in the profession." Revolutionary shifts occur, but they are rare, in part because of scientific pedagogy in mature science that teaches conformity to the textbook, with exemplary problem solutions that show the student what problems matter and how to solve them. Science rests, Kuhn said, on a "dogmatic initiation in a pre-established tradition of apprenticeship." Science produces innovations because of the ways in which the scientist's puzzle-solving activities reliably expand the matrix of scientific beliefs and occasionally call those beliefs into question following an accumulation of anomalies that can no longer be ignored. The successful scientist lives in a community of essential tension between the double roles of "traditionalist" and "iconoclast." Simply by offering this interpretation, Kuhn positioned himself as an iconoclast (Kuhn 1977, 227, 229, 230).

Of special significance at the time was Kuhn's argument that the nature of scientific knowledge lies in what he variously called dogma, belief or tradition—all of which sounds disturbingly like ideology, faith and religion. Kuhn's insistence on scientific "belief" was not entirely new, but the one million copies sold of his book in his lifetime brought the notion to a new audience. Kuhn drew brief attention in *Structure* to the earlier description by the bacteriologist Ludwik Fleck of the thought-models and thought-collectives that restrict what problems are deemed significant and what kinds of answers can be sought (Fleck 1935). Kuhn also cited the physical chemist Michael Polanyi for his statements of the role in science of established beliefs and the importance of apprenticeship through which the scientist absorbs the tacit knowledge essential to future scientific practice (Polanyi 1946, 1958).

In Kuhn's view, however, Polanyi put too much emphasis on the individual scientist—the "personal" in Polanyi's terminology—and on the individual experience of conversion that can be likened to a change in Gestalt. In contrast, Kuhn said that he wanted to emphasize the collective process in the scientific community by which innovation is recognized and legitimated. Reading Fleck, wrote Kuhn, made him realize that his own ideas about scientific tradition and scientific revolution needed to be set within a sociological account of the scientific community.

At this time, the sociology of science in the United States was just emerging from its recent association with Left and Marxist alliances. Anti-Marxist views affected the reception in the US in the 1930s of Boris Hessen's account of the social and economic origins of Newton's *Principia* (Hessen 1931), J. D. Bernal's description of the social organization and social function of science (Bernal 1939) and Karl Mannheim's sociology of knowledge (Mannheim 1929, 1936), all of which nonetheless got the attention of the young sociologist Robert K. Merton, whose 1936 dissertation offered a powerful but non-Marxist interpretation of the social and economic aspects of scientific development in seventeenth-century England (Merton 1936, 1970). Merton's attitude toward Mannheim was especially important. Merton wrote a review for *Isis* of the English translation of *Ideology and Utopia*, familiarizing himself with Mannheim's arguments for the social determinants of what Mannheim called "thought-models" in the social sciences. Merton noted Mannheim's exclusion of the natural sciences from analysis and suggested, presciently, that the sociology of scientific knowledge was a future task for sociologists once they had accomplished the project of empirically studying the institutions, norms and values, priority and reward systems, and disciplinary networks of the scientific community (Merton 1937).

It was this latter kind of sociology that Kuhn had in mind for better understanding the workings of normal science and its traditions of belief and practice. In a 1968 essay on "The History of Science" for the *International Encyclopedia for the Social Sciences*, Kuhn noted past Marxist influences in the "external" study of non-intellectual aspects of scientific culture. With mention of Merton and sociologists such as Joseph Ben-David and Warren Hagstrom, Kuhn suggested that the greatest challenge now facing the history of science profession was to bring together the "internal" and "external" approaches (Kuhn 1968).

This is exactly what happened after the dust settled from early debates about *Structure*. More recently, in 2012, various conferences marked the anniversary of *Structure*. Some scholars said that Kuhn's book generated no Kuhnian research school, despite the fact that Kuhn taught and collaborated with some later quite distinguished historians of science. Some insisted that Kuhn's main personal interest lay in the history of ideas (and in the philosophy of language and incommensurability), rather than in the study of scientific institutions and scientific communities, much less sociology of knowledge. Fair enough. Nonetheless, Kuhn's call for a history of science that would combine the so-called internal and external approaches was important, and Kuhn's evolving notion of the nature of the paradigm and normal science as what he began calling a "disciplinary matrix" provided historians of science with a powerful tool. With that tool, they have studied and expanded the scope of the history of science by studying in great detail in different times and places the many ways in which scientific traditions in the natural sciences have been codified, transmitted and transformed.

Some of this work has seemed to undermine confidence in the integrity and reliability of science in ways that Kuhn—as well as Polanyi, Fleck, Mannheim and others—certainly did not intend in their emphasis on what they saw as the constructive and stabilizing constraints of scientific tradition, dogmas or thought-models in scientific practice. One of the most widely read early essays to express concern was Stephen G. Brush's 1974 article in *Science* titled "Should the History of Science Be Rated X?" Brush suggested that recent historians' accounts of the way that scientists behave might not provide a good model for science students. Among Brush's examples were recent articles on "fudge factors" in the work of scientific heroes such as Newton, Mendel and Millikan (Brush 1974). Here, as Kuhn had argued, were accounts of great heroes of science who resisted anomalies and discrepancies because of their committed theoretical beliefs.

By the mid-1970s the new field of social studies of science joined the Merton school in influencing the history of science. Whether in Paris or Edinburgh or Philadelphia, science studies paid attention to social determinants of scientific knowledge and its thought-models. The science studies principle of impartiality demanded social explanation, rather than rational explanation, of widely accepted theories in the natural sciences, including the physical sciences. Rather than claiming that a theory is true or false, its acceptance must be explained through understanding the motives and strategies of the producers of knowledge and of dominant social interests, an argument made independently by the philosopher Michel Foucault for viewing disciplinary regimes in the social and human sciences.[1] Was this line of scholarship also reason to rate the history-of-science X? The notion that science is just belief, relatively independent of something like objective empiricism or convergent reality, could legitimate the arguments by science-deniers that theories of evolutionary biology or climate change are scientific dogmas controlled by a power elite within scientific disciplines.

When he was writing *Structure* in the heyday of post-World War II and post-Sputnik public support for science, Kuhn did not foresee such outcomes. Nor did he likely realize just how catchy his book title and his scheme of paradigms and revolutions might become during the 1960s political turmoil of civil-rights, women's-rights, anti-Vietnam-war movements and the Paris and Czech uprisings of May '68. These political developments brought unexpected attention to Kuhn's book on revolutions, along with new commercial possibilities for Berkeley street vendors who began selling bumper stickers that read "Subvert the Dominant Paradigm." Among historians of science, however, a surprising thing happened. Kuhn's notions of the dominant paradigm, sudden rupture and discontinuity were undermined by decades of historical studies combining the so-called internal and external history that he had highlighted as the challenge for the future.

Post-Kuhn historians have built upon Kuhn's notion of tradition, but especially his definition of disciplinary matrices, to study in detail research groups

[1] Bloor (1973); Barnes and Bloor (1982); Collins (1992); Foucault (1967, 1970).

and schools, laboratories and instruments, periodicals and textbooks, techniques of pedagogy, development of scientific lexicons, scientists' responses to anomalies and scientific controversies. The results, for example in the field of the history of chemistry, have largely undermined Kuhn's claims of sudden and incommensurable change except perhaps for the notion of incommensurable methodology (Chang 2012). Historians have found the long century of Lavoisier's so-called Chemical Revolution to be a period of small and gradual changes in chemistry and a period characterized by continued use of old practices alongside new ones in chemical methods, theories and languages (Holmes 1989; Klein and Lefèvre 2007). Historians of nineteenth-century chemistry have found change to be substantial, but so gradual and so endemic that it constituted what Alan J. Rocke has called a "quiet revolution" (Rocke 1993). Similarly, in mid-twentieth century chemistry, theoretical frameworks as competitive and different as Linus Pauling's atomic valence-bond theory and Robert Mulliken's molecular-orbital framework have turned out to be complementary despite the different premises, languages and tools of the two paradigms (Brush 1999).

These kinds of results in the history of the natural sciences tend to support the gradualist and evolutionary explanation of scientific change that Kuhn briefly broached at the very conclusion of *Structure*, at odds with the book's main argument. In his Rothschild Lecture at Harvard in 1991, Kuhn may have surprised some people in his audience by saying that: the "episodes that I once described as scientific revolutions are intimately associated with . . . speciation" that produces a "variety of niches within which the practitioners of these various specialties practice their trade" (Kuhn 1992). Detailed historical studies of developments in physical chemistry, solid state physics, molecular biology and other "hybrid" fields that have emerged alongside older disciplinary matrices seem to confirm this gradualist interpretation, as do many philosophical studies.

Our histories of science now differ greatly from those familiar to Thomas Kuhn at the time that he published *Structure*. For one thing, they are less heroic. For another, they rarely take the form of simply tracing materially and culturally independent or disembodied ideas. Our histories mostly are finely grained in their timelines and locales, as we analyze the investigative pathways and social settings within which science has been practiced and as we study its cultural meanings. Our histories also reflect recent changes in science. Big Science has become even bigger. The numbers of women in the sciences have greatly increased, as have the numbers of non-European scientists working in the traditional Western bloc and outside that bloc. The assistants and technicians aiding scientists have become more numerous and more visible. The distinction between fundamental and applied scientists has become harder to make. Correspondingly, we have made these people visible in our histories of past science and have explained the social mechanisms that long made them absent or invisible, finding continuities between past and present that sometimes surprise us.

In the end, I think that Kuhn's legacy is stronger than sometimes now claimed, although not entirely as he might have wished it to be. It is ironic that the history of tradition rather than revolution became the legacy. The first excitement and the first dissent over *Structure* centered on Kuhn's statement of the dogmatic character of scientific belief (which he incorporated into the notion of paradigm) and scientific revolution as a dramatic process of historical discontinuity between two incommensurable paradigms. Revolution was a catchword in the 1960s. The next generations of historians of science mostly disconfirmed the thesis of rupture and discontinuity in favor of gradualism and continuity, as they restudied the so-called scientific revolutions and focused on the everyday practices and everyday scientists of what Kuhn called the traditions of normal science. The idea of the influence in science of tradition and belief is no longer heretical. For this we owe Kuhn and others a considerable debt for giving us conceptual tools that have expanded the history of science away from the heroic and into the ordinary practices of science, however fallible but also committed its practitioners may be.

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Chapter 22
Experimental Turnaround, 360°: The Essential Kuhn Circle

Carsten Reinhardt

Kuhn uses quite a few examples of experiments in his 1962 *Structure of Scientific Revolutions* (Kuhn 2012). They relate, among other things, to the pitfalls of standard experimental procedures when facing uranium fission (pp. 60–1), to the intertwinement of "factual and theoretical novelty" in the "discovery" of oxygen (p. 53), to the failure of precision apparatus in detecting ether drift (p. 73). In all these cases, however, Kuhn gave prominence to the theoretical side of the scientific enterprise. But he also emphasized the puzzle-solving activities of normal, everyday science and the crucial role of acknowledged experimental methods when he developed the various meanings of paradigm. In doing so, he conceived of scientific communities as carriers of paradigms and understood the paradigm as being the constitutive parameter of a community. Kuhn later recognized the circularity of this argument, pointing to empirical sociological analysis for the determination of scientific communities. However, when it came to the question of explaining the binding forces of such social groups, he kept referring to paradigms, or to the disciplinary matrix.[1]

During the 1970s and 1980s, historians, sociologists and philosophers of science began to move experimentation into the center of their works. At first sight, in an anti-Kuhnian stance, the 'New Experimentalism' put the theoretical, experimental and instrumental dimensions of science on an equal footing.[2] Although these dimensions are interrelated, they are supposed to have a life of their own, according to Ian Hacking's often quoted phrase. Among the more famous, if disputed, claims of the adherents of the 'experimental turn' are the following: Experimentation is largely independent from theory because it is based on the interplay of theory, material things and data. Thus, the autonomy is constructed by reliance on many conditions, not just one. Moreover, at least in modern science, facts are the products of a complex laboratory technology, leading to the "self-vindication of the laboratory sciences" (Hacking 1992). In addition, even if we accept the impact of theory, interpretation of data mainly rests on low-level

[1] Kuhn (1974), see Hacking (2012, xxiv).
[2] One crucial text in this regard is Galison (1997).

concepts, and not on high-level theory. This proclaimed autonomy of the laboratory sciences constituted the discourses of quite a bit of the work done on the history of experimentation, focusing on the "inner laboratory" (Galison 1997, 4), and therefore underlining the inherent momentum of experimental practice.

It would be unjust to reduce the experimental turn to a kind of 'new internalism' in the history of science. Just by citing Hacking's phrase in full ("Experimentation has a life of its own, interacting with speculation, calculation, model building, invention and technology in numerous ways") (Hacking 1983, xiii), one recognizes that the 'new experimentalists' from the beginning took seriously influences from beyond the laboratory walls. This understanding is strengthened when we consider works of an STS bent, focusing on the social construction of scientific knowledge, and the tradition of ethnographic laboratory studies, laying open the multitude of epistemic cultures and their interplay. The history and philosophy of experiment and the STS direction clashed more often than not, especially when it came to questions of scientific (entity) realism or of Actor-Network-Theory. They share, however, an attitude that emphasizes the role of practice and gives particular attention to the interactions of epistemic cultures in building up Kuhn's "scientific communities."

So far, we may get the impression that both HPS and STS, Kuhn's most powerful heirs, parted company with the fundamental argument in *Structure*, according to which it is the accepted, "unprecedented" and "open-ended" achievement of scientific practice (the paradigm) that creates at the same time both a coherent research tradition and the corresponding scientific community.[3] However, this is not the case.

For example, Hans-Jörg Rheinberger recognizes the drive behind scientific research in a dialectic interplay of epistemic things and technical objects (Rheinberger 1997). The latter constitute the established methods and instruments of an experimental system, and they serve to stabilize the epistemic things, understood as yet unknown entities, in the investigative process. Rheinberger clearly positions his work in a "post-Kuhnian move away from the hegemony of theory" (p. 1). However, he also resists a Heideggerian dominance of technology, as would be smuggled in by terms such as "technoscience." It is the interaction of imagination and technical skill that constitutes the experiment: "Experimental reasoning […] transcends its technical conditions and creates an open reading frame for the emergence of unprecedented events" (p. 31). In explicitly connecting to Kuhn, Rheinberger introduces the notion of experimental cultures, sharing "styles of experimental reasoning" and circumscribing the "informal communities of researchers" (p. 138). Hacking, with his notion of laboratory style, also holds a similar argument. Moreover, both approaches enclose the scientific en-

[3] For Kuhn's argument see Hacking (2012, xxiii).

terprise inside the laboratory walls and thereby shield it from external (mainly technical, but also economic and implicitly political) repercussions.

Even if we accept that the laboratory is not an enshrined space, we have difficulties escaping the Kuhnian circle. Terry Shinn's notion of research technology, for example, explicitly addresses the hybridity, or interstitiality, of the careers of his subjects[4]. Being part of a transversal regime, research technologists move endlessly between the spheres of industry, government and academia. There is no place for an encapsulated scientific community in Shinn's system. The products of research technologists, viz., the instruments and apparatus of modern science, are not only based on advanced technology but have generic qualities as well. Thus, they can be applied in and adapted to many different niches in science (and technology) at the same time, being disembedded and re-embedded in various contexts. Moreover, the appeal of genericity creates an autonomy of research technology, in making it independent from direct pressure toward application and giving rise to an epistemic standing in its own right. Research technology not only answers research questions, but also creates its own research problems. In doing so, it forms its own standardized language, its metrology. It is this latter property that allows research technologists form their own communities, including journals and institutions of their own.

I have just listed the tip of the iceberg of works that can be included under the rubric of new experimentalism. It is evident that their main difference from the Kuhnian picture is their emphasis on the material dimension of science, and their rejection of theoretical hegemony. Most of the approaches underline the stabilization of technical craft, experimental practice and theoretical knowledge during the research process. Only if this was achieved, could the apparatus be trusted, transferred and appropriated, giving rise to a certain style, or mode, of experimental thinking. Many studies focus on standardization and teaching. While this approach embraces the textbook problems of Kuhn's *Structure*, it goes beyond them by including hands-on seminars, the standardization of data, and their interpretation and representation. Some tackle the new social and institutional forms that came with the reliance of science on expensive, high-tech instrumentation. These include laboratories concentrating on the development and dissemination of new methods, and the forming of close alliances between academic scientists and instrument manufacturers. Normal science, in Kuhn's diction a puzzle-solving activity, has been supplemented by the generation of methods for their own sake, which is essentially a puzzle-seeking activity. In my understanding, methods are pathways of research, routinized experiments that both define and enable scientists to solve research problems at hand. Their size and scope can range from technical gadgetry to whole knowledge domains. Methods do

[4]See the contributions in Shinn and Joerges (2001).

structure the inner economy of science, and they serve as connections to technology, economy and politics. In the mid-twentieth century, so my thesis, a novel type of scientists emerged in a triangle of academic science, instrument industry and governmental science funding: the method makers. Method makers focus on "Methods for Methods' Sake" (N.N. 2004, 1), as they develop research techniques for use by other scientists. In doing so, they change the inner economy of science, introducing a certain division of labor, and they affect the prevalent epistemology, turning methods into potential end-products of scientific activity (Reinhardt 2006).

The experimental turn has produced many achievements that have changed our understanding of science in fundamental terms. In analyzing scientific research, it has created a sound balance of theory, experiment and instrument. It has led to the partial break-down of the laboratory walls as a metaphor of the autonomy of the epistemic core. It has even opened the way to a possible alliance of the sociology of knowledge and the philosophy of science. However, I would argue, we have not escaped the Kuhnian trap that connects paradigm and community with a circular argument. Even if we take into account different functions, heuristics and social roles, we stick to this circle. Stressing the simultaneous co-creation of both community and paradigm is perhaps the only thing we should do, as this creates the self-referring system that David Bloor explains so vividly in his contribution. Thus, for scholars of scientific communities and institutions, the "Kuhn circle" described here offers a particular opportunity. It constitutes the link between epistemic activities and social order, and gives rise to studies of institutions that put the epistemic side on an equal footing with the social part. In the case of studies of experiment, this has led to a plethora of types of experimental communities and cultures, enriching the "zoo" of scientific institutions, and especially connecting it to practical, craft-like activities. Moreover, works on archives, libraries, fieldwork, museums and exhibitions have enriched and substantially expanded our view with regard to the classifying, collecting and exhibiting of "scientific" entities. It needs to be seen, however, what can be done in the frame of this thinking when we consider the more general or universal institutions of science, such as the university or research organizations of various kinds. Are they more than containers for the epistemic activities just mentioned? What are their constitutive socio-epistemic norms and values? Certainly, the co-construction of cognitive and social order is at work there, too.

