Chapter 19
The Pitfalls and Possibilities of Following Koyré: The Younger Tom Kuhn, "Critical Historian," on Tradition Dynamics and Big History

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Abstract Late in his career, Thomas S. Kuhn practiced more as a philosopher of science than as a historian of science. However, his earlier work—leading up to *The Structure of Scientific Revolutions* and during the majority of his tenure in the Princeton history of science group—focused on "mapping" the shape of the history of the physical sciences and on modeling the dynamics, or "motor," of scientific traditions. This paper examines the younger Kuhn's excursions in map and motor design. It views Kuhn as a "critical historian," that is, a historian who constructs explanatory categories in order to apply them to large-scale narratives, evaluation of which can suggest modification of those guiding categories.

The younger Kuhn's map and motor design was largely shaped by the work of his historiographical idol, Alexandre Koyré. Kuhn's creative articulation of Koyré's position explains his innovations concerning Scientific Revolutions (plural), his loosening of Koyré's central category of "metaphysics," and his invention of the crucial conception of "normal science." Additionally, Kuhn's devotion to Koyré explains some historiographical pitfalls and blind spots that bedeviled his historical work: for example, his ignoring early modern natural philosophizing as an institution and culture in its own right and his failure to capitalize on his correct insight into the nature of scientific discovery as the nonrevolutionary yet tradition-modifying core process in the sciences. The paper is concerned with Kuhn's work as a critical historian and his legacy for younger historians, not with philosophical debates about his texts.

Keywords Thomas S. Kuhn • Alexandre Koyré • The Scientific Revolution • Discovery • Historiography of science • Kuhnian normal science • Kuhnian revolutionary science • Metaphysics of science • Experimental sciences • Sociology of scientific knowledge • Internalist/externalist debate

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19.1 Introduction: Tom Kuhn, Critical Historian (Circa 1958–1977)

Late in his career, Thomas S. Kuhn practiced more as a philosopher of science than as a historian of science. However, his earlier work—leading up to *The Structure of Scientific Revolutions*[1] and during the majority of his tenure (1964–1979) in the Princeton history of science group—focused on what I shall call "mapping" the shape of the history of the physical sciences and on modeling the dynamics, or "motor," of scientific traditions.[2] This paper examines the younger Kuhn's excursions in map and motor design. It views Kuhn as a "critical historian," that is, a historian who deliberately constructs explanatory categories in order to apply them to large-scale narratives, evaluation of which can suggest modification of those guiding categories.[3]

I argue that much about the younger Kuhn's map and motor design was shaped by the work of his historiographical idol, Alexandre Koyré. Kuhn's creative articulation of Koyré's position explains his innovations concerning Scientific Revolutions (plural), his loosening of Koyré's central category of "metaphysics," and his invention of the crucial conception of "normal science." Additionally, Kuhn's devotion to Koyré explains some historiographical pitfalls and blind spots that bedeviled his historical work: for example, his ignoring early modern natural philosophizing as an institution and culture in its own right and his failure to capitalize on his correct insight into the nature of scientific discovery as **the** nonrevolutionary yet tradition-modifying core process in the sciences. There is a complex dialectic to be unraveled in the younger Kuhn's creative yet self-consciously devoted relation to Koyré. Similarly, there is a complex dialectic of relation between post-Kuhnian historians and sociologists of science and the younger Kuhn, whose historiographical thinking unintentionally pointed toward innovations that were to be Kuhnian in spirit but mostly rejected by Kuhn himself. Note that I say post-Kuhnian historians and

[1] Hereafter cited as *SSR*, from the 2nd edition of Kuhn (1970). The first edition was published in 1962.

[2] One could take the terminus point for Kuhn's activity as a critical historian as 1977, with the publication of Kuhn (1977a) and particularly the essay, Kuhn (1977c), which first appeared in 1976. In 1979, Kuhn moved to MIT., but he had been on leave and away from Princeton quite a bit in the previous two years. I restrict the use of the term "young" for Kuhn up through 1962. "Younger" covers that period and on through 1977.

[3] Any field of history, including history of science, consists of two interacting levels: One is where we craft together our public, published products—narratives imprinted with explanations of what was happening and why. The other level is where one designs the categories that are being both applied and revised in our narrative/explanations. Some areas of history require more concentrated attention to that second level than others—history of science is one of them. Hence, a critical historian is one who explicitly attends to the formation of categories and evaluates the goodness of the narrative/explanations in which they are deployed, with an eye to modification and improvement. The younger Kuhn certainly was a critical historian, especially concerned with the legacy of Koyré.

sociologists, not philosophers. We are concerned here with Tom Kuhn as a practicing and explicitly theorizing historian.