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Chapter 23
History of Science: The French Connection
John Stachel

Why the French Dog Didn't Bark

> *Gregory* (a Scotland Yard detective): "Is there any other point to which you would wish to draw my attention?"
> *Holmes*: "To the curious incident of the dog in the night-time."
> *Gregory*: "The dog did nothing in the night-time."
> *Holmes*: "That was the curious incident."
>
> Sir Arthur Conan Doyle, "Silver Blaze"

Often, the most interesting question is why the dog *didn't* bark. Here the question is: Why did Kuhn's work, which in the 1960s had such a great impact on the Anglophone history and philosophy of science communities, have so little impact on their Francophone counterparts?

In his "Translator's Preface" (Bachelard 1984, xv–xxiv), Arthur Goldhammer raised a reciprocal question:

> English-speaking readers will no doubt interpret Bachelard's place in the history of philosophy in their own way. There are surely anticipations in his work of many ideas that have gained prominence in recent Anglo-American debates on the philosophy of science. But it is un-Bachelardian to look for precursors. A more salutary exercise, as he might say, would be to ask why our philosophizing didn't take a Bachelardian turn. What "epistemological obstacles" stood in the way (to borrow an expression from a work with a title similar to that of the present book: *The Formation of the Scientific Spirit*)? (Bachelard 1984, xxiii–xxiv)

So now I shall rephrase my question: What epistemological obstacles account for the Francophone dismissal of Kuhn in the 1960s? My answer is that there is a French tradition in history and philosophy of science, dating back to

Comte and still flourishing in the 1960s[1], that anticipated—and not infrequently surpassed—a number of the ideas usually attributed by Anglophones to Kuhn, as well as to Karl Popper, Imre Lakatos and / or Paul Feyerabend.

But more important, the branch of that tradition launched by Gaston Bachelard and Jean Cavaillès actually runs counter to the main ideas of the new Anglophone philosophers and historians of science. I shall illustrate this by a discussion of two 1970s critiques of Kuhn by French proponents of that tradition: one by the non-Marxist Jean-Jacques Salomon and the other by the Marxist Dominique Lecourt. Then I shall contrast this with the earlier Marxist critique of Bachelard by Jacques Solomon.[2]

Next I shall give a more positive account of the Bachelard-Cavaillès approach, largely in their words, and of Kuhn's missed opportunity to understand this approach. I hope this paper will encourage an Anglophone *Rettung*[3] of the Bachelard-Cavaillès branch of the French tradition.

A Striking Contrast

Consider the contrast between two books, each based on a conference in the late 1960s. Figure 23.1 shows a well-known volume based on a session of the International Colloquium in the Philosophy of Science held in London, 13 July 1965, which focused on Kuhn's *Criticism and the Growth of Knowledge*. The text, "a rational reconstruction and expansion" of the discussion, does not mention Bachelard, Cavaillès or any other French scholar from the 1920s onward except Henri Bergson, whom Lakatos mentions once, and Alexandre Koyré, whom he praises in three footnotes.[4]

[1]See Jay (1984, 1993) for early discussions of the Marxist strands of the French tradition, and Gouarne (2010) and Carolino (2014) for more recent discussions.

[2]Note that Jean-Jacques Salomon and Jacques Solomon are two quite distinct persons.

[3]"'Rettungen' nannte Lessing eine Reihe von Schriften, in denen er Autoren aus ganz verschiedenen Zeiten und Literaturgattungen öffentlich verteidigte, die das Unglück hatten, von der Mit- oder Nachwelt verfolgt oder verdammt zu sein." Schmitt (2002): "Lessing entitled a series of writings 'rescues,' in which he publicly defended authors, from quite different epochs and branches of literature, who had had the misfortune to be persecuted or condemned by their contemporaries or successors," to which I would only add: or forgotten.

[4]"The people who did most to reverse the anti-metaphysical tide in the philosophy and the historiography of science were Burtt, Popper and Koyré" Lakatos and Musgrave (1970, 183). Lakatos discusses earlier Francophones Duhem, Le Roy and Poincaré at some length, and Comte in a footnote; Paul Feyerabend mentions Poincaré once, and John Watkins mentions Duhem in a footnote.

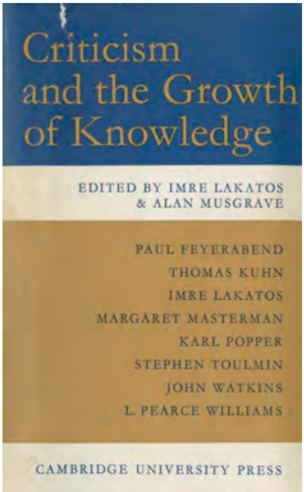

Figure 23.1: Imre Lakatos and Alan Musgrave, eds. (Cambridge University Press 1970).

Figure 23.2: *Les cahiers du Centre d'Études Socialistes, Dialectique Marxiste et Pensée Structurale*.

Several English-language speakers (e.g., Stanley Pullberg, Bertell Ollman, Norman Rudich) participated in the four celebrated "debates," held in Paris between 18 January and 26 April 1967, on which *Les cahiers* (1968) is based (Figure 23.2); yet it is practically unknown in the Anglophone world.[5] Perhaps this is because, while many Italian and German scholars are cited, there is no mention of Kuhn, Popper, Lakatos or any other current Anglophone philosopher.

Mutual Neglect

Based on a conference held at the Max Planck Institute for the History of Science on "Epistemology and History from Bachelard and Canguilhem to Today's History of Science," Schmidgen, Schöttler and Braunstein (2012) contains an important clue to one reason for Francophone neglect of Kuhn. In the only mention of Kuhn in the book, Pierre-Olivier Méthot notes: "[…] for [Canguilhem] Kuhn misconstrues the nature of scientific rationality" (Méthot 2012, 126–127)—an accusation that recurs frequently in the Francophone literature.

Sometimes neglect was replaced by conflation. In 1975 Dominique Lecourt wrote:

> Many French critics—including Marxist ones—have thought that they could detect an accord between Kuhn's theses and the Bachelardian epistemological current. […] To put it plainly: I think this is completely wrong. (Lecourt 1975, 9–10)

Yet the tendency to conflate Bachelard's and Kuhn's views continued in the Anglophone literature. Indeed, (Schuster and Watchirs 1990) is a critique of "the Kuhn / Bachelard problematic," while (Danny 1999) is a defense of "Historical and Constructivist Philosophies of Science: Kuhn, Bachelard and Canguilhem."

But by and large, the work of the French school in general, and of Bachelard and Cavaillès in particular, was still ignored in the Anglophone literature.[6] As early as 1976, Robert D'Amico complained about the neglect of Bachelard:

[5] *Raison présente* (1970) is based on a similar series of articles and debates, held 22–23 and 27–28 February 1968 at the Sorbonne, and originally published in 1967–68 in *Raison présente*. Only Francophone scholars contributed, and again no Anglophone is even mentioned. Halbwachs (1972) is a notable exception. It is a report on a 1968 conference held in Geneva at the Center for Genetic Epistemology, directed by Jean Piaget, at which Kuhn presented a paper. Halbwachs discusses this paper at length, but *Structure* is not mentioned.

[6] In so far as Bachelard was remembered in the English-speaking world, it was largely for the literary aspect of his work rather than the natural-scientific. For early discussions of this dualism, see Roy (1977); Smith (1982).

> His prodigious writings in the history of science, his persistent criticism of philosophy, and his critical studies of epistemology constitute the indispensable background for understanding contemporary French thought, as well as a resource for the history of the sciences which, outside of France, has been inexplicably ignored. (D'Amico 1976, 334)[7]

The first serious attempt to insert Bachelard into the Anglo-American discussion was the publication of (Bachelard 1984), the English translation of (Bachelard 1934). In his "Foreword" (Bachelard 1984, vii–xiii), Patrick Heelan wrote:

> Anglo-American logical empiricism [...] has changed in many ways since Bachelard's death in the early sixties. A number of Bachelardian themes—scientific observations are theory laden, "revolutions" occur in the history of science, science is not value neutral, science is a community endeavor and reflects community interests, among others—are all presently part of the confused picture of science in the new Anglo-American philosophy of science. Although Bachelard's writings considerably antedate these changes, there is little evidence that Bachelard's views were influential to any considerable extent in bringing them about. [...] The principal agents of change within Anglo-American philosophy of science were in fact the critical writings by, to mention just a few, Stephen Toulmin, N. Russell Hanson, Paul Feyerabend, Mary Hesse, Karl Popper, as well as those by the historians of science, influenced by the work of Thomas Kuhn [...] These changes bring Bachelard's work into relevance for the mainstream of current philosophy of science in America and indicate the peculiar importance of some of his contributions—the role of the imagination, epistemology as historical, ontology as the acceptance of a value, science as an effort to produce "epistemological ruptures," the stress on instruments as constituting a "phenomenotechnology." (Bachelard 1984, xii–xiii)

Two books by Martin Jay did much to remedy this neglect (1984, chapters 9–13; 1993), but neither discusses Kuhn.[8]

[7] D'Amico (1999) is an important contribution to remedy this neglect.
[8] Jay (1984, 394, note 38) is only a reference to other sources.

An English translation (1970) of Cavaillès' posthumous major work (1960)[9] was published in (Kockelmans and Kiesel 1970), an anthology devoted to "Phenomenology and the Natural Sciences":

> The goal of this anthology is to give the English-speaking reader a first impression of the contributions made by phenomenology to the vast domain of the philosophy of the natural sciences. (Kockelmans and Kiesel 1970, xi)

Perhaps this association with phenomenology did Cavaillès more harm than good. Michel Foucault states the problem well:

> It seems to me that one could find another dividing line which cuts through all these oppositions. It is the line that separates a philosophy of experience, of sense, and of subject and a philosophy of knowledge, of rationality and of concept. On the one hand, the network is that of Sartre and Merleau-Ponty; and then another is that of Cavaillès, Bachelard and Canguilhem. In other words we are dealing with two modalities according to which phenomenology was taken up in France, when quite late—around 1930—it finally begins to be, if not known, at least recognized. (Foucault 1978, x)

Kockelmans and Kiesel (1970) do not make this opposition clear. At any rate, Anglophone neglect of Cavaillès has been even greater than that of Bachelard.

By the turn of this century, things had begun to change a bit in the Anglophone world—but only a bit. The volume on Kuhn in the series *Contemporary Philosophy in Focus* (Nickles 2003) has an article on "Thomas Kuhn and French Philosophy of Science" (Gutting 2003). In his introduction, Thomas Nickles wrote:

> Gutting explores parallels between Kuhn's account of science and those of the prominent French historical-philosophical tradition, including Brunschwicg, Bachelard and Canguilhem. The French took a historical approach to the intellectual appraisal of science long before Kuhn and the post-Kuhnian historical philosophy of science. In several instances the French thinkers anticipated postmodern insights commonly attributed to Kuhn in the Anglophone world. Gutting suggests that the French tradition provides resources for solving

[9] "Cavaillès was a philosopher of science and mathematics, a critic of Husserl and Kant, and a (twice) decorated hero of the French resistance. The role of his work in the changes that took place in the French philosophical scene after World War II—he was executed in 1944 by a Nazi firing squad—is perhaps unfairly neglected outside of France" Tasić (2001, 84–85).

> Kuhnian problems concerning objectivity, rationality, and realism. (Nickles 2003, 12–13)

Yet there is no mention elsewhere in the volume of these three or of any other figures in this "French historical-philosophical tradition." Quite recently, Knox Peden noted:

> [A]lthough Cavaillès is beginning to procure an intrepid readership keen to understand the role played by the philosophy of mathematics in recent French thought, his student Jean-Toussaint Desanti has garnered scarcely any attention beyond the hexagon. By and large, these philosophers remain unknown quantities to an Anglophone audience. (Peden 2014, 8)

I have not made an extensive study of the German literature, but the following example leads me to suggest that it may not be exempt from similar strictures. In a book entitled *Science as a Historical Process / The Anti-Positivistic Turn in the Theory of Science*, Bayertz writes:

> In the wake of the reception of Kuhn's *Structure of Scientific Revolutions* a historical understanding of science began to prevail, the goal of which is the philosophical analysis of scientific praxis and its transformations in the course of history. Besides Thomas Kuhn, Stephen Toulmin, Norwood Russell Hanson, Imre Lakatos and Paul K. Feyerabend are among the representatives of this anti-positivistic current. (Bayertz 1980, 7)[10]

There is no mention of Bachelard, Canguilhem, Cavaillès, Foucault or any other French source of this "anti-positivistic current."[11]

Two French Critics of Kuhn

In the early 1970s, two French exponents gave accounts of the Bachelard-Cavaillès approach to epistemology that emphasized its differences with the Kuhn approach. One, Jean-Jacques Salomon, was head of the Science Policy Division of the OECD and a non-Marxist. The other, Dominque Lecourt, was a self-described "Marxist-Leninist in the domain of epistemology" (Lecourt 1975, 8). Fortunately, both their accounts are available in English (Salomon 1973; Lecourt 1975). Both were students of Canguilhem, who in turn was a student of

[10] Unattributed translations are by the author.

[11] Here too, the tide has begun to turn more recently. See, e.g., Rheinberger (2005); Schöttler (2012).

Cavaillès; so, while referring often to Bachelard, they acknowledge their debt to Canguilhem rather than directly to Cavaillès:

> Raymond Aron and Georges Canguilhem encouraged a work which was threatened by many professional constraints, and were kind enough to allow him to go forward under the auspices of the Sorbonne. (Salomon 1973, xxii)

> *Gaston Bachelard's Historical Epistemology*, written under the guidance of Georges Canguilhem and published thanks to him, dates from the autumn of 1968. (Lecourt 1975, 8)

Althusser, on the other hand, was directly inspired by Cavaillès, as Warren Montag emphasizes:

> [T]his early essay by Althusser [...] sketches out a critique of both sides of the debates that raged in French philosophy in the 1950s: consciousness or structure, or in Althusser's terms, subjectivism or formalism, both of which positions could be, and often were, defended with citations from Husserl. His critique, although couched in Marxist terms, was in fact drawn from two thinkers whose influence on Althusser was enormous: Jean Cavaillès and Georges Canguilhem. It is Cavaillès in particular who figures most centrally in Althusser's examination of the alternatives around which French philosophy, especially insofar as it addressed the problem of scientific knowledge, appeared to be structured. His most influential work was his last, *Sur la logique et l'histoire de la science*, written in prison in occupied France in 1943. (Montag 2013, 40)

Now I shall turn to the content of the two critiques.

1) Jean-Jacques Salomon

Salomon establishes his debt to Bachelard:

> All contemporary research consists of reciprocal feedback between concept and application, between theory and practice, or, in Bachelard's words between 'the mind which works' and 'the matter which is worked.' In this relation *theoria* is the first instance of *techne*, in time if not in the hierarchical sense, and without its epistemological priority bearing a constant relation to the technical

achievements which justify it; the road to the conquests of science lies through the conquests of technology. (Salomon 1973, 77)

But the tree of science also hides the forest: scientific research does not consist solely in science in the sense of knowledge which is the sole source of applications. *From the most abstract reflection through to development, scientific research constitutes a process whose different elements are so many links in a continuous and retroactive system*.

'The two societies, the theoretical society and the technical society [says Gaston Bachelard, with great truth], are in contact with each other. They cooperate. They *understand* each other.' (Salomon 1973, 81)

What Bachelard said about a rationalized theory of electricity in fact applies to the whole structure of contemporary scientific research:

'The rational and the real must be apprehended together in a veritable coupling in the electromagnetic sense of the word, constantly stressing the reciprocal reactions of rational thought and technical thought.'

This idea of coupling represents movement from the most theoretical to the most applied research and movement back again not as a fortuitous transition from intellectual adventure to technology, but as *the deliberate organization of reciprocal exchange*. […] Scientific research results in discoveries which are also inventions, in inventions which are also discoveries: *It is the deliberate and organized application of human labour to the production of new knowledge, processes and products*. (Salomon 1973, 83–84)

Then he distinguishes this approach from that of Kuhn:

[Jantsch] challenges the whole idea of a 'pure' science with characteristics of such a kind that its evolution cannot in any way be foretold. This is the theory of what he calls the 'encapsulation' of science—its withdrawal into an ivory tower, immune from the pressures of the profane world—and of which he finds, not without reason, one of the best examples in the book by Thomas Kuhn, *The Structure of Scientific Revolutions*. According to Kuhn, scientific progress is made up of two sorts of movement, that of 'normal science' which develops within the limits of established 'paradigms', and that of science in a period of crisis, when the revolution set off by the 'anomalies' of the concepts in use takes the form of strife between the old and the new 'paradigms', until the victory of the new concepts, recognized and adopted, gives rise to a new 'normal science'.

In many respects Kuhn's anomalies recall the 'epistemological obstacles' of Bachelard in *La formation de l'esprit scientifique* (1938, 91). But, just as the idea of 'paradigm' is vague, so that of the 'epistemological obstacle' is precise and rich to the point of being the principle which explains the 'anomalies' themselves (Salomon 1973, 112).

This assuredly 'purist' conception, which rejects all influence over the course of science other than that of its own problems, contrasts at the opposite extreme with the conception of the 'integration' of science in the social system. The empirical course of history takes precedence here over the theoretical consideration of knowledge, as though knowledge had no significance except in so far as it is conditioned by history. *If these conceptions are diametrically opposed, it is not so much because they both alike find illustrations founded on the facts, but rather because they each refer back to irreconcilable ideologies*. We have a dialogue of the deaf, because each camp refers to an objective which could be defined independently of the values it attaches to the objective.

Both positions [said Georges Canguilhem] come down to treating the subject of the history of science as the subject of a science.

Hindsight or foresight, the misapprehension is the same; If one finds, here and there, formulated in terms as absolute as those of the 'idealistic' interpretation, the conception of the total integration of science in society, it is because the subject of reflection is in both cases presumed to be determinable *as the subject of a science*. (Salomon 1973, 113)

2) Dominique Lecourt

Now I turn to Dominique Lecourt. His "Introduction to the English Edition" (1975, 7–19) took up the challenge of describing what he saw as the sharp distinction between the approaches of Bachelard and Kuhn:[12]

> *The New Scientific Mind*, Gaston Bachelard's first great epistemological work, was published in 1934, the same year in which Karl Popper's famous book *The Logic of Scientific Discovery* appeared in Vienna. During the subsequent thirty years the works of the one and the other have been developed, enriched, corrected and broadcast without it ever being possible to register either the beginnings of a confrontation or a sign of any emulation between them. […] However, two recent events [have led to] a serious misunderstanding. These two events are […] the translation of Althusser's works into English and […] the appearance a short time ago of Thomas Kuhn's *The Stucture of Scientific Revolutions* in French. A number of British commentators have seen 'convergence' if not identity pure and simple of the epistemological positions defended by Althusser and by Kuhn. Many French critics—including Marxist ones—have thought that they could detect an accord between Kuhn's theses and the Bachelardian epistemological current. […] To put it plainly: I think this is completely wrong. (Lecourt 1975, 9–10)

But he starts by describing the role of Althusser in current French Marxism:

> A theoretical encounter […] which has brought together, in France, dialectical materialism—Marxist philosophy—and a certain epistemological tradition inaugurated by Gaston Bachelard. The site of this unexpected encounter [is] the work of Louis Althusser […]. Let me say straight away: for more than ten years now this encounter has whipped up an incredible series of political storms in the Marxist camp. On this side of the Channel the whirlwinds of these storms have not yet stopped forming and reforming. […]
>
> [U]ntil 1968 the wind of criticism was set from the right, from 'Garaudyism', from that so-called 'Marxist humanism' […] Althusser was then accused of 'scientism' and 'dogmatism'. These attacks

[12] In the body of Lecourt (1975), there is no mention of Popper, Lakatos or Kuhn. The only philosophers writing in English that he mentions, such as Bernal, Price and Reichenbach (p. 121), are characterized as positivists.

> took as their main theoretical target the notion of 'epistemological break' which he had borrowed from the works of Gaston Bachelard.

> Bachelard had coined it to remind the historians of science, too inclined to continuism, that a science is only installed by breaking with, by *cutting* itself off from its own past; that the object of a science is therefore not an immediate given and does not pre-exist the process of its production. (Lecourt 1975, 7)

> [T]he dominant tendency of the Bachelardian tradition is materialist whereas the tendency 'Popperism' and its variants is, despite certain appearances, frankly idealist. [...] I shall examine two texts in which the proximity of the two traditions might seem flagrant: on the one hand, Bachelard's *The Rationalist Activity of Contemporary Physics*; on the other, *The Structure of Scientific Revolutions*. These two books do indeed seem to be in accord in essential matters. (Lecourt 1975, 10)

Lecourt lists three points of "apparent proximity":

> [B]oth present a *discontinuist* conception of the history of the sciences [...] both present, unevenly developed, a reflection on the scientific division of labour, and its material instances [...]. Finally [...] both speak of the 'normality' of science. (Lecourt 1975, 10)

Then he critically examines them. He starts by explaining Bachelard's approach:

> [W]ith the expression [epistemological value] Bachelard is aiming at a tendency within the philosophy of sciences itself: the positivist tendency. Against the dissertations about the 'value of science' which have been traditional since Poincaré, against the skeptical and relativist professions of faith to which they have given rise, Bachelard invites the epistemologists to take cognizance of the constant emergence of new epistemological values in contemporary scientific practice. [...] The notion of epistemological value thus also has the function of combating what he calls [...] a 'vague relativism' and an 'outmoded scepticism.' (Lecourt 1975, 11–12)
>
> The history of the sciences is the history of the defeats of irrationalism. But the fight is without end [...]. (Bachelard 1951, 27)
>
> Bachelard expresses this thesis a hundred times in his last works: in it he sees what he rightly calls the very *dialectic* of scientific thought.

> Hence one can argue without paradox that Bachelard is defending a position which is both *materialist* and *dialectical* in philosophy. From this position in philosophy he is able to revolutionize the status of epistemology: to institute what I have called a historical epistemology and to demarcate himself radically from every form of positivism. (Lecourt 1975, 13)

Then he turns to Kuhn:

> Kuhn [...] picks up one answer after another to an insoluble question. The very question that Bachelardian epistemology refuses to ask; the question on the repudiation of which it has established its own terrain: the idealist question of the objectivity of scientific knowledges (how is it guaranteed? How is it to be founded?) No doubt Kuhn poses this question in terms that seem 'concrete', current and scientific: there is no question in his work either of a *cogito* or of a transcendental subject, it is a question of 'scientific groups', of laboratories, and it is in this that the book 'speaks' to the scientists of today—better no doubt than Bachelard's works—but it is essential not to be taken in by words: the theoretical core of this work is an old philosophical notion, an idealist question accompanied by the cortege of answers it imposes, in the circle of which Kuhn—and not he alone—has allowed himself to be trapped.
>
> That is why, despite the 'discontinuum' and a few other appearances it seems to me, decidedly, that the two tendencies of contemporary epistemology cannot meet. I repeat: this is because of a reason of position in *philosophy*. The one is, timidly and confusedly but indisputably, ranged in the materialist camp, the other is inscribed in the orbit of idealist philosophies. (Lecourt 1975, 18–19)

Jacques Solomon

As Lecourt makes clear, he was following Althusser in his positive evaluation of Bachelard. But he was not the first French Communist to discuss Bachelard. During the 1930s Jacques Solomon was a prominent physicist as well as a Party militant.[13] He was a close friend of Leon Rosenfeld, visited Bohr several times, and was an advocate of the Bohr-Rosenfeld approach to quantum mechanics and

[13] See Bustamante (1997).

quantum field theory. Ironically, after WWII this would have brought him into direct conflict with the Soviet "Diamatchiki" and their French adherents.

Written in 1942 and posthumously published, (Solomon 1945) is a critique, of (Bachelard 1940) for its idealist conception of science—just the inverse of (Lecourt 1975), which as we have seen defends Bachelard as a materialist. In Bachelard's extensive discussion of quantum theory in the book, there is no reference to Bohr; and this absence may well have influenced Solomon's negative judgment of the book.

> Bachelard sees things backwards. He constructs a labyrinth of concepts in order to try and extract reality from the physicist's head; however the physicist attempts to extract his ideas from reality.
>
> It is thus not quite exact to declare like Bachelard that "the veritable solidarity of the real is essentially mathematical": actually, it is the real which dictates and verifies the mathematical.
>
> Every step forward, every modification of our concepts of the structure of matter thus shows that, contrary to the opinion of Le Roy, for whom "the facts are the *facts*" (an opinion shared by Bachelard, for whom science is a *phenomeno-technology*), one cannot understand the development of science unless one conceives it as the ever more exact reflection of external reality in our minds. (Solomon 1945, 50, 51)[14]

Solomon is here espousing Lenin's reflection theory of knowledge (Lenin 1909, 1947), which only recently had been so devastatingly criticized from a Marxist point of view by the Dutch astronomer and long-time militant Anton Pannekoek (1938, 1975). Solomon concludes by magnanimously recognizing Bachelard's dialectic, and blaming his idealist approach for all the book's errors.

> Following Bachelard on the terrain of contemporary physics, we were thus forced to recognize that the very evolution of that science

[14] "M. Bachelard voit les choses à l'envers. Il construit un labyrinth de concepts pour essayer de tirer la réalité de la tête du physicien, cependant que le physician s'efforce de tirer sa pensée de la réalité. […] [I]l n'est donc pas exact de déclarer avec M. Bachelard que "la véritable solidarité du réel est d'essence mathématique": c'est le réel, en vrai, qui dicte et vérifie le mathématique. […] Chaque pas en avant, chaque modification de nos conceptions de la structure de la matière montrent ainsi que, contrairement à l'opinion de M. Le Roy pour lequel "les faits sont les *faits*" (opinion que rejoint M. Bachelard pour lequel la Science *est* une *phénoménotechnie*), l'on ne peut comprendre le développement de la science si l'on conçoit celle-ci comme le reflet toujours plus exact de la réalité extérieure dans notre conscience."

> refutes his views, and we have seen that this originates in the idealist conception that forms the basis of Bachelard's philosophy; and which has been refuted and continues to be refuted ceaselessly by the progress of physics, as of the other sciences. […] One can only congratulate him for having been able to recognize the manifestations of a dialectic in modern physics, of which we have given several examples. But, in our opinion, precisely because his philosophy is idealist he does not reach a correct recognition of its fundamental features. (Solomon 1945, 54–55)[15]

Bachelard and the "Epistemological History of the Sciences"

Now I shall try to present more positive, less polemical accounts of what Bachelard and Cavaillès actually accomplished.[16] Shortly after Bachelard's death in 1962, Georges Canguilhem penned the following tribute:

> In so profoundly renewing the meaning of the history of the sciences by rescuing it from its previously subordinate position, in promoting it to the position of a first-rank philosophical discipline, Gaston Bachelard did more than clear a path: he set a goal. (Canguilhem 1963)[17]

Dominique Lecourt and Michel Pêcheux and Étienne Balibar elaborate:

> For almost a quarter of a century, Bachelardian epistemology has consisted of close attention to the contemporary progress of the physical and chemical sciences, an incessant polemical vigilance with respect to philosophical theories of knowledge; and, as the fruit of these combined interests, a progressive rectification, through a constant "self-polemic," of its own categories. […] [T]his "historical epistemology" opened the way for a new discipline, in which

[15] "En suivant M. Bachelard sur le terrain de la physique contemporaine, nous sommes donc contraints de reconnnaître que l'évolution même de cette science infirme ces vues et nous avons reconnu que l'origine en est dans la conception *idéaliste* qui est au fond de la philosophie bachelardienne et qu'a réfutée et qui réfute sans cesse le progrès de la physique, comme des autres sciences. […] On ne peut que le féliciter d'avoir su reconnaitre dans la physique modern les manifestations d'une dialectique dont nous avons plus haut indiqué quelques exemples. Mais justement parce que sa philosophie est idéaliste, notre auteur n'arrive pas, à notre avis, a en reconnaître correctement les traits fondamentaux."

[16] See Bachelard (1971) and Ginestier (1968) for surveys based on extensive excerpts.

[17] "En renouvelant ainsi profondément le sens de l'histoire des sciences en l'arrachant à sa situation jusqu'alors subaltern, en la promouvant au rang d'une discipline philosophique de premier rang, Gaston Bachelard a fait plus de que frayer une voie, il a fixé une tâche."

> others since then have taken part: "the historical epistemology of the sciences." (Lecourt 1972)[18]

> In the historical process of formation of scientific physics, the point of "no-return" (to use F. Regnault's expression), with which that science begins, is called the *epistemological break*. […] The term "point of no-return" constitutes a taking of sides in the polemic in epistemology and history of science between a "continuist" position (Brunschvicq and the permanent play of the human mind in science, Duhem and the question of precursors […]) and a "discontinuist" position that may be conveniently designated by the names of Bachelard and Koyré. (Pécheux and Balibar 1969, 8–9)[19]

Cavaillès, Canguilhem, and Foucault

Bachelard always felt very close to Cavaillès and in (Ferriéres 1950) paid tribute to his fallen friend and ally:

> And we had our projects: jointly defending rational thought, returning philosophy to the demands of testing. I admired the rigor of a philosophy that was intended to be demonstrative. Even in abstract thought, Jean Cavaillès had a heroic willpower. (Ferriéres 2003, 137)[20]

> The work of Jean Cavaillès cannot be summarized. One cannot even single out its general characteristics because all of its chapters and

[18]"[L]'épistemologie bachelardienne […] se constitue d'une attention tendue, pendant près d'un quart de siècle, aux progrès contemporains des sciences physique et chimique, d'une vigilance polémique sans défaillance à l'égard des theories philosophiques de la connaissance, et, fruit de ces interets combines, d'une rectification progressive, dans une "autopolémique" constant, de ses propres catégories. […]. [C]ette "épistemologie historique" ouvrait le champ d'une nouvelle discipline, où d'autres, depuis, se sont engages, "l'histoire épistémologique des sciences"."

[19]"Dans le processus historique de formation de la physique scientifique, on appelera coupure épistémologique le point de "non-retour" (selon l'expression de F. Regnault) à partir duquel cette science commence. […] Le terme de "point de non-retour" constitue une prise de position dans la polémique qui oppose en épistémologie et en histoire des sciences une position "continuiste" (Brunschvicq et le spectacle permanent de l'esprit humain present dans la science; Duhem et la question des précurseurs […]) à une position "discontinuiste" qu'on peut désigner commodément par les noms de Bachelard et de Koyré."

[20]"Et nous fîmes des projets: défendre ensemble la pensée rationelle, rappeler la philosophie aux exigences de la preuve. J'admirais la rigeur d'une philosophie qui se voulait demonstrative. Déja dans la pensée abstraite, Jean Cavaillès avait une volonté de héros."

> even its pages were written with a will to give only the *essence* of his thoughts. Nothing is superfluous, nothing even explanatory in such an exposition. […] By working in such a compact style, Cavaillès was acting in obedience to an ideal. […] For Cavaillès, *every pure thought* had to be a *sure thought*, discursively attached to his criteria. […] For him, a rationalism that pursued a slow historical growth did not suffice. He believed that contemporary mathematical science installed us straight off in abstract, autonomous thought. […] In Cavaillès' thought, the same condemnation liquidated psychologism and historicism. He wrote, in a formulation of marvelous density, "Nothing is so little historical […] as the history of mathematics." (Ferriéres 2003, 238–239)[21]

Even posthumously, Cavaillès played a major role. In 1967, François Chatelet spoke of:

> This epistemological movement of which, in the natural sciences, works such as those of Cavaillès, of Koyré, of Bachelard and of Canguilhem have given us the analysis. (*Raison présente* 1970, 277)[22]

The fullest account of Cavaillès' role in mathematics is (Sinaceur 1994). Tasić (2001) gives the best discussion in English of Cavaillès' significance today:

> [I]f we think of Hilbert's plan in terms of its self-proclaimed proximity to Kant, then we can think of Cavaillès' philosophy of science as, so to speak, doing a little Hegel on Hilbert's "Kant." […] But first let me introduce Cavaillès and indicate why I think he can be viewed as bridging the great divide between Hilbert's formalism and certain parts of postmodern theory. […] I will concentrate on his influence on Foucault, but it extends much further than that. Skipping over unnecessary details, let me simply say that Cavaillès'

[21] "L'oeuvre de Jean Cavaillès n'est pas une oeuvre qu'on puisse rêsumer. On ne peut même pas en dégager les caractères généraux, car tous les chapitres, et les pages elles-mêmes, ont été écrits avec une volonté de ne donner que l'essence des pensées. Rien de superflu, rien même d'explicatif, dans un tel exposé. […] En travaillant d'une manière aussi serrée, Cavaillès obéissait à un ideal. […] Pour Cavaillès, *toute pensée pure* devait être *pensée sure*, pensée discursivement attachée à ses critères. […] Une rationalisme suivant une lente croissance historique ne lui suffisait pas. Il estimait que la science mathématique des temps modernes nous installait d'emblée dans une pensée abstraite, autonome. […] Une même condamnation, dans la pensée de Cavaillès, liquidait le psychologisme et l'historicité. Il a écrit, dans une formule d'une merveilleuse densité:"Il n'y a rien de si peu historique […] que l'histoire mathématique."

[22] "[C]e movement épistémologique dont […] dans les sciences de la nature, des travaux comme ceux de Cavaillès, de Koyré, de Bachelard et de Canguilhem nous ont donné l'analyse."

work helped remove the "spell" that the intuitionist Bergson and the existentialist Sartre cast on French philosophy.

Cavaillès […] spoke of "science of science," blurred meta-mathematics into mathematics, and maintained that the truth is *in* the demonstration, *in* the method itself. […] He went after the "philosophy of the subject" in general, especially Husserl's and Kant's ahistorical intuitions. Cavaillès's science of science appears to be blessed with a "Hegelian" slant, which is clear from his rejection of the pure / applied dualism, his critiques of the philosophy of individual consciousness, and his concern for change and movement that constitute the structure through which science manifests itself *to itself*. This is not unlike Hegel's spirit, which through a dialectical movement comes to know itself *as* that very movement.[23]

Science cannot be reduced to the intentions of individual scientists, but is an entity in itself. Applying this to the particular case of mathematics, we get the following picture. A theorem is not true because *someone* got an idea and then applied the universal, immutable laws of logic or mathematics, thereby proving the theorem. Rather, the "truth" of the theorem is in its very demonstration, which represents a necessary movement within the structure of science itself. "The true meaning of a theory is not in what is understood by the scientist, he wrote in *On Logic and the Theory of Science*, "but in a conceptual becoming that cannot be halted."

Following this idea, we come across Cavaillès' line that scientific progress is not a history of accumulation of truths but a perpetual revision through deepening and erasure. On this view, the task of historians of science is to study the *constitution* of truth as a historical concept within an era, rather than to study what was

[23]Brendan Larvor (1998) has drawn attention to the parallels between Cavaillès's and Lakatos' views of mathematics. In 1938 Cavaillès wrote to a committed Marxist friend: "*Quoique philosophiquement je ne sois pas orienté par le materialism dialectique* […] *je t'avais déjà dit que je me trouvais conduit à des resultants qui ne sont peut-être pas tellement exclus par votre attitude*" (Gouarné 2010, 369), which notes that the letter is cited by H. Mougin, "Jean Cavaillès," *La Pensée*, 4, juillet-septembre 1945, p. 70–83, p. 79. "Although philosophically I am not oriented by dialectical materialism […] I have already told you that I find myself led to results that are perhaps not excluded by your point of view."

> believed to be true in that era.
>
> There is a strong historical link between Cavaillès and Foucault. Foucault himself acknowledges his debt to the French historian of science Georges Canguilhem, his mentor. Canguilhem, on the other hand, admired Cavaillès's work and personal courage—even wrote a book about him, *Vie et mort de Jean Cavaillès*—and was one of the people whose influence paved the way for postmodern theory in the somewhat rigid world of the Parisian academia. (Tasić 2001, 84–86)

The Bachelard-Cavaillès Approach

Finally, I shall let Bachelard explain, with some excerpts from (Bachelard 1950) what I call the Bachelard-Cavaillès approach to the natural sciences:

> As Cavaillès says, "Every observation must be changed into a demonstration." [...] This epistemological substitution is ever more necessary as, with the infinite, science approaches a domain where verification is not possible. "Through a revolutionary reversal, it is number that is driven out of the realm of perfect rationality, and the infinite which comes into it." (Cavaillès 1970, 370). "Perhaps for the first time," with Bolzano "science is no longer considered as a simple intermediary between the human mind and being in itself, depending as much on one as on the other and not having its own reality. Now science is regarded as an object *sui generis*, original in its essence, autonomous in its movement."
>
> Can one conceive of a better formula for defining the new "meta-science" situating scientific knowledge in its specific being, in its independent being! Henceforth, science is a human creation, about which the human mind should be learning, be constructing. No longer could it be accepted naively, no longer could it be developed empirically, even were it an empiricism of intellectual discoveries. Its unity is always in movement: "Since it is a question here not of a scientific ideal, but of realized science, incompleteness and the requirement of progress are part of its definition."
>
> In passing Cavaillès notes the philosophical weakness of an epistemology that believes the sciences may be characterized as

> hypothetico-deductive systems: "How can a principle or a union of principles, which in their content and in their totality are not themselves intelligible, be the starting point for an intelligible development? The heterogeneous alliance of a verified pure concrete and a mode of rational sequence is a simple image without thought."
>
> So here is the problem of a theory of science for a contemporary philosophy of science: to understand science in its creative process, "to find this structure again, not by description but apodictically, insofar as it elaborates itself and demonstrates itself. In other words, the theory of science is an a priori, not prior to science but the soul of science." (Ferriéres 2000, 371–2, 374)[24]

"Kuhn's Missed Opportunity"

A number of critics of Kuhn have emphasized the priority of the French tradition in philosophy and history of science. Gary Gutting discussed this issue in his articles (1990;[25] 2003) and book (2001). In "Continental Philosophy and the History of Science," he distinguishes this tradition from the other major trends:

[24]"Il faut changer, comme dit Cavaillès, "toute constatation en demonstration." […] Cette substitution épistémologique est d'autant plus nécessaire que la science aborde, avec l'infini, un domaine où l'on ne peut constater. "Par un renversement révolutionnaire , c'est le nombre qui est chassé de la rationalité parfaite, l'infini qui y entre." Pour la première fois peut-être avec Bolzano, "la science n'est plus considérée comme simple intermédiaire entre l'esprit humain et l'être en soi, dépendant autant de l'un que de l'autre et n'ayant pas de réalité propre, mais comme un objet sui generis, original dans son essence, autonome dans son movement."
Peut-on concevoir meillure formule pour définir la nouvelle "métascience" posant le savoir scientifique dans son être spécifique, dans son devenir indépendant! La science est, désormais, une création humaine sur laquelle l'esprit doit s'instruire, se construire. On ne peut plus le recevoir naïvement, on ne peut plus la développer empiriquement, fût-ce comme un empirisme des trouvailles spirituelles. Son unité est toujours en movement: "Comme il ne s'agit pas ici d'un idéal scientifique, mais de la science realisée, l'incomplétude et l'exigence de progrès font partie de la definition."
Cavaillès note au passage la faiblesse philosophique d'une épistemologie qui croit pouvoir caractériser les sciences comme des systèmes hypothético-déductifs:"Comment un principe ou un réunion de principes qui, dans leur contenu et dans leur assemblement, ne sont pas eux-mêmes intelligibles, peuvent-ils être point de départ pour un déroulement intelligible? L'alliance hétérogène d'un concret pur constaté et d'un enchainement rationel est simple image sans pensée."
Voici donc le problème d'une théorie de la science pour une philosophie de la science des temps modernes: il faut appréhender la science dans son procès créateur, en retrouver la "structure non par description, mais apodictiquement en tant qu'elle se déroule et se démontre elle-même. Autrement dit, la théorie de la science est un a priori, non antérieur à la science, mais âme de la science"."

[25]Most of the article is devoted to section 3: "The French Network: Bachelard, Canguilhem and Foucault" Gutting (1990, 133–146).

The phenomenological and Marxist approaches to science discussed so far have operated on a rather high level of philosophical generalization and have paid little attention to specific episodes in the history of science. There is, however, a major twentieth-century French approach to the philosophy of science that is deeply and firmly rooted in the history of science. This approach is closely tied to a long French tradition in the history and philosophy of science that began with Comte and was continued in the work of Duhem, Poincaré, Meyerson and Koyré. The central figures of this approach are Gaston Bachelard, who developed his views on science in a series of books published from the 1920s through the 1950s, and Georges Canguilhem, Bachelard's successor as director of the Institut d'Histoire des Sciences et des Techniques at the University of Paris. […] Although not so well known outside of France, Bachelard and Canguilhem have provided a major alternative to both the phenomenological and the Marxist approaches to science.

Because of his demand that the philosopher of science work from the historical development of the sciences, the centre of Bachelard's philosophy of science is his model of scientific change. This model is built around three key epistemological categories: epistemological breaks, epistemological obstacles and epistemological acts.

Bachelard employs the concept of epistemological break (*rupture*) in two contexts. First, he uses it to characterize the way in which scientific knowledge splits off from and even contradicts common-sense experiences and beliefs. […] The second sort of epistemological break is that which occurs between two scientific conceptualizations. For Bachelard, the most striking and important of such breaks came with relativity and quantum theory, which he saw as initiating a 'new scientific spirit'. This 'new spirit' involved not only radically new concepts of nature but also new concepts of scientific method (e.g. new criteria of scientific adequacy. Bachelard's detailed treatments of this topic preceded by two or three decades similar discussions by Anglo-American historians of science such as Kuhn and Feyerabend. (Gutting 1990, 133–4)

Kuhn himself, unfortunately, had only a glancing contact with this tradition and no serious understanding of it. The main contact came

> through Koyré.

> Kuhn was, like so many historians of science of his generation, strongly influenced by Alexandre Koyré, but the influence was primarily historiographical not philosophical. (Gutting 2003, 45, 63)

Kuhn himself comments on Koyré:

> Trained as a philosopher and historian of philosophy, Koyré's transition to the history of science was marked by the publication in 1939 of his three brilliant *Études galiléennes*. Within a decade […] they and his subsequent work provided the models which historians of science increasingly aimed to emulate. More than any other single scholar, Koyré was responsible for the first stage of the historiographical revolution […] Koyré showed how sympathetic and extended *explications de textes* could transform our image of the Scientific Revolution of the seventeenth century and of the men who made it. (Kuhn 1970, 67–68)

> [S]hortly before Alexandre Koyré died […] I had a last letter from him. […] He said, "I've been reading your book," and I don't know what adjective he used, but it was a thoroughly agreeable one. He said, and again I had not seen this coming—when I thought about it, I thought he was right—he said, "You have brought the internal and external histories of science, which in the past have been very far apart, together." Now, I hadn't thought of that at all as what I was doing. I saw what he meant, and coming from him it was particularly agreeable because he had been so anti-external history; his gifts were as an analyst of ideas. And that made an impression, or at least it pleased me tremendously. (Kuhn 2000, 286)

Koyré advised Kuhn to visit Bachelard. Gutting summarizes what happened:

> Koyré […] urged him to meet Bachelard and provided a letter of introduction. The upshot, as Kuhn tells the story, was more a comedy of errors than a meeting of great minds. To begin with, Kuhn had the idea—no doubt vaguely based on information about Bachelard's interest in the literary imagination—that he was an expert on English and American literature and so would surely speak English. "I assumed he would greet me and be willing to

> talk in English." But, although Kuhn opened with "My French is bad, may we talk English?," the "large burly man in his undershirt [who] came to the door […] made me talk French." We can well understand that, as Kuhn puts it, "this all didn't last very long."
>
> Kuhn says he later read a bit more of Bachelard and thought he was on to something but that his thought was too constrained by preset categories: "he had categories, and methodological categories, and moved the thing up an escalator too systematically for me." Nonetheless, Kuhn concluded, "there were things to be discovered there that I did not discover, or did not discover in that way." Here, at least, Kuhn's judgment of Bachelard was correct. There are substantial similarities in the approach and problems of Kuhn's philosophy of science and those of Bachelard's tradition, and these similarities can sustain a mutually fruitful dialogue, even though the exigencies of history prevented it from actually occurring between Kuhn and Bachelard. (Gutting 2003, 45–46)

As Castelão (2004) puts it, this was "Kuhn's missed opportunity." As noted in the Preface to (Tiles 1984), this places Bachelard and Kuhn in an even broader context:

> The reception of [Bachelard's] thought could not have been predicted. On the one hand, although Bachelard was no Marxist, various of his views were appropriated by a whole generation of Marxists. On the other hand, the impact he made on analytical philosophy in the central area of his interest, philosophy of science was, quite unjustly, negligible. His historicism, which preceded that of Kuhn or Foucault, has never been properly discussed. […]. Mary Tiles shows in this book how Bachelard's views are related to the concerns which analytic philosophers have about the status of science and rationality and their debates concerning realism, operationalism and relativism. […] Dr. Tiles makes this clear by relating many of Bachelard's arguments to those of thinkers like Putnam, Lakatos, Feyrabend or Van Fraasen. (Martin 1984, xi–xii)

Conclusion

Mary Tiles has published a remarkable defense of the historical epistemology / epistemological history of Gaston Bachelard and Georges Canguilhem against a direct assault on it by Bruno Latour (Tiles 2011). I shall not attempt to summarize her argument, but simply urge everyone to read it and ponder its message:

> [H]istorical epistemology, as defined by Bachelard and Canguilhem and extended by Bourdieu [has much to contribute to] problems that require engagement with the politics of nature, with the politics of the sciences of nature and with the epistemological challenges associated with the need to deploy multiple disciplines in the service of complex, practical, policy relevant problem solving. (Tiles 2011)

In several earlier papers, I have tried to help develop a new view of the social role of scientific knowledge (Stachel 1974; 1994; 1995; 2003; 2012). I can only hope that this article may contribute modestly to an *aggiornamento* of the Bachelard-Cavaillès tradition.

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Chapter 24
The Professionalization of Research on the History of Science in China and the Influence of Eurocentrism on Chinese Historians of Science

Baichun Zhang

Kuhn's publication of *The Structure of Scientific Revolutions* exerted a significant influence upon the historiography of science. However, prior to the 1980s, its impact on Chinese studies on the history of science was limited, despite the fact that research on the history of science began at the end of 1910s in China. This article focuses on the process of the professionalization of history of science, as well as the influence of Eurocentrism on historical research in main land China.

Early Motives for Chinese Scholars' Research on the History of Science

Historiography was a well-developed subject in pre-modern China, and included certain contents related to astronomy, geography etc. As a result, Yuan Ruan (1764–1849) compiled *Biographies of Astronomers and Mathematicians* in 1799. By the late nineteenth century, Western missionaries in China, such as Alexander Wylie (1815–1887), Joseph Edkins (1823–1905) and William Alexander Parsons Martin (1827–1916), as well as Sinologists, like Stanislas Julien (1797–1873) and Wilhelm Schott (1802–1889) had started to study Chinese scientific traditions.[1] They paid attention to the development of science in China, as well as the relationship between science and politics, economy and society. Their works exerted an influence upon Chinese scholars.

It wasn't until the twentieth century, under the influence of modern learning, that the significant transition of historiography occurred in China. The history of science began to capture the attention of Chinese scholars, and gradually become a "specialized subject." Some Chinese scholars, especially those who were not only proficient in science but also fond of history, became especially interested in

[1] See, for example, Iwo Amelung. *Sinology and the History of Science—Some examples from Frankfurt University*. Presented at the Institute for the History of Natural Sciences, CAS, on September 18, 2012: (http://english.ihns.cas.cn/ns/am/201209/t20120917_91084.html), accessed 16 September 2015.

studying the history of science. As a teenager, Yan Li (1892–1963) read *Biographies of Astronomers and Mathematicians*, but was not satisfied with its content and organization. Later, when he noticed that some foreigners mentioned Chinese mathematics in their writings, he sighed that "traditional Chinese knowledge will be over" (Li 1917). Therefore, in his twenties, he tried to write *A History of Chinese Mathematics*. Baocong Qian (1892–1974) also read ancient Chinese books on mathematics in his twenties, so took to studying the history of Chinese mathematics in depth, writing scholarly works on the subject (Qian 1935).

At the same time, foreign historians of science had a clear impact on diverting the attention of Chinese scholars towards science in history. Co-ching Chu (1890–1974) went on to study at Harvard University in 1910, where he was influenced by George Sarton (1884–1956). He began to publish his papers on the history of science in English in 1918 (Guo 2008). After returning to China, he continued writing articles on the history of science, such as *The Reason Why Experimental Science Was Not Developed in Ancient China* (1935). Some of Sarton's works also came to be translated into Chinese, and were accepted by Chinese historians and scientists. In 1941, Zishui Mao (1893–1988) published an article introducing Sarton's *The History of Science and the New Humanism*. He strongly advocated setting up departments of history of science in universities. Having read Sarton's *The History of Science and the New Humanism*, Baocong Qian published a book review on it in 1947.

In the 1950s, the Communist Party's ideology in the context of the Cold War had am impact on almost all Chinese scholars on the mainland, including scientists and historians of science. Patriotism caused people to pay more attention to the study of scientific discoveries and inventions in China's past. In the early phase of the Korean War, *The People's Daily* published a series of articles on scientific achievements in ancient China, which met the social needs of "advocating patriotic education and criticizing blind faith in foreign things." Co-ching Chu, a former vice president of the Chinese Academy of Sciences, became the most important advocate of the history of science and technology. He wrote articles that emphasized the great contribution of pre-modern Chinese astronomy and meteorology to the world. In August 1954, as one of the leading scientists in China, he published an article entitled "Why Study the History of China's Ancient Science," in which he said:

> Thirty years ago, a bourgeois idealist philosopher Alfred North Whitehead (1861–1947) made the following statement in terms of the contribution that ancient Chinese art, literature, philosophy and natural sciences had made to the history of world human culture: "There is no reason to doubt the intrinsic capacity of individual Chinamen for the pursuit of science. And yet Chinese science is

> practically negligible."[2] Whitehead's subjective and biased conclusion is obviously untrue. This question can only be answered after studying the specific facts in history [...] As we know our ancient history has left rich heritage in natural sciences, therefore, they should be categorized, comprehensively analyzed and summarized [...]
>
> Scientific materials in history can not only boost economic construction, but also facilitate basic theoretical research on the basic disciplines [...]
>
> The important issue is not which happened first, but the influence of those invented or transmitted during cultural exchange on the people. [...]
>
> History of natural sciences is a part of the cultural history. Works of world history published in capitalist countries were imbued with the fascist ideology of "western nations are the best nations," while Chinese culture was seldom mentioned. Ancient history of Chinese natural sciences resembles a barren countryside but filled with treasures. It is the responsibility of historians and natural scientists to discover the treasures, whether for patriotism sake or the sake of internationalism. (Chu 1954, 3)

Generally, the first generation of Chinese historians of science, most of whom followed a career path from being a scientist to becoming a historian, wanted in the first instance to discover the science that existed in pre-modern China. At least some of them argued for Chinese contributions to science and invention in order to overcome or refute Eurocentrism or the centrism of Western culture (Zhang 2001). In their opinion, Joseph Needham (1900–1995) was an important ally in this regard.

The Professionalization and Institutionalization of Research on the History of Science

The professionalization and institutionalization of research on history of science was carried out by national scientific institutions throughout the 1950s. In 1952, entrusted by the president of the Chinese Academy of Sciences (CAS) Moruo Guo (1982–1978), Co-ching Chu called together some scientists to discuss research in the fields of history of science and technology (Xi 2002). In 1954, CAS set up

[2] Whitehead's original text, see Whitehead (1926).

the Research Committee for the History of Natural Sciences in China, which consisted of seventeen scholars, and a research group for the history of natural sciences at the Second Institute of History. In the same year, the Chinese Academy of Agricultural Sciences set up the Research Division of Chinese Agricultural Heritage. In February 1956, Co-ching Chu convened another meeting of scholars to discuss how to promote studies in the history of science. In July of the same year, the First Conference on the History of Science in China took place in Beijing, at which scholars discussed the national plan for research on history of science that belonged to the Long-Term Program for Developing Sciences and Technology Between 1956 and 1967, launched by the State Council. According to the Long-Term Program, CAS, Academy of the Traditional Chinese Medicine, Chinese Academy of Hydraulics, Chinese Academy of Building Research and a few universities established research institutions that conducted professional research on the history of science, technology and medicine.

In September 1956, Co-ching Chu led a three-member Chinese delegation to the eighth International Congress for the History of Science in Italy. On September 9, the People's Republic of China was accepted as a member of the International Union of History and Philosophy of Science. Through Chu's efforts, CAS established the Research Division of History of Chinese Natural Sciences on 1 January 1957, where Yan Li, Baocong Qian and their colleagues became professional historians of science and started to train graduate students. The Research Division, which was headed by Yan Li, produced the first issue of the journal Annual of History of Science in 1958. Unfortunately, research on the history of science all but stopped for a decade after 1966, during the Cultural Revolution. In 1975, the Research Division was renamed the Institute for the History of Natural Sciences (IHNS), and since then it has played a flagship role in the field of history of science in China (Xi 1997).

With the commencement of the policy of reform and opening up to the world in 1978, research on the history of science was quickly revived. The Chinese required an understanding not only of the pre-modern scientific traditions, but also the history of modern science and technology in the West. Historians of science from CAS were invited to give the leaders of the central government a lecture on the modern history of science and technology in 1980. The politicians were very interested in the key roles played by science and technology in economic and social modernization. In such an environment, many scholars and scientists were attracted to history and philosophy of science, resulting in the creation of the Chinese Society for the History of Science and Technology (CSHST) in 1980. Twenty-five years later, the IHNS and CSHST succeeded in hosting the 22nd International Congress of History of Science in 2005.

Since the 1990s, the institutionalization of teaching and research of the history of science has been developing quickly in Chinese universities. In 1999, for example, Shanghai Jiao Tong University established the Department of History and Philosophy of Science in collaboration with the IHNS, while the University of Science and Technology of China set up the Department for the History of Science and Scientific Archaeology. Not long afterwards, Inner Mongolia Normal University established the Department of the History of Science and Scientific Management.

Methodology in the Field of the History of Science in China

Qichao Liang (1873–1929), one of the most important Chinese scholars of early modern times, wrote in his article "The New History" (first published in 1902):

> History is the most extensive and essential branch of knowledge. It is the mirror to the citizen, and is the source of patriotism of a nation. Now, a half of the reason why nationalism is well developed in contemporary Europe and why European countries are making progress in civilization, belongs to the contribution of the study of history.
>
> If now we want to advocate nationalism and let our 400 million compatriots gain a strong standing on this world, in which the superior wins and the inferior loses, national history should be a subject everyone must pursue, no matter they be old or young, male or female, intelligent or unintelligent, worthy or unworthy. (Liang 1936)

In the same article, Liang says: "History is a branch of knowledge to narrate progressive development." Yan Li, the historian of science, expressed a similar social Darwinist opinion in 1930: "History is a branch of learning for research on the progressive evolution of people, and the history of mathematics is a branch of learning for research on the progressive evolution of mathematics" (Li 1931, 1).

The first generation of Chinese historians of science received a modern science and technology education, that is, they were trained in a discipline of science or technology. They approached the subject from the perspective of modern science, beginning their research on the history of the field with which they were familiar, in accordance with modern discipline criteria. They selected and analyzed historical sources and archaeological finds, and revealed scientific discoveries or technological inventions in order to construct the so-called history of ancient "disciplines." They spent a great deal of energy in solving the problem of what existed historically in the field of science and technology? They constructed a research framework or criteria on the basis of Eurocentric modern sciences, yet they argued in favor of Chinese culture, reconstructing knowledge in ancient China to disprove Eurocentrism.

Some advocates hoped that historians would focus not simply on science and technology, but also their social context. In the foreword to the first issue of *Annual of History of Science*, Co-ching Chu writes: "The mission of historians of science is not only to record scientific achievements of a particular era, but also to point out the cause and effect, backdrop and the reasons why such an achievement appeared in some society during some era rather than others" (Chu 1958). In fact, the first generation of Chinese historians of science did not succeed in accomplishing this mission. Joseph Needham made comparatively more contributions in this aspect.

In the 1980s, Chinese scholars, and even the public, were very interested in the so-called "Needham Puzzle" of why modern science did not originate in China (or India) but only in Europe? This puzzle encouraged Chinese historians of science to make further studies of Chinese traditional science as well as the origins and development of modern science in the West. Western scholars' works on history of science and technology, philosophy of science and sociology of science began to be translated into Chinese.

The "scientific revolutionist" Thomas S. Kuhn (1922–1996) came to the attention of Chinese scholars in the 1980s (Wu 2012). *The Structure of Scientific Revolutions* was translated into Chinese and published in 1980. This book, as well as his *The Essential Tension*, quickly made Kuhn well known among Chinese scholars, resulting in keen discussions about the concept of scientific revolutions. Underlying this phenomenon lay the desire to achieve modernization through the development of science and technology, and the possible opportunity for a new scientific revolution. In 1998, CAS encouraged historians of science to start the study of science policy and strategy from historical perspectives. Some historians of science have also become interested in scientific culture or the relationship between science and humanities since the early twenty-first century.

Since the end of the 1990s, Chinese historians of science have been thinking about and testing how to break the research model of "achievement-identifying and -describing" and how to reconstruct the history of science in context in order to avoid destroying the original structure of pre-modern scientific knowledge, and to cast off the Eurocentrist framework (Zhang 2007). They place great importance on such questions as: How was scientific knowledge created and transmitted in the Chinese cultural context? How did Chinese knowledge interact with the scientific knowledge transmitted into China from other cultural traditions, such as from Europe? Chinese historians are also devising new questions about modern science. For example, some of them are making a study of the relationship between scientific revolutions, industrial revolutions and the modernization of nations.

Conclusions

In the early twentieth century, the modern era of the history of science in China began, and Chinese scholars started to study the history of science under the influence of Western missionaries, Sinologists and pioneering historians of science, such as George Sarton. In the 1950s, promoted by Co-ching Chu and his allies, such national scientific institutions as the Chinese Academy of Sciences carried out the professionalization and institutionalization of research on the history of science. Influenced by government ideology, especially patriotism, Chinese historians followed the classifying framework of Eurocentric modern science to sift through and study China's scientific heritage. They emphasized its pre-modern, especially ancient, achievements to disprove Eurocentrism, so that a historiographical model of "achievement-describing" came into being. Since the 1980s, Chinese historians and the public became more and more interested in so-called "Scientific Revolutions" and their impact on modernization.

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Chapter 25
On Kuhnian and Hacking-Type Revolutions

Silvan S. Schweber

Since the mid-1970s we have been witnessing a deep structural change in the practice of the sciences, in the institutions that produce new scientific knowledge and new practitioners, and in the nature of that knowledge. And all these are at odds with the assumptions that underlay Kuhn's thesis in *Structure*.[1]

What happened in physics in the late 1970s was the culmination of the synthesis of quantum mechanics and the special theory of relativity into the quantum theory of fields. It resulted in the formulation of the "standard model of particle interactions" as the lowest level, context-free, description of the dynamics of the entities out of which the presently observable physical world is believed to be composed.[2] Furthermore, a justification was given for the representation of physical phenomena in quasi-independent atomic, nuclear and sub-nuclear levels. This hierarchical ordering goes far beyond the notion of the quantum ladder that Weisskopf had advanced in the early 1960s wherein each rung of the ladder is distinguished from its neighbors by the dramatic difference in the order of magnitude of the dimensions of the motions involved, and hence of the energy transfers involved in each of these levels (Weisskopf 1962).

Each of the present levels has been given a foundational theory—foundational, not fundamental—called an "effective field theory," the representation of the dynamics of the elementary entities out of which the more complex structures that populate the domain are composed.[3] Moreover, the relations between the effective field theories governing adjacent sublevels are calculable (Cao and Schweber 1993).

Each of the atomic, nuclear and sub-nuclear domains has been further subdivided by the amazing instrumental and theoretical advances of the past 50 years. These hierarchies are not considered independent, nor are they disconnected. There are highly accurate measurements of atomic energy levels that reveal nu-

[1] The present paper is based on joint work with Roly Belfer, "Hacking Scientific Revolutions," to be published.

[2] For a popular account see Weinberg (2013).

[3] Steven Weinberg is responsible for the extensive present day use of effective field theories. See Weinberg (1979, 1991, 1995–2000).

clear and sub-nuclear features. Similarly, the recent startling discovery of the necessity to assume the presence of cold dark matter—consisting of as yet undiscovered sub-nuclear entities—in order to make sense of new cosmological observational data is indicative of the linkage between the various levels. But these new observations have not destabilized the current amazingly accurate representations of the atomic world. And, needless to say, the linkage of these levels becomes explicit as soon as one tries to answer evolutionary questions.

Most importantly, to a *very high degree of precision*, advances in lower levels do not destabilize the effective field theory in any given level. Consequently, a degree of finalization has been achieved which implies that the aims of research in the physical sciences at the atomic, molecular, nano, meso and macro level are no longer the determinants of a fundamental theory, as was the previous aim of the sub-disciplines concerned with these realms. Instead it is the creation of novelty, the unraveling and conceptualization of the possible new structures that can emerge by composition or by the attainment of previously unreachable low temperatures and the representation of the dynamics, which are to describe the macro-systems and their relationship to lower level foundational theory.[4]

Furthermore, advances in computer hardware, software and memory devices have dramatically altered both experimental and theoretical physics. Kuhn was faulted for his emphasis on theory. One should now not only talk of experimental and theoretical physics, but in addition of computational physics. Computational complexity theory studies the intrinsic difficulty of problem-solving, that is, classifying which problems can be solved efficiently by computer and which cannot. Should there be a proof that claims problems exist that are significantly more difficult to solve than to verify a claimed solution, (i.e., the resolution of the P versus NP question, as claimed by the mathematician Avi Widgerson)[5] this should be considered a *law of nature*. This indicates the limits by virtue of the computational complexity of being able to compute the properties of the stable entities that populate a given level given the effective field theory of a more foundational one, and thus indicate limits of reconstructing the world from a foundational effective field theory without putting in additional empirical data.

[4] There is little question that a deep structural change has occurred. The explanation for the change has for the most part been concerned with political, economic and cultural factors, with less attention paid to cognitive factors internal to the various scientific and engineering disciples. The above suggests that cognitive factors are surely one of the reasons that the Bayh-Dole legislation has had such consequential impact on the restructuring of universities.
The Patent and Trademark Law Amendments Act (now known as the Bayh-Dole Act) was enacted by the US Congress in December 1980. The legislation gave American universities, small businesses and non-profit organizations exclusive patenting rights of inventions and control and property rights over intellectual materials that resulted from governmental funding. The legislation had been sponsored by senators Birch Bayh of Indiana and Bob Dole of Kansas.

[5] See, for example, Deutsch (2011).

Just as physics has been transformed, so has chemistry. Undoubtedly it is the biological and medical sciences that have been most deeply affected by internal developments: Crick and Watson, genetic codes, recombinant technologies, DNA-sequencing, genome projects, bioinformatics, CRISPR. It is in the biological and medical sciences that the entrepreneurial aspects of the university are most visible. My task as a historian of modern physics is to try to give an account of how the above outlined conceptualization of physics and the changes in its practices came about. The physics community would probably be satisfied with a *longue durée* narrative of the quantum "revolution," in which the notion of "revolution" and the contributions of the individuals believed to have been responsible for seminal, important advances are emphasized. When applied to physics, and more generally to science, "revolution" is a metaphor. Its political meaning implies the forceful, and at times sudden and / or unexpected, removal of a pervasive and dominating power, this in the name of an alternative, generalized view and ultimately offering a differing ordering of things.[6] As a metaphor in the history of science, "revolution" has been applied to describe the overthrow of a dominating tradition, as in the case of the "Einsteinian" general relativistic revolution, which overthrew the traditional "Newtonian" view of regarding space-time as a stage unaffected by the events occurring in it. Whether used metaphorically or otherwise, "revolutionary" developments in the sciences cannot be wrenched out of the contexts in which they take place and must be connected with the economy, culture, politics, institutions and so forth, of the societies in which they occur. This has been done for the "Scientific Revolution" by Schaffer, Shapin, Floris Cohen, Heilbron and others[7], by Ian Hacking[8] and others for the probabilistic "revolution," and is also being done for the quantum revolution.[9] My description of the quantum revolution emulates what Hacking did for the probabilistic revolution. Here I will only consider the "epistemological rupture" (Foucault 1976) aspects of the quantum revolution within the time frame 1900–1980.

The first point I wish to make is that the theoretical concerns and advances that culminated in the formulation of non-relativistic quantum mechanics by Heisenberg, Born, Jordan, Schrödinger, Dirac and Pauli in 1925–26 cannot be disconnected from matters of mathematics, chemistry, applied science, engineering and computing. Mathematics is a special language that makes objectivity

[6]This to differentiate a revolution from a *coup d'état*. It should be noted that even when a revolution fails it launches a long-term process of changes at every level.

[7]Cohen (2010); Heilbron (2013); Shapin (1996); Shapin and Schaffer (1985).

[8]Hacking (1987). See in that same volume the two other introductory essays: Cohen (1987) and Kuhn (1987).

[9]See for example Kragh (1999), and more recently Staley (2013).

and the unambiguous exchange of information. Furthermore, mathematics and physics have always been "co-constructed".[10]

A second point to be emphasized is that what was fundamentally new in Schrödinger's wave mechanics—in contrast to classical physics—is that the interacting entities participate as *objects*, whose structure, couplings and other attributes can change as a result of the interactions, and more particularly, that new *objects* can be formed. As important as were the calculations of Pauli and Schrödinger were in obtaining the level structure of the hydrogen atom—a new object formed by the interaction of an electron with a proton, or Heisenberg's explanation in explaining the level structure of the (two electron) helium atom, and the subsequent calculations to explain the periodic table, a further *crucial* calculation[11] was that of Heitler and London, which explained the formation of the hydrogen molecule.[12] By indicating how the charge density of the two electrons when in a singlet spin state lowered the energy by being locatable between the two protons, thus increasing the attractive forces between electrons and protons and shielding the repulsive force between the two protons, Heitler and London formulated the quantum mechanical basis for the covalent bond. The calculation gave a new quantitative perspective on bonding and saturation. In addition, the directional characteristics of orbitals when electrons were not in s-states were used to indicate how quantum mechanics could explain the bonding properties of the carbon atom, which was to understand the structure of organic compounds. A morphic element was thus introduced into quantum mechanical explanations (Gottfried and Weisskopf 1984–1986).

The quantum mechanical modeling of the atomic and nuclear world had two further attributes that were recognized early and shaped the approach to understanding phenomena at both the micro and macro levels:

1. A quantum description gives a measure of certainty to our knowledge of the world: it asserts that all hydrogen atoms in their ground state when isolated are identical; the same ist true for ^{23}Na atoms in their ground state. Similarly, that all lead ^{206}Pb$_{82}$ nuclei in their ground state are identical[13]

[10] This claim is expounded in the paper on which the present one is based. See Dear (1995); Gillies (1992) and therein Mehrtens (1992a and 1992b). For essentially a validation of the statement, see Krieger (2003), a remarkable, deeply insightful, historically sensitive study of mathematics and its relations to physics.

[11] I owe the notion of a crucial calculation to my colleague Howard Schnitzer at Brandeis.

[12] Heitler (1927). See Gavroglu and Simões (2012) for the details and subsequent developments.

[13] Upon receiving the 1993 Orsted medal for his contributions to the teaching of physics, Bethe in his acceptance speech stated "that there is a certainty principle in quantum theory and that the certainty principle is far more important for the world and us than the uncertainty principle. That doesn't say that the uncertainty principle is wrong. It says that the uncertainty principle just tells you that the

2. When computing the properties of atoms, molecules and solids, the value of the parameters that enter into the Schrödinger equation describing the dynamics of the system characterizing the electron and the nuclei—such as their mass, spin, magnetic moment, electric quadrupole moments—the values of these parameters are empirically determined. After the discovery of the neutron in 1932, and after models of nuclear structures had been advanced, these nuclear parameters were to be explained and their value quantitatively determined by the "lower level" theory that was to account for the structure and stability of nuclei (i.e by a description of nuclear dynamics in terms of neutrons and protons and the nuclear forces by which they interact).[14]

During the 1930s, many instances occurred that led to a novel conceptualization of physics began assuming an ever-greater importance. The quantum field theoretical demonstration that the electromagnetic interactions between charged particles could be explained as due to photon exchanges,[15] Fermi's formulation of a field theory of β-decay and Yukawa's suggestion that in analogy to electromagnetic forces the short range nuclear forces between nucleons could be generated by the exchanges between them of a hitherto unobserved massive particle were all examples of this. This novel conceptualization involved recognizing that, at the level of accuracy of possible physical measurements and the corresponding theoretical representations, the physical world could be considered *hierarchically ordered* into fairly well delineated realms and concerns: the macroscopic (consisting of solids, liquids, gases, their structure and their properties); the molecular and atomic realm; the nuclear; and the sub-nuclear ones and that the physical processes by which their connection is implemented could be given.

The atomic, nuclear and subnuclear realms became describable by separate (foundational) ontologies and corresponding quantum dynamics. The ontologies are connected to a given level—electrons and nuclei for the atomic and molecular realm and neutrons and protons for the nuclear level, with the latter's interactions at first described phenomenologically by nuclear potentials, and later assumed to be derivable from a quantum field theory of nucleons and mesons, once mesons were included in the basic ontology. The entities that comprised the foundational

concepts of classical physics, position, and velocity, are not applicable to atomic structure" Bethe (1993).

[14] It did so first in term of phenomenological internucleonic potentials. See Bethe (1937); Bethe and Bacher (1936); Livingston and Bethe (1937). [The above three lengthy articles comprise the "Bethe's Bible." They were republished as Bethe, Bacher and Livingston (1986).] Thereafter in attempts to determine these potentials on the basis of meson theories, and more recently in terms of the standard model. See Brown and Rechenberg (1996).

[15] See, for example, Fermi (1932).

ontology were considered the building blocks of the composite objects that populated that level.

The synthesis of quantum mechanics and special relativity resulted in the formulation of the quantum theory of fields. In the early 1930s, it predicted "antimatter." After the formulation of renormalization theory in the late 1940s, quantum electrodynamics gave a much more precise description of atoms. The formulation of non-Abelian gauge theories in the late 1960s to describe the weak interactions resulted in the unification of electromagnetism and the weak interactions, and provided the electroweak part of the standard model. Finally, the discovery of asymptotic freedom of non-Abelian gauge theories in the early 1970s completed the construction of the standard model, which encompasses most of the laws of physics known today.[16] But the inability to incorporate gravity into the standard model seems to indicate the limit of a quantum field theoretical description.[17]

What I have outlined is the thesis that the quantum "revolution" constitutes a Hacking-type (HT) scientific revolution[18], named after Ian Hacking who characterized the probabilistic revolution of the nineteenth century[19] in terms of the crucial novel feature of a scientific revolution: its *style of reasoning*.

Styles of reasoning are the constructs that specify what counts as scientific knowledge and constitute the cognitive conditions of the possibilities of science. They are made concrete through the specification of theories, their ontological assumptions and their explanatory models. A style of reasoning introduces new types of objects, evidence, sentences, (new ways of qualifying truth or falsehood), laws, modalities and most importantly, new possibilities.[20] Different styles of reasoning can coexist. Styles of reasoning are bound in scope with definite limits of applicability. But they are "big": they must encompass several scientific disciplines.

HT revolutions are considered emplacement revolutions, rather than replacement-revolutions. They change the way science is practiced without necessarily abandoning all the previous concepts by transforming it from within, through shifting the questions being asked and the criteria for acceptable answers, (these being a characteristic of an "emplacement revolution") (Humphreys 2011, 132).

[16]Wilczek (1991), for a concise and authoritative overview of quantum field theory.

[17]Some physicists, e.g. Leonard Susskind, have suggested that the failure to synthesize quantum mechanics and *general* relativity has indicated the limits of the quantum mechanical description of physical nature. See Susskind and Lindesay (2005).

[18]Schweber and Waechters (2000); R. Belfer and S. S. Schweber, "Hacking Scientific Revolutions", to be published.

[19]Hacking (1987). See in that same volume the two other introductory essays: Cohen (1987) and Kuhn (1987).

[20]Hacking (1981a, 1981b, 1982, 1983 [reprinted 2002b], 1985, 1992a [reprinted 2002c], 1992b, 1996, 1994, 2002a, 2009, 2010 [reprinted 2011b]), *and especially* (2011a). See also Kusch (2010).

HT revolutions amalgamate pure and applied concerns. They transform a wide range of scientific practices and are multidisciplinary, with new institutions being formed that epitomize the new directions. These "new" institutions can however be "old" ones that have been restructured. The time scale of HT revolutions is the *longue durée*, but the *durées* have become shorter as the scientific community has increased. HT revolutions are linked with substantial social change, and after an HT revolution, there is a different feel to the world.

An HT revolution is characterized by a new style of scientific reasoning and conversely, the genesis of a new style of reasoning is indicative that an HT revolution is in the process of evolving, with self-authentication and self-stabilization that are characteristic features of the evolutionary process. Following Crombie (1994) Hacking gave the following examples of styles of reasoning: postulation in the mathematical sciences, ordering by comparison of variety and taxonomy, experimental exploration and measurement, the statistical analysis of populations and finally the derivation of genetic development.

HT scientific revolutions that are of particular interest have an additional feature: they make use of a characteristic *language* to formulate, corroborate, self-authenticate and self-stabilize the style of reasoning it introduced.[21] For the probabilistic revolution the statistical analysis of population regularities was its style of reasoning and the calculus of probabilities its language.

The style of scientific reasoning I associate with the HT quantum revolution is characterized by the hierarchization of the microscopic physical world and quantum field theory is its language.[22]

Considering a "big" scientific revolution such as the quantum revolution as a Hacking-type revolution allows for greater continuity with previous knowledge; it emphasizes the interdisciplinary aspect of the growth of knowledge and makes the social, sociological, cultural and the epistemological an integral part in the historical inquiry. It also considers the limits of the new knowledge and what it entails, which demarcates the revolution. Such a view challenges us to be better historians, yet recognizes the special character of being a historian of science.

[21] I do not wish to stretch the notion of language and associate with each Hacking revolution a language. But when relevant, I do place great emphasis on this notion of language.
[22] S. S. Schweber, "Hacking the Quantum Revolution," to be published.

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Chapter 26
Goethe Was Right: 'The History of Science Is Science Itself'
M. Norton Wise

In preparing for a recent conference reflecting on the significance of Tom Kuhn's *Structure* I was struck by how forthrightly the organizers stated that "little current work in the history or philosophy of science engages with Kuhn directly. Why and how did his program unravel?" I tend to agree with their assessment and want to engage with their question here. But the MPIWG conference that gave rise to the present volume displayed something that seems contradictory. Several of our most prominent representatives of social studies of science remarked on how deeply *Structure* inspired their own *sociological* work. They included such notable figures as David Bloor, Harry Collins and Martin Rudwick. Now this is strange, as there is no sociology in *Structure* and Tom never wrote what could be called a social history. Harry Collins dismissed this observation with the remark that people never know what's in their own books. I suggested that he and others saw the *need* for sociology in such statements as that paradigms were what a group shares so that they went about supplying it. This did not satisfy David Bloor. Nevertheless, I want to propose that the most basic reason so little current history of science engages with *Structure* is that social history, especially the social history of *practice*, plays such a fundamental role in current work, while it played little role in *Structure*, despite repeated references to practice, especially as exemplary problem solutions in the postscript. Tom, in fact, was rather hostile to the priority of practice in social studies of science and remarked more than once in conversation that he just could not get practice in their sense. Why is that? A first answer is that Tom understood history of science as history of ideas, in the strong sense that ideas were the active agents in history. But that answer has a more general context, which is the theory-dominated character of both science and the history and philosophy of science at the time he was writing (and he had of course been trained as a theoretical physicist himself). By theory-dominated I am referring in the first instance to the theory-ladenness of observation and experiment but also to the priorities of reduction and deduction, if only in a loose sense. In that world, the priority of practice was not quite comprehensible. But the world has changed, both in terms of the sciences and of the history of science.

And it is ultimately that change that I want to get at, with emphasis on narrative explanation.

Work

I begin with a brief characterization of some aspects of this change in historiography and then give an example from my own engagement with Tom and his work. For historians writing today the sociality and historicity of everyday life in the sciences, that is scientific practice, has become an unquestioned assumption. With that we take for granted not only the multiplicity and diversity of the sciences—on which Tom himself insisted—but also their embeddedness in economic, political and cultural contexts—which he severely circumscribed. So when we want to understand a development in one of the sciences we try to give a richly embedded historical development of its practices and representations. The science is presented within what I think of as its "field of interactions." Tom liked to say that he wanted to "get *inside* people's heads," to understand how they were thinking. Today we want to get *outside* people's heads, to understand the tools that make thinking and acting possible. "Distributed cognition" may be ultimately what we want to understand, in the sense that cognition is distributed over our tools and social relations, rather than only taking place in our individual brains. But more immediately, we aim at a narrative of how the field of interaction develops, and that developmental narrative constitutes our explanation of what happens. There are alternative narratives and there are better and worse narratives, depending on how well they incorporate the full range of evidence available. One could say that there remains something deeply Kuhnian in this, namely the essential historicity of science. And it is all too easy to forget how radical that notion was at the time of *Structure*. But the historicity now has a different character.

Let me illustrate this shift with respect to one of Tom's well-known papers. When he wrote "Energy Conservation as an Example of Simultaneous Discovery" (1959), he drew heavily on "the engineering concept of work" as one of three crucial intellectual constellations that fed into the early expressions of what would be identified as energy and its conservation. This paper remains a classic in the history of science, admirable for the clarity with which it brought into view issues that had remained largely outside history of physics. And it is characteristic of the best intellectual histories that Tom wrote. But it would not do today, nor even in the 1970s, simply because "work" remained largely a disembodied idea, an idea extracted from concerns with engines and "applied in deriving the abstract scientific conservation law" (Kuhn 1977, 92). The engineers and their interests thus disappear in the "abstract scientific" object of the analysis. When I began working with Tom in 1971, this kind of history had already begun to call out for something more, just as his discussion of paradigms did in *Structure* as read by sociologists of knowledge. Reference to the energy paper in *Structure* occurs in the Preface in a footnote attached to his acknowledgment that he had said nothing about "the role of technological advance or of external social, economic and intellectual conditions in the development of the sciences" but contending that, although they might affect the timing of a crisis or the range of revolutionary reforms available, their explicit consideration "would not, I think, modify the main theses developed in this essay" (Kuhn 2012, xliv). The footnote seems to back off a bit from this position with respect to energy conservation, but not with any specificity or consequence.

In fact a rather strict formulation of the internal-external distinction is essential for the "esoteric" and "professional" character of a mature science governed by the paradigms presented in *Structure*. Their "very special efficiency" depends on "the unparalleled insulation of mature scientific communities from the demands of the laity and of everyday life." Even in conditions of crisis for a paradigm, "technical breakdown would still remain the core of the crisis" so that "external factors" were secondary (Kuhn 2012, 164, 69). It is just this insulation that had come into question from many directions in the 1970s.

In reworking the story of "work," here with reference to both William Thomson in Britain and Hermann von Helmholtz in Germany, it seemed necessary to put the actual engines producing work at the center of attention, as active agents, especially in their role within the factories of industrializing economies (Wise 1988; Brain and Wise 1994). Where machines replaced humans, "work" replaced labor value as the source of the value of commodities and of the wealth of the industrial nation. This process of revaluation was intimately bound up with the emergence of energy conservation. But even writing at that level was too general. To understand "work" required figuring out what it meant to particular people in particular places and how it was valued and measured, not simply as force times distance but as embodied in engineering practice, for example as registered visually using indicator diagrams. In this way the study of "work" becomes an intensely local enterprise. The explanation of how energy, measured as work, became the most fundamental concept of physics in the nineteenth century, then becomes a story of detailed local histories and their interrelation. The concept of "work" does not generate these histories; *they* generate *it*. As such, they *explain* it, or so I propose to say.

I am sensitive to the fact that some people, especially scientists, may still see Tom's history of ideas as more satisfying as an explanation because of its simplicity and conceptual clarity. It also conforms more nearly to the way in which physicists at the time he was writing preferred to explain things within a theory-driven enterprise. But that situation has changed rather dramatically for many scientists, particularly those dealing with complex systems, where it is common to use simulations to gain understanding, indeed to provide “explanations.”

Snowflakes

The designation “complex” in this usage implies that the organization of the system is not subject to either reduction to a lower level of constitutive elements or to deduction from general laws. In this situation investigators typically explore the developmental dynamics of the system either experimentally or by using simulations as an alternative. I have been particularly interested in the way in which the simulations take on a historical character. To make clear what I mean, I will borrow my favorite example from a previous discussion (Wise 2011). It concerns snowflakes. Perhaps most of us will think of the typical snowflake as exhibiting an intricate geometrical pattern, highly symmetric, with six identical arms. This is the idealist image that Kepler, Descartes and Hook all presented in the seventeenth century. Their familiar assumption of mathematical regularity as the foundation for order and beauty in nature continued to govern studies of snowflakes through much of the twentieth century. Despite the fact that full hexagonal symmetry was very rarely observed, the asymmetry was ascribed to accidental disturbances of various kinds.

One major exception to this rule appeared in work of Ukichiro Nakaya, first published in English in 1954. Trained as a nuclear physicist, but lacking nuclear facilities at Hokkaido University in the north of Japan, Nakaya turned to taking photomicrographs of both natural and artificial snow crystals, which he grew in a cold chamber. Figure 1 shows his first artificial flake. Finding that “a perfectly symmetric snowflake is very rarely observed,” Nakaya studied asymmetries and irregularities of many kinds, which focused attention on the normal processes of growth rather than on supposed states of perfection (Nakaya 1954). The result was effectively a natural history of snowflakes, published as a “museum” of micrographs, including both stages of growth and diversity of form. Such a museum was just not the sort of thing that interested most physicists in the 1950s, particularly not those who sought their explanations in elegant mathematical models. Nakaya’s snowflakes gained little recognition.

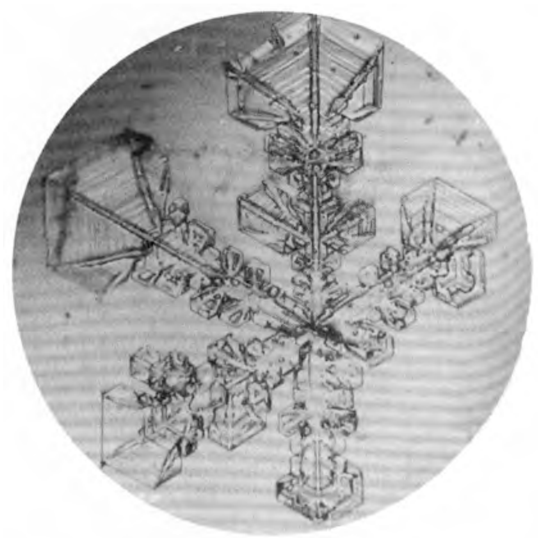

Figure 26.1: The first artificial snowflake (Nakaya 1954, 152).

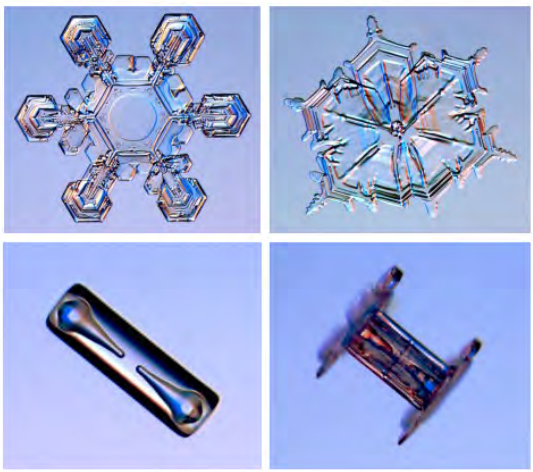

Figure 26.2: Photomicrographs of snowflakes (Libbrecht 2011).

The climate for work like his changed dramatically during the 1970s and 1980s as problems of complexity became ever more important in mainstream physics research. Only very recently, however, has a physicist at California Institute of Technology taken up snowflakes as part of his work on pattern formation in nonlinear, nonequilibrium systems. Kenneth Libbrecht has extended Nakaya's natural and artificial crystals with much higher resolution equipment (figure 26.2), yielding in 2006 what he called a *Field Guide to Snowflakes* (Libbrecht 2006). I take the term "field guide" to be explicit recognition that natural history and the study of nonlinear dynamical systems have much in common. Indeed, Libbrecht writes about snowflakes in terms of their "life history." The life history yields a lesson: "Complex history [produces] complex crystal shape" (though, ironically, even he harbors the idealist aesthetic preference: "I always select their most symmetrical crystals to display") (Libbrecht 2013).

The lesson of complex history is apparent also in the work of two mathematicians who do simulations. Even a decade ago it was not practicable to simulate the evolution of a snowflake at high resolution. But Janko Gravner and David Griffeath, have produced a three-dimensional, mesoscopic, computational model that replicates many of the basic forms or "habits" of snowflakes—dendrites, needles, prisms—along with their more intricate "traits"—sidebranches, sandwich plates, hollow columns (figure 26.3) (Gravner and Griffeath 2009, 1, 17).

Figure 26.3: Simulated snowflakes (Gravner and Griffeath 2009, 13).

Gravner and Griffeath employ a conceptually simple computational model, which grows a virtual snowflake from a small seed of ice surrounded by water vapor and governed by only three mechanisms: diffusion of water vapor from the crystal; freezing and melting in a narrow boundary layer; and attachment rates at the boundary that favor concavities. Despite this conceptual simplicity, however, implementation of the model in a continually updating cellular automaton requires many parameters and large amounts of computing time, even for a fully symmetric snowflake (about 24 hours on desktop computer). Gravner and Griffeath forthrightly acknowledge that it is not very clear just how their intuitively plausible parameters correlate with physical processes and that their simulations do not treat important issues of non-symmetry, randomness, singularities and instabilities. They nevertheless believe that the evolutionary simulations provide "explanations" of many of the characteristics of natural snowflakes, both in general morphology and in the details of their traits. Run many times over, with varying parameters, the simulations explore the space of possible snowflakes. These explorations discover previously unknown properties in natural snowflakes and suggest new kinds of observations.

The explanations and discoveries obtained in this work are natural historical in kind. Key terms are *trait*, *habit*, *morphology*, *seed*, *evolution*, *field guide*. The simulations not only generate a museum of snowflakes, but explain their characteristics by the conditions of their development, read as evolution. The algorithms governing the evolution may not be the Darwinian principles of variation and selection but they are nevertheless generative algorithms capable of explaining how the entire phylogeny derives from something like a common ancestor developing under varying environmental conditions. *That is, the simulations generate an evolutionary narrative which explains the natural order of snowflakes as an essentially historical order*. Every individual snowflake is a unique product of its history, full of contingencies and accidents. The virtual history of a snowflake, then, is its explanation. This is a long way from the traditional reductive and deductive explanations in physics, or indeed from anything Tom contemplated.

Finally, the role of visualization requires comment. Visual images have always been crucial in physics to guide intuition and to illustrate solutions. But the role of visualization in many simulations is qualitatively different, for it typically serves as the only effective means for understanding the growth process and its intricate results. The snowflake simulation, for example, must incorporate a technology for converting its calculations into visually legible images comparable with photomicrographs. Slide shows and movies naturally result. Inevitably these visual media enhance the sense that the simulation is productive of a narrative, a narrative that describes how a complex history generates a complex system.

Museums and History

The old question arises of whether historical narratives really explain. I am struck by the degree to which the literature on this question has been shaped by Carl Hempel's articles of 1942 and 1963 on explanation (Hempel 1965, 2001). His view that explanation requires subsumption under general laws reflected the assumption that theoretical physics supplies the model for all natural science. Narratives as such do not explain. The social sciences explain to the degree that they find principles of rational action for typical situations. Noretta Koertge, drawing on Popper and Hempel, gave a succinct formalization of this kind of explanation (Koertge 1975). The only point I would like to make is that such explanations seek to find something in the social sciences that would be analogous to deductions from general laws. The same remark applies to Arthur Danto's effort to defend narrative explanation, arguing that one could inscribe tiny historical micro-changes into the Hempelian mold, while nevertheless insisting that for macro-narratives describing long-term developments "no general law need be found to cover the *entire change*" (Danto 1985, 255). The problem of course is that all of this loses its point if, as in the snowflake example, deduction from general laws is not at issue.

The major alternative accounts of narrative, from more literary figures like Hayden White and Paul Ricœur, focus on its fictional character and largely dismiss the relation to natural science. This seems equally unhelpful. So I propose to throw out both traditions and to start over by returning to the early nineteenth century when history was gaining newfound prestige as a form of knowledge, especially in Germany, and when Goethe published his *Zur Farbenlehre* (*On the Theory of Colors*) in 1810.

Suppose then that there is good reason to think that in many areas of natural science explanations at one level of phenomena, say snowflakes, cannot be reduced to a lower level under general laws, say the dynamics of the water molecules that make up the snowflakes. What options are available if explanation has to rest on things and their relations all at one level? Basically, I think, we are left with two avenues: museums and histories. By "museum" I mean a collection that displays the diversity of generically similar things, ordered in an illuminating manner. That is one part of what the new snowflake people give us, whether as the natural snowflakes in Libbrecht's *Field Guide* or the simulated ones of Gravner and Griffeath. Under "history" I include two aspects: context and development in time. This second part is what the simulations of snowflakes provide: a context for water vapor under particular conditions of pressure, temperature, density and other parameters, and a developmental history of how a seed of ice grows in time as it falls for an hour or so through this continually changing environment, or context. The two parts, museum and history, are presented together as a natural history museum.

This combination is what Goethe prescribed in his *Farbenlehre* when he famously remarked: "The history of science is science itself." He meant this in two senses, the first concerning light itself, "we attempt in vain to express the inner nature of a thing. We experience only effects, and a full history of these effects comprises the essence of the thing." Here a "full history" is effectively a natural history museum, and he devoted Part I to comprehensively collecting and to showing how to produce such a history of the various effects of light as color, the "acts of light" as he put it (von Goethe 1890, ix). On this reading, the snowflake museums collect the diverse *acts* of freezing water vapor. The emphasis on the need simply to find out what kinds of things are in the world and how they come to be there is quite common among complexity people. In their manifesto for the twenty-first century, the condensed matter physicists Robert Laughlin (Nobel laureate) and David Pines said in 2000 that "The central task of theoretical physics in our time is no longer to write down the ultimate equations but rather to catalogue and understand [i.e., collect and organize] emergent behavior in its many guises, including life itself" (Laughlin and Pines 2000, 30).[1] Here the natural history imperative turns into an attack on pretentious theory, namely on quantum mechanics as a grand unifying theory, a theory of everything.

This attack mode captures the second sense of Goethe's dictum, the more infamous one. Just as *things* should be understood in terms of their histories, he insisted, so also with the *sciences* of things. The science of light should be understood in terms of its history of development. But this history had been marred over the last 100 years by a seriously distorting accident, namely the prominence of Newton's theory of light and colors with its mathematical reduction to rays of various refrangibility. So Goethe felt the need to rid the history of arrogant Newtonian reduction in his Part II, the polemical part, before going on to recovering the positive history in Part III.

Goethe's sense of contingency in history here, of wrong paths and alternative paths, is quite radical. I am reminded of the chauvinistic Berkeley professor who responds to the arrogance of his Harvard colleague with the observation: "If the Puritans had landed in San Francisco, Boston would never have been discovered." Since about 1970 there has been plenty of this kind of polemic coming from condensed matter physicists contesting the reductive and deductive pretensions of elementary particle people. Laughlin and Pines, for example, in their own appeal to the need to understand physics in terms of its history, say: "Indeed, one could ask whether the laws of quantum mechanics would ever have

[1] See also Laughlin, Pines, et al. (2000), where they invoke evolution, growth, aging and adaptation to capture the analogy of physical to biological processes.

been discovered if there had been no hydrogen atom" (Laughlin and Pines 2000, 30). My snowflake people are not given to polemics, but if they were they would attack the 300 year prejudice for mathematical idealism that insisted on reducing the rich and complex history of snowflakes to simple, perhaps simple-minded, hexagonal symmetry.

Narrative—Suggestive Directions

Leaving Goethe and polemics aside, I want to make the wholly unoriginal point that the great explanatory power of museums and histories lies in analogy. And it is the power of analogy that is exploited in a number of related methods of investigation that employ non-reductive methods of understanding. Here I will just mention a few that figure in a volume on *Science without Laws* that developed from a two-year workshop at Princeton University (Creager, Lunbeck, and Wise 2007). They are model systems, cases and exemplary narratives. Simulations also appear but I will not say more about them.

Model Systems are surely one of the most powerful tools of twentieth-century biology. While eschewing any reduction of phenotype to genotype they offer strong heuristics for relating such things as tumors in nematode worms to human cancers, or just jet lag in humans to that in rats, based on conservation of evolutionary genetic acquisitions. Even physicists have been learning how to use the analogies of model systems in understanding complex phenomena. And historians of science have taken them up in a big way both as subjects of investigation and as tools for investigation. Angela Creager is one of them with her important book on tobacco mosaic virus (Creager 2001; also Kohler 1994). Model systems attain their great strength precisely because they are used in the first instance effectively to generate museums of the diverse "acts" of the system and secondly because of the developmental histories that they produce. Indeed this strength depends on a whole community of workers who develop the natural (or unnatural) history of the model system: the mice people, worm people and fly people. So following the model system provides a means of unpacking an interconnected network of materials, instruments, institutions and people.

In some ways similar to model systems are cases. The study of cases, and of case histories as narratives, has of course long been a standard means of using analogy in medicine, law and the social sciences. Again, the museums of cases and their histories are crucial. Excellent examples for the history of science appear in the work of Mary Morgan, both in her contribution to the *Laws* volume and in a new book on models in economics, *The World in the Model* (Morgan 2012). She looks in detail at a series of cases of economists employing models from Ricardo to the present. The result is a history of how economists have come to use and think about models over two hundred years. She analyzes also the function that a wide variety of specific narratives play in allowing economists to attach very simple models, like the 2x2 matrix of the prisoner's dilemma, to diverse situations in the world. This appeal to narratives operates quite widely in other analogical methods, as I have indicated for simulations. The important feature of Morgan's use of narratives is that she studies them as a tool for exploring the functionality of the models.

Exemplary narratives offer yet a third means of pursuing museological understanding. One of the best discussions I know is by Carlo Ginzburg in a paper in *Science without Laws*. He narrates the history of a particular individual acting in a richly described eighteenth-century context in order to explore the dynamics of the period in which the individual lives (Ginzburg 2007). The particular narrative is exemplary not in the sense of its being typical but in that it is representative for the situation. Thus Ginzburg sharply differentiates his generic approach from idealized models like Weberian ideal types. Here again, we can learn a great deal from Ginzburg's historiography about the function of narrative as an investigative tool.

To model systems, cases and exemplary narratives I would add one further area of contemporary history of science that belongs to my story of natural histories and narratives. I claimed for the snowflake simulations that "they generate an evolutionary narrative which explains the natural order of snowflakes as an essentially historical order." This generative aspect has a clear analogue in Hans-Jörg Rheinberger's analysis of "experimental systems" as "generators of the future." They are the laboratory systems that give life to what he calls "epistemic things," those not yet understood objects of investigation that the experimental system may or may not convert into an object of knowledge. The system will support a variety of narratives about what is going on and the outcome cannot be predicted. But it is generated by a historical process that can be explored and understood in retrospect. This is the task of history, to explain how the object comes to be known through as full an account as possible of the dynamical operation of the experimental system. Explorations of such systems provide the empirical base for what Rheinberger calls "historicizing epistemology" (Rheinberger 1997, 2010).

To conclude, I would return to the original question of the fate of Thomas Kuhn's *Structure*. Clearly it was a great inspiration for many people, including me, to pursue what we thought the book implied needed to be pursued: sociology, practice, materiality, political economy, culture. But none of those things were actually engaged in *Structure*. I have argued that they could not have been, not only because of who Tom was but because the world was rapidly moving out from under both science and history of science as he knew it. But these developments only enhance what was very much in *Structure*, namely the historicity of science. In the end, that has been both its most radical and its most lasting import.

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Chapter 27
History of Science and Technology in Portugal: Networking in the European Periphery

Ana Simões

Introduction

Perhaps one of the most striking changes that took place in the discipline of history of science and technology in the last few decades has to do with the parallel processes of professionalization and internationalization of communities located outside of well-established communities such as those in the UK, France, Germany, the Low Countries or the United States of America. This process has encompassed communities in many countries of the European periphery, as well as countries in Central and South America, Asia, Oceania and to a lesser extent, Africa. The study of how the discipline has been developing in these various places will enable us to put into perspective developments that are associated with the so-called "European Center"[1] and the US, which are often taken as exemplary models. I have in mind questions such as: the background and training of future historians of science and historians of technology, the location of these communities within local university systems, their positioning vis-à-vis scientists and social scientists, their struggle for financial support and their relation to other co-related communities, such as those of historians of technology and historians of science, philosophers of science, science and technology studies scholars and so on. Furthermore, it will enable the enrichment of scholarly debates with various case studies stemming from other locations, using different methodological apparatuses and often contributing to the revision of former historiographical perspectives and the introduction of new ones, creating a global history of science and technology.

Notwithstanding contributions to the history of the nautical sciences and cartography associated with the Age of Discovery, as well as episodic and scat-

[1] Since the sixteenth and seventeenth centuries, the reference to European science has usually encompassed the space enclosed by a "polygon starting in Krakow, going onto Padua and Florence, proceeding to Paris and London to Edinburgh and completed at Kracow including the Low Countries," which has come to roughly define the European Center, Gavroglu (2012, 311–12).

tered contributions to the history of medicine and mathematics, often authored by scholars interested in the history of their respective disciplines, in Portugal, the emergence of a professional community of historians of science and technology working according to international standards of scholarship is a relatively recent phenomenon, dating back only to the early 1990s. This process profited from the confluence of several events. Among these were the end of a long dictatorial regime spanning half of the twentieth century, the establishment of a democratic regime following the revolution of 25 April 1974 (known as the Carnation Revolution), the subsequent decolonization process from former colonies in Africa and the integration into the European Union. These all posed innumerable challenges, which created the conditions for a reassessment of Portugal's position in the mosaic of nations, and the rethinking of its national identity in relation to other European countries, its former colonies and the world at large. Long held beliefs about national grandeur and the "importance" of Portugal during the last four centuries came under intense scrutiny, both at an academic level as well as in the public discourse. The environment and energy crises, together with severe criticisms towards science and technology as had been previously understood, brought forth the need for different models of development and questioned the strong relation of science and technology to "progress." In this framework, many people started to reconceptualize the past roles of science and technology, which came to include the history of science and technology, as well as the philosophy of science. With the onset of the democratic regime and the free circulation of information, the Portuguese book market opened up and publications skyrocketed. This was the period when Thomas Kuhn's *The Structure of Scientific Revolutions*, (in English or in various translations),[2] and books from participants of the *Annales* school, invaded academic libraries, bookstores and private homes, becoming catalytic for Portuguese intellectuals who started to rethink former models of historical and scientific change. These events set the scene for the creation of the first academic courses in history of science and technology and philosophy of science, as well as the training of those who were to become part of the first generation of professional historians of science and technology.

In the past 20 years, new undergraduate and graduate courses have been created, younger generations have steadily been trained and networking has contributed to the consolidation of the ties among members of this emergent community. At the same time, it has reinforced their participation in international events and collaboration in international research projects and / or networks. On a national level, this community has been discussing new case studies and revising

[2] Despite the fact that Kuhn's *The Structure of Scientific Revolutions* was just translated by a Brazilian publisher, never by a Portuguese one, Spanish and French translations, together with original versions, soon populated academic libraries.

old ones, avoiding the past discourses that often oscillated between nationalistic claims of unacknowledged grandeur or sheer miserablism.

This process has run parallel with a concerted effort to inscribe scientific and technological historical narratives within the narratives of the history of Portugal, which despite new trends, is still often oblivious of the role played by science and technology in shaping past political, institutional, economic and social processes and episodes. At the international level, networking has proved fundamental to discuss, experiment and appropriate various methodological tools suitable to frame the narratives of Portuguese historians of science and technology.

There is much specificity about Portugal that should be kept in mind: Portugal is a very old peripheral European nation, facing the immensity of the Atlantic ocean, which has stabilized mainland borders dating back to mid-thirteenth century, and which possessed a huge and long lasting overseas empire stretching over three different continents (South America, Asia and Africa), posing huge logistical and political problems. Some historiographical frameworks have proved particularly helpful in informing new historical narratives.

In this paper, I will analyze how the recent process of professionalization and internationalization of an emerging community of Portuguese historians of science and technology has profited from its networking abilities, specifically from the ensuing discussion of a number of questions such as: how should one address the study of science and technology in countries of the European periphery, such as Portugal, in order to avoid the pitfalls of a historiography of transmission with its associated Manichaeistic opposition between creative center and passive periphery? How does the attention paid to the role of agents and settings stemming from peripheral contexts enrich the analysis of the circulation of knowledge in extended networks? How do they help to reassess the articulation between the local and the global? How does the perspective from the periphery help to historicize the notion of European science and to deconstruct its purported homogeneity into a dynamic interaction of heterogeneous spaces of practices evolving in an interconnected, lumpy world?

Mapping the Contours of a Young Professional Community

As often happens in peripheral countries such as Portugal, it was due to international networking, and specifically to the efforts of Aldo Mieli, that the so-called Portuguese Group of History of Science was created in the early 1930s, during the dictatorial regime of António de Oliveira Salazar. The group was responsible for the organization of the Third International Congress for the History of Science in 1934, in which George Sarton, then president of the Académie Internationale d'Histoire des Sciences, participated and delivered the inaugural speech.[3] They launched a journal called *Petrus Nonius*, the Latin version of the name of the sixteenth-century Portuguese mathematician and chief-cosmographer, Pedro Nunes, who had fostered the transformation of navigation from a practical art into a scientific subject. The journal ran from 1937 to 1951, the year in which the group was dismantled. Eclectic in composition, the group included mostly university professors or academics, specifically physicians and mathematicians, whose approaches often resembled those of amateur scientist-historians.[4] Many held positions at the University of Coimbra, the oldest institution for higher education in Portugal. Besides articles from members of the group and foreign experts, the journal published news of events related to the discipline, including conferences and courses, and discussed the connections between history of science and the teaching of science. The prominence given to topics in the history of the nautical sciences (fifteenth and sixteenth centuries) and to a lesser extent, to subjects associated with the eighteenth century reforms of the enlightened despot, Marquis of Pombal, reflected a nationalistic zeal to claim a place for Portugal in the scientific history of Europe. Furthermore, by often emphasizing the establishment of priorities as an integral part of the construction of a national historiography, papers often called attention to the purported originality of local scientists.[5]

Nationalistic (and colonialist) discourses were also typical of many commemorative events organized by the dictatorship, such as the 1937 First Congress on the History of Portuguese World Expansion, and the Congress on the History of Portuguese Scientific Activity, which took place during the impressive Exhibition of the Portuguese World (1940), organized to celebrate the nation's foundation in 1140 and its independence from Spain in 1640 (Corkill 2005). In these partisan celebrations the past of the sciences was used in a master narrative to exemplify the leading role of the country in building Western civilization.

From the 1940s onwards, the history of the nautical sciences, cartography and medicine, attendant to the history of Portuguese maritime expansion, grew as the result of the research of an handful of outstanding Portuguese historians (with backgrounds in science), including the brothers Jaime Cortesão (physician) and Armando Cortesão (agronomist engineer), Luciano Pereira da Silva, Duarte Leite and Luís de Albuquerque (all mathematicians by training). They often op-

[3] Sarton's recollections were published in *ISIS* (1935).

[4] Both in the history of mathematics and the history of medicine, a tradition can be identified going back at least to the nineteenth century. The physician Maximiliano Lemos contributed to the history of medicine, and the mathematician Gomes Teixeira to the history of mathematics, both of them playing leading roles in conferring authority and credibility to their respective areas.

[5] For a detailed discussion of the events mentioned in the first three paragraphs of this section, see Simões, Carneiro, and Diogo (2008). In a sense, this paper is a follow up of the discussion initiated therein.

posed the received historiography with its congratulatory and patriotic overtones, although from different perspectives. The Cortesão brothers, for example, took the standpoint of the History of Portugal, for which colonialism and territorial expansion were part and parcel of "being Portuguese" (*portugalidade*), while Albuquerque took the viewpoint of the history of science. He discussed with erudition and rigor the role played by the navigational practice of pilots and seamen, as opposed to a theoretical knowledge of nautical sciences, in informing successive stages of the oceanic voyages.[6] Through Albuquerque, the Dutch historian of science, R. Hooykaas became aware of the role of the Portuguese oceanic voyages and geographical expansion, which for European and North American historians were obfuscated by those of the Spanish and the Dutch. Hooykaas took it upon himself to bring the attention of the international community of historians of science to the role of the Portuguese encounters in setting the stage for the emergence of modern science (Hooykaas 1966).[7]

A decade after the disappearance of the Portuguese Group of History of Science, an attempt to introduce history of science courses in the curricula of undergraduate science courses at the University of Coimbra took place due to the connection between Albuquerque and Hooykaas. While this attempt failed, it was only in democratic Portugal after 1974 that history of science courses were successfully introduced into the undergraduate curriculum in some Portuguese universities. The longstanding interest of some science professors at the University of Coimbra explains why the historian of science Allen Debus was invited to teach some short courses in the 1980s. It also accounts for the parallel organization of an international meeting, sponsored together with the International Union of the History and Philosophy of Science and the International Council of Scientific Unions (Debus 2009), as well as the organization of two celebratory symposia on the occasion of the bicentenary of the Academy of Sciences of Lisbon (founded in 1779), in which both Debus and the historian of science William Shea participated. These symposia gave way to the publication of various volumes addressing history of science in Portugal, often from a positivist perspective, unaware of recent trends in the social and cultural history of science (Peixoto 1986; 1992).

Contrary to the Anglo-Saxon world, history of science and history of technology courses were introduced in the mid-1970s in Faculties of Sciences and Faculties of Science and Technology, not in Departments of History or integrated in Faculties of Humanities. They became part of the curricula of science and

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[6] The bibliography on this topic is extensive. See, for example, Albuquerque (1988).

[7] The impressive contributions to Enlightenment science in Portugal by the secondary school teacher, autodidact historian and poet, Rómulo de Carvalho, also date back to this period. For more details, see Simões, Carneiro and Diogo (2008).

engineering undergraduates (then amounting to a five year study program). At the Faculty of Sciences of the University of Lisbon, the introductory course titled "History of Ideas in Physics" was created, following the establishment of the democratic regime and upon the request by undergraduate students. It was taught by the physicist João Luís Andrade e Silva, a former PhD student and long-time collaborator of the Nobel Prize winner Louis de Broglie. Andrade e Silva was an enthusiast of history of science, deeply influenced by Alexandre Koyré.[8] Following this course, many students of science read Kuhn's *The Copernican Revolution* (1957) and *The Structure of Scientific Revolutions* (1962). Despite the initial attraction to concepts such as revolutions, paradigms, crisis and incommensurability, which at the time bore such strong resonance to the national political landscape, in the long run it was the constraints imposed on scientists by training and the role of educational institutions, the multifarious faces and inherent complexity of the practice of normal science, the importance of community building and its space-bound specificities, the role of resistances, persuasion and of legitimizing processes, all of which offered future Portuguese historians of science and technology conceptual tools to analyze the meanders of scientific and technological practices in the European periphery.

During the late 1980s, the first group of Portuguese students went abroad to get their PhDs in the History of Science and the History of Technology, most of them already holding undergraduate degrees in science. Certainly, these contingent events have shaped the ways and sites in which history of science and technology has developed as a new teaching and research field. There are both advantages and shortcomings in the proximity of historians of science to scientists' and engineers' academic environments, and the concomitant distance from historians. In any case, the physical separation from historians' working places has materialized in the unfortunate inexistence to date of history of science and technology courses in history departments and the awkward intellectual gap often separating Portuguese historians of science and technology from historians. The fact that in counteracting this old trend, an increasing number of professional historians of science and technology hold undergraduate degrees in history (often with a strong background in the *Annales* tradition in economic history, history of culture and history of ideas) may help bridge the gap between the two communities and ease the way to an effective integration of scientific and technological historical narratives within narratives of the history of Portugal.

In the early 1990s, following the integration of a small group of historians of science and technology trained abroad at several faculties of science,

[8] Others, such as Ana Luísa Janeira and António Manuel Nunes dos Santos, followed the lead of Andrade e Silva in teaching history of science courses.

post-graduate courses were created[9] simultaneously with the establishment of research units, which were regularly evaluated by international panels and funded by the Portuguese Foundation for Science and Technology (FCT).[10] These were established in such a way that the minimum conditions were in place to develop and consolidate a community of professional historians of science and technology abiding to international standards of scholarship, publishing in international scholarly journals (not just in national ones) and entertaining networking ties with the international community.[11] The initial years of this steady professionalization process have run parallel with episodic contributions to the discipline by people marginally related to it, and to the participation of scientist-historians (especially mathematician-historians), a trend that has tended to diminish in time.[12] To date, there is still no active scientific society.[13]

Of particular importance, not only for the consolidation of international connections, but also for the creation of strong bonds among members of the emergent national community, was the participation in the project *Prometheus*, funded by the European Commission and coordinated by Kostas Gavroglu. The project aimed at studying how the ideas and practices stemming from the Scientific Revolution circulated and were appropriated by actors of the countries of the so-called European peripheries, especially during the Enlightenment.[14] In what concerns Portuguese participation, it set the stage for a prosopographical study of the pro-

[9]Presently, there are two graduate programs on the history and philosophy of science, (both in Lisbon), one including both a MSc and a PhD program and offered at the Faculty of Sciences of the University of Lisbon, the other at the Faculty of Sciences and Technology of the New University of Lisbon, offering only a PhD program. A proposal to fuse the two PhD programs into one is under evaluation. A Minor in history and philosophy of science (corresponding to one semester out of a three-year undergraduate degree) is also offered at the Faculty of Sciences of the University of Lisbon. There are not yet undergraduate degrees being offered in these areas.

[10]At present there is just one research center on the history of science and technology accredited and funded by FCT. The center located in Lisbon is called the Interuniversity Center for History of Science and Technology (CIUHCT), and resulted from the fusion of two research centers associated with the University of Lisbon and the New University of Lisbon.

[11]By professional historians of science and technology, I mean those who hold PhD degrees in the field and / or those who, regardless of their fields of origin, publish regularly on scholarly international forums.

[12]See Tavares and Leitão (2006). A similar survey is being done for the following decade.

[13]A Portuguese Society for the History and Philosophy of Science was founded in 1988, but its existence never went beyond the formalities of its creation. Following the professionalization of historians of science and technology and their scarce collaboration with the small community of philosophers of science, the aims of such a society were preempted. The agenda of the community of historians of science and technology has not included the creation of a society for the history of science and technology.

[14]CE, Human Capital and Mobility, Scientific and Technical Cooperation Networks, "Project Prometheus. The Spreading of the Scientific Revolution From the Countries Where it Originated to the Countries in the Periphery of Europe during 17th, 18th and 19th Centuries"(1994–1996), CHRX-CT93-0299.

files (educational, religious, political and scientific) of the eighteenth century *estrangeirados* (Europeanized intellectuals) and the analysis of the contours of their appropriation of the ideas and practices of the new sciences, their networking tactics and legitimizing strategies, often inspired by their determined will to contribute to the modernization of their country (Simões, Carneiro, and Diogo 1999; Carneiro, Simões, and Diogo 2000).

Presently, the community of Portuguese professional historians of science and technology includes roughly 30 senior historians, and roughly the same amount of postdoctoral scholars. Senior historians of science and technology are mostly professors affiliated with major Portuguese universities (University of Lisbon, NOVA – New University of Lisbon, University of Évora, University of Coimbra and University of Aveiro), with a noticeable concentration in Lisbon. A few are researchers associated with universities, research institutes or museums of natural history and of science. There are around 40 PhD students and a much smaller number of MSc students. In the Lisbon area, there are currently 50 historians of science and technology (30 senior and junior scholars, and 20 post-docs, including Spanish, Italian, Swiss and German members) and around 20 graduate students.[15] They are mainly affiliated with the University of Lisbon and NOVA—New University of Lisbon, and simultaneously members of the Interuniversity Center for the History of Science and Technology (CIUHCT). The history of science and technology has benefitted considerably from the program launched by the Portuguese government in 2007–2008 aimed at hiring junior post-docs at an international level and recently (2014) the Interuniversity Center for the History of Science and Technology was classified as Exceptional (the highest mark awarded) by an international panel of the European Science Foundation. However, the recent European economic crisis and its repercussions in Portugal, not only in what relates to the entrepreneurial reforms of the university system and the attacks suffered by the humanities and social sciences, but also in relation to cuts in research funding, may well be responsible for a reversal of this promising situation.

The degree of internationalization of this community can be measured in part by its ability to organize major international conferences[16] and participate in sev-

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[15] These numbers should be viewed against a population of roughly 10 million inhabitants with a narrow 10% holding undergraduate degrees, and a high 2.7% holding graduate degrees (relative to the population in the age range of 30–34 years). For more information on the activities of this research unit see (http://www.ciuhct.org).

[16] A considerable number of national and international meetings have recently been organized by the Portuguese scholarly community, and others are in preparation. They include the 2nd STEP Meeting (2000), HoST Annual Meetings associated with the online journal HoST (since 2006), 19th-Century Chemistry: Spaces and Collections (2007), the XXVII Symposium of the Scientific Instruments Commission (2008), SHOT 50, the Society of the History of Technology Annual Meeting (2008), Annual

eral networks. Examples of this are the international group Science and Technology in the European Periphery (STEP), Tensions of Europe (ToE), International Network of Engineering Studies (INES) and several other European networks which included "Circulation of Knowledge in Early Modern Science," "Scientific Periodicals in Modern Europe," and "Thesaurus-Network of Portuguese and Brazilian Museums of Science."

This networking reflects the importance conceded by the Portuguese community to explore the interface between history of science *and* history of technology, which has been facilitated by the strong ties entertained between Portuguese historians of science and historians of technology, to the extent that in the Portuguese context, one should talk about a community of historians of science *and* technology. This community is behind the creation of an online international journal specifically devoted to the history of science *and* technology (HoST). Launched in 2007, it aims to strike a balance between local concerns and international trends, interweaving history of science and history of technology (and also at times history of medicine) and extending the geography of papers' provenance, while giving a place for contributions by Portuguese authors.[17]

Networking options also reflect a view of history of science *and* technology broadly conceived in order to include material culture, instruments and scientific collections (Pantalony 2009). This last connection has profited from the relation of the Interuniversity Center for the History of Science and Technology to the University of Lisbon, which houses an impressively broad scientific heritage, integrated in the National Museum of Natural History and of Science, located at the heart of Lisbon, and including an impressive nineteenth Chemical Laboratory and Amphitheatre, the Botanical Garden and two nineteenth-century astronomical observatories, one devoted to teaching and the other to research (Simões, Diogo, Carneiro, 2012). This relation also accounts for the preeminence of research topics focusing on Portuguese institutions, approached from various perspectives and

International Workshops Libraries and the Scientific Book (fifteenth–seventeenth centuries) (2011, 2012, 2013), Annual International Workshops History of Iberian Cartography. Old Maps, New Approaches (2012, 2013), History of European Universities. Challenges and Transformations (2011), University Collections. University History and Identity (2011), and more recently the meeting Shaping Landscapes and Building Expertise. The Role of Imperial Technology in the Making of the 19th and 20th Century World (2013), the 2014 ESHS meeting and the 2014 STEP meeting. See (http://www.ciuhct.org).

[17]So far, issues have been often, but not always thematically organized, addressing topics of relevance both to the national and the international communities. Examples of this are The Circulation of Science and Technology; The Fascistization of Science; Moved Natural Objects. Spaces in Between; and Communication Science, Technology and Medicine. See (http://www.johost.eu).

for the importance conceded to the organization of exhibitions, which brings the history of science to the public at large.[18]

The scarcity of bibliography on the history of science and technology in Portuguese libraries and the limited accessibility to online journals is still a problem haunting the community of historians of science and technology. In addition, many Portuguese archives are difficult to access and are often poorly organized or not organized at all, and offer only reduced timetables for the interested public. Small steps to circumvent this constraining context include translations of recent landmarks of the literature on history of science,[19] publication of primary sources, (both printed and manuscript [20]) and the offer from the well-known historian of science S. G. Brush to use his private library.[21]

The community of professional historians of science and technology covers a wide variety of thematic areas, ranging from early modern science, to science and technology in the twentieth century. With a few exceptions, most publish on Portuguese topics. This fact does not indicate a lack of internationalization, but rather the willingness to unveil and interpret many new episodes, revise received views in the cases in which they exist and offer case studies informed by recent mainstream historiographical trends, and in the process enrich international scholarship with case studies stemming from the history of science and technology in Portugal. The exploration of alliances with the community of Brazilian historians of science and technology is also deemed fundamental, but is still at an initial stage. Historians of science and technology apply a broad range of methodological approaches, most of them in accordance with recent trends in the social and cultural history of science and technology, as well as science and technology

[18] Examples are "360º Ciência descoberta," Fundação Calouste Gulbenkian (2013), Exposição *Medir os Céus para dominar a Terra: a Astronomia na Escola Politécnica de Lisboa, 1837–1911*, Museu de Ciência da Universidade de Lisboa (2009–2010), "À Luz de Einstein," Fundação Calouste Gulbenkian (2005).

[19] I refer to the collection titled *História e Filosofia da Ciência* (*History and Philosophy of Science*) published by the well-known publisher Porto Editora and organized by Ana Simões and Henrique Leitão. Starting in 2003, 14 volumes came until the collection was discontinued recently "due to the economic crisis." See (http://www.ciuhct.com).

[20] The publication of *Obras de Pedro Nunes* (*Complete Works of Pedro Nunes*) coordinated by Henrique Leitão, with extensive critical comments, is coming to its end and is an impressive scholarly venture. Other examples include the publication of catalogues listing the rich collections, for long unknown, of scientific manuscripts held at the National Library of Portugal (BNP), or the Collection titled *Ciência e Iluminismo* (*Science and Enlightenment*) published by the well-known publisher Porto Editora, and organized by Ana Simões, Francisco Contente Domingues and José Luís Cardoso, which includes printed and manuscript transcriptions of works by Portuguese eighteenth century natural philosophers, and was discontinued after the publication of 9 volumes due to the crisis. For other ventures, see (http://www.ciuhct.com).

[21] Recently, some of the books by the physicist Per Dahl were also offered to CIUHCT's branch located at the Faculty of Sciences of the University of Lisbon.

studies, including an integrated approach to material culture and collection-based history of science and technology.

Networking and Historiographical Options

In the past 20 years, the conceptual and methodological contributions from social and cultural history of science, together with post-colonial and subaltern studies, have put emphasis on science and technology in action, the situatedness of knowledge production and the emphasis on the circulation or transit of knowledge as a creative process, as well as on the innovative role of peripheries and colonial spaces and agents.[22] However, this theoretical apparatus has been applied mainly to peripheral and colonial spaces that have a close relationship with the so-called European Center as a common characteristic, either as "satellites" of centers located in their mainland territories or as part of their colonial empires.

European peripheral countries and their colonies remain in the shadows for a variety of reasons, including the language barrier and more difficult access to historical sources. In what relates to Portugal, it fell on the emerging community of historians of science and technology to reverse this state of affairs, aided by its networking abilities and especially by its participation in the international groups Science and Technology in the European Periphery (STEP) and Tensions of Europe (ToE), all the more so that through its history, Portugal has become a privileged laboratory for the study of European and colonial topics, as well as those related to centers, peripheries and ultra-peripheries.

The frameworks developed within these international networks have contributed to the ongoing debates on the various difficulties that have hampered a systematic study of the sciences and technology in the *European periphery*, the dynamics of the hidden agenda of Europeanization, the process of *Europeanizing the World* and *Provincializing Europe* and the role of both as privileged standpoints to illuminate and deconstruct the notion of *European science and technology*, in the sense of enlightening the process of emergence of science and technology as a global phenomenon and as one of the main building blocks in the construction of an imagined, European intellectual identity.

The international group STEP was created in 1999, and presently gathers around 200 members from 30 different countries and four continents (Europe, North and South America, Asia and Oceania). A considerable fraction of its members come from the European periphery, especially from Greece, Portugal and Spain, where a substantial part of the group's founding members are from.

[22] The bibliography on these topics is extensive and well known to readers of this volume. Some representative examples are Biagioli (1999); Chakrabarty (2007); Raj (2007); Schaffer et al. (2009); Secord (2004); Simon and Herran (2008); Sivasundaram (2010).

It is purposefully a loosely structured group, sharing a website and a discussion list.[23] The group organizes conferences every two years, which were thematically arranged until 2008.[24] Besides individual publications, it has published several collective volumes[25] as well as historiographical reflections related to science and technology in the European periphery.[26]

The study of the circulation of science and technology within Europe has been done in such ways as to overcome the constraints of local contexts often heavily tinted by positivist approaches, avoid the dangers of parochial antiquarian approaches, solve the problem of fragmentation produced by a myriad of local studies and at the same time, exploring ways to tie research endeavors with mainstream historiography. By using different methodological approaches to discuss a variety of topics, encompassing travels, textbooks, popularization of science and technology, science and technology in the press, national historiographies of science, science and religion, universities, transnational histories, science and gender and so on, the contours of a new historiography of science and technology in the European periphery (EP) have been delineated.

By criticizing the value-ladenness associated with the center-periphery dichotomy and the assumptions behind diffusionist models, they moved away from a historiography of transmission to a new historiography built on the concept of appropriation. The concept of appropriation stems from cultural history and calls attention to the specificities of the "receiving" culture, with its social, political, religious and cultural specificities. In this new framework, the local agents are endowed with creative functions, and attention is paid to the ways practices are transformed when they move from one place to the other. Furthermore, appropriation draws attention to the fact that when practices "arrive" at a certain place, they are never integrated into an ideological vacuum. On the contrary, they are articulated with the multiple cultural traditions of a specific society at a particular moment of its history. New scientific discourses are articulated in the local context, legitimizing strategies and spaces are created and resistance to the new

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[23] Website: (http://147.156.155.104). List: NODUS: Science and Technology in the European Periphery e-mail list (nodus@uv.es).

[24] Scientific Travels, Lisbon, Portugal 2000; Scientific and Technological Textbooks, Aigina, Greece, 2002; Traditions and Realities of National Historiographies of Science, Aarhus, Denmark, 2004; Scientific and Technological Popularization in the European Periphery, Mao, Minorca, 2006; Looking Back, Stepping Forward, Istanbul, Turkey, 2008; Galway, Ireland, 2010; Corfu, Greece, 2012; Lisbon, Portugal, 2014.

[25] STEP volumes coming out of STEP conferences include Belmar et al. (2006); Papanelopoulou, Nieto-Galan and Perdiguero (2009); Simões, Carneiro and Diogo (2003); Special Issue *Nuncius* (2008). For more publications by the STEP group see the group's website.

[26] Examples are Fontes da Costa and Leitão (2008); Gavroglu (2012); Gavroglu et al. (2008); Nieto-Galan (2011); Patiniotis (2013); Patiniotis and Gavroglu (2012); Raposo et al. (2014); Simon and Herran (2008).

practices usually emerges. The local peripheral context "chooses" to be influenced in certain specific ways, and choices are taken in simultaneity with the rejection of various forms of influence.

The former historiographical standpoint was aimed at unravelling the specificities and contours of appropriation processes that took / take place in different peripheral contexts, in different periods and for different thematic situations. By stressing various and multidirectional responses, one contributes to the international historical scene with a variety of new case studies, which enrich current views and often revise received ones. Additionally, and without eliminating asymmetries, its main purpose is to highlight *similarities*, rather than differences, among the various peripheral contexts in order to unveil common trends.[27] This novel enterprise is oriented towards writing a historical narrative, which will concur to the emergence and structuring of a concept of *periphery*, beyond the traditional center-periphery dichotomy with its associated value judgments, and based on the awareness of the dynamics of the historical co-construction of *both* centers *and* peripheries. The re-definition of the concept of periphery should be mainly operational, in the sense of enabling historians to move from the *perspective of the center* to the *perspective of the periphery*. In this sense, *Science and Technology in the European periphery* is taken as a *historical problem* while the *European periphery* becomes a *historical actor*.

ToE is an international network consisting presently of almost 300 social scientists. Like STEP, it was founded in 1999, but contrary to STEP, its founding members were social scientists from Great Britain, Germany, Sweden and the Netherlands, who were joined in subsequent years by scholars from Southern and Eastern Europe. Biannual conferences, summer schools and a series of 20 thematic workshops were organized (one in Lisbon in 2000). By 2004, around 200 social scientists from over 21 countries had already joined the network. The project "Inventing Europe: Technology and the Making of Europe from 1850 to the Present"[28] was presented during the first ToE conference in Budapest (2004) and has given way to a major publication of a new history of Europe: a six-volume book series *Making Europe: Technology and Transformations, 1850–2000* of which the first volumes have already been published.[29]

[27] Traditionally, a sub-group of comparative reception studies has been concerned with accounting either for the *differences* between centers and peripheries or between peripheries. While there are not many comparative studies written by "peripheral" authors, impressionistic comments are abound, oscillating between a hagiographic type and the rhetoric of backwardness or decadence. In turn, the accounts about peripheries built up by historians of the so-called centers tend to assess peripheries using criteria stemming from the center, thereby overlooking the creative role of peripheries.

[28] (www.tensionsofeurope.eu/www/en/files/get/Intellectual_Agenda.pdf).

[29] See (http://www.makingeurope.eu). So far, the books Oldenziel and Hård (2013); Kohlrausch and Trischler (2014) have come out. A virtual exhibit will accompany the book series, allowing for a more

A broad range of themes has been explored, centered on the role of technology as an agent of change in European history in the nineteenth and twentieth centuries. Unveiling both collaborative agendas and fierce disputes, it focused on the linking and delinking of infrastructures, the emergence of transnational technical communities and the circulation and appropriation of artifacts, systems, knowledge and people, both within Europe and former European empires (Misa and Schot 2005; Schot, Misa, and Oldenziel 2005). The analysis of the role played by European / Western technology in the organization and hierarchical structure of colonial and postcolonial worlds became a topic of keen interest very early on, together with issues such as mobility, the rise of consumer society, agriculture and food, communication, big technological systems, military technology and information systems. Unlike traditional accounts of European integration, mainly based on a political approach, which highlights the international relations between nation-states, the emphasis has been on a historical narrative based on how the design and uses of technology became critical actors in the "hidden integration," but also in the "hidden fragmentation" of Europe.

Inspired by the two former theoretical frameworks, the community of Portuguese historians of science and technology has framed the study of science and technology in Portugal by analyzing the dynamic relationships of Portuguese actors (scientists, engineers, agents of various profiles and institutions, etc.) among themselves and with actors of other countries (European or otherwise). In addition, they scrutinize how scientific, technical and engineering expertise was crucial to the Portuguese agenda concerning management and exploitation of the overseas territories in Africa from the mid-nineteenth century to the 1970s, and finally, elucidating how *both* science and technology have informed successive political agendas and have been used as tools (often forgotten yet extremely powerful) of the former Portuguese Empire. Furthermore, they have looked at the role played by travels, circulation and networking; the writing of books, textbooks, papers and the exchange of correspondence; the creation of scientific and technical institutions; the material vehicles used for the communication of science and technology in a country with a largely illiterate population and the images of science and technology they conveyed.[30]

They have done so by shifting the emphasis from transmission to appropriation, from the perspective of the center to the perspective of the periphery and from the isolated study of the periphery to the comparative assessment of developments. This theoretical shift has informed historical narratives produced

interactive approach to the topics discussed in the books, and providing an innovative pedagogical instrument useful for teaching purposes

[30] Some examples are Carolino and Simões (2012); Diogo and Amaral (2012); Silva and Diogo (2006); Simões and Carolino (2014); Simões, Carneiro and Diogo (2012); Simões, Diogo and Carneiro (2012b, 2012a); Simões, Zilhão, et al. (2013).

by Portuguese historians of science and technology when dealing with European and other spaces (often colonial), due to the specificities of Portugal—a small peripheral European country, which acted towards its huge overseas empire as the central metropolis. More recently, they have been exploring ways to reappraise the recent historiography of circulation, by focusing on the associated notions of exchanges, displacements and translations, not only as a way of mobilizing knowledge but also as a way of producing it. They also focus on *locality* as a notion not necessarily coincident or constrained by *location*, and on how closely scrutinizing the relations between purported centers and peripheries will give way to a much more nuanced picture of circulation within networks of evolving lumpiness (Raposo et al. 2014).

Concluding Remarks

Conversant with the new trends in social and cultural history, it is not too optimistic to predict that the first preliminary overview of many episodes can be offered, answering new questions, revising old ones and contributing in the not so distant future in the creation of a "big picture" of the history of science and technology in Portugal. On one hand, Portuguese historians of science and technology are contributing to the enrichment of recent narratives of early modern Iberian science and technology, complementing the wealth of new sources already analyzed and re-assessing historiographical revisions proposed,[31] with narratives offering an integrated and balanced account, able to explore the similarities and differences in the scientific contributions of the two Iberian countries.[32] On the other hand, in what relates to later periods (from the seventeenth and eighteenth centuries up to the present), and again focusing on the circulation of people, instruments, objects and skills, contributions will help to build an integrated historical narrative by focusing on the co-production of scientific and technological knowledge and its various forms of circulation *and* the political agendas of the different political regimes which ruled Portugal and its colonies during these centuries.

In sum, the goal of the Portuguese community of historians of science and technology has been threefold. By stressing the circulation of science and technology of all sorts of agents including experts, expertise and skills, between European countries, between Europe and its overseas colonies, as well as between colonial powers, and by bringing to the forefront the case of smaller peripheral countries which acquired power in Europe through the translation of colonial into

[31] Now already standard accounts of this new trend are Barrera Osorio (2006); Bleichmar et al. (2008); Cañizares Esguerra (2002, 2004, 2006); Navarro Brotons and Eaman (2007); Pimentel (2001); Portuondo (2009).
[32] See, for example Leitão (2006, 2007, 2009); Leitão and Alvarez (2011).

national power in the global arena, they have sought to enrich the international literature on the history of science and technology with new case studies stemming from a country of the European periphery, and in the process contributing to the rewriting of the history of Portugal in such a way that science and technology play a central role and at the same time, inscribing their narratives within standard accounts of the history of Europe.

Acknowledgments

I would like to thank Maria Paula Diogo, Kostas Gavroglu, Ana Carneiro and Henrique Leitão for their critical reading of this paper, Maria Luísa Sousa for help with the bibliographic references and the referees for their remarks and suggestions.

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