Before we begin, several caveats need to be set out. First of all, this essay is not an exercise in textual hermeneutics concerning Kuhn's published references to Koyré. I envision something broader, deeper, and more implicit in what Kuhn was doing (and, as we shall see, sometimes signaled in Kuhn's teaching history graduate students about Koyré and the challenges of big, highly theorized history of science). Second, I avoid any hasty identifications of the younger Kuhn with particular post-Kuhnian developments. For example, Kuhn is not treated here as a "forerunner" of a "post-Kuhnian" sociology of scientific knowledge (hereafter SSK) or of historical epistemology. We shall see that Kuhn's own concern with the micro-dynamics of traditions does not map very well onto later SSK and where it does this best, as in the nature of discovery and continuous change in scientific research traditions, Kuhn, for reasons we shall explore, did not pursue those insights. Finally, this study is in part intended to help shape the outlook of young scholars in the field of history of science about historiography and about Scientific Revolution studies. In this sense, the paper continues and radically updates the kind of questioning of Kuhn's intentions, accomplishments, and relations to Koyré that I and other apprentice historians of science, then under his direct tutelage at Princeton, used to pursue in the 1970s.[4]

19.2 The Young Tom Kuhn on "A Role for History"

The young Kuhn, critical historian, appears in the first chapter of *SSR*, "A Role for History." Kuhn tells us that the stimulus for his project was his observation that his studies of the development of science simply failed to match up with accounts given by abstract methodologies of science or by science textbooks. He did not mean that brute historical facts speak for themselves. Rather, he meant (1) that scientists and philosophers have no monopoly on purveying frameworks of interpretation to historians and (2) that historians are their own best purveyors, building up revisable models of process that are applied to narration and open to revision in the light of

[4] I was a graduate student in the Princeton Program in History and Philosophy of Science, Department of History, from September 1969 to August 1973. From September 1973 to July 1974, I was an "instructor" in the HPS Program and the Department of History. Michael S. Mahoney was the chief supervisor of my doctoral dissertation, *Descartes and the Scientific Revolution: An Interpretation*, and Kuhn was co-supervisor. In August 1974, I began my first regular academic appointment in the Division of History and Philosophy of Science, Department of Philosophy, University of Leeds. Its senior member, Jerome R. Ravetz, was a keen student of the work of Kuhn, and his seminal, *Scientific Knowledge and Its Social Problems* (1971), had enlarged and improved the Kuhnian model for "normal science" in ways paralleling the contemporary initial development of post-Kuhnian interpretative sociology of scientific knowledge, in the hands of scholars such as Mulkay (1979), Collins (1975), Barnes (1974, 1982), and Shapin (1982, 1992). My first comment on these Kuhn/post-Kuhn developments was Schuster (1979).

further inquiry and debate. Kuhn intended to be one of these historian-purveyors—to suggest a model for the dynamics of process of the sciences and to map in narration large regions of the history of the sciences.

Kuhn also states that he intends to correct the idealizations and abstractions of philosophy of science. But, rather than conclude that he was mainly or entirely a philosopher at that moment, perhaps we should work on the assumption that these corrections were intended to come from providing precisely what I have called a mapping and motor. If that is so, it is also plausible that the young Kuhn underestimated the difficulties involved in proposing to professional philosophers that his historiographical conceptualizations could serve in their own field as a substitute for their work. For purposes of this study, therefore, I assume that as far as Kuhn was concerned at this stage of his career, what were at stake were properly historical technique and problematics, that is, the professional concerns of intelligent and critically aware historians. If philosophers were to be corrected and even if Kuhn at some deep level viewed himself as a philosopher, it is still the case that he, perhaps naively, thought that his winning position in philosophy would arise from this kind of critical historical work. Certainly, having moved to Princeton two years after the 1962 publication of *SSR*, he was for the next few years mainly involved in teaching apprentice historians of science who were officially located in the great Princeton Department of History and therefore also studying the economic and social history of their preferred historical period.

As one of those apprentices, exposed to Kuhn's teaching and informal historiographical lessons in graduate courses (called seminars at Princeton), I can report that many of us thought we were being shown deep problems, and solutions, about the shape of the history of Western sciences and how their dynamics should be studied and set out in narrative *cum* explanations. Additionally, most of us saw affinities between Kuhn's historical theorizing and the grand, layered structures of explanation (cultural/ideological, social, and economic) being offered by Princeton's two leading experts on European early modern history, Lawrence Stone and Theodore K. Rabb. Both of these formidable scholars were interested in long time periods, critical awareness of interpretive frameworks, as well as the historiography of the Scientific Revolution itself.[5] Kuhn, who held an endowed chair in that same Department of History, presumably felt, as we did, the gravitational pull of deep critical thought about history.[6]

[5] Consider synthetic works of these authors: Stone (1972) and Rabb (1975).

[6] I am not suggesting that Kuhn himself was accomplished in social, political, or economic history. He was not, and he often pointed that out to the graduate students, telling us explicitly that whatever history of science problem we worked on, we would require gigantic loadings of knowledge of "context, context, context," knowledge he was in no position to impart.

19.3 Alexandre Koyré as Historian of Science: Kuhn's Point of View

To the young Thomas Kuhn, Alexandre Koyré was both a professional and historiographical model. Kuhn had much to say, in print and in the seminar room, about his hero. Emulation of Koyré accounts to a large degree for the goals of Kuhn's work in critical history of science, as well as for many of its strengths and weaknesses. Here we examine Koyré himself as a critical historian of science, rather than as a philosopher, and we do that from Kuhn's point of view, rather than as an exercise in modern Koyré exegesis.[7]

Koyré belonged to the second generation of a group of Continental neo-Kantian historians of philosophy, such as Ernst Cassirer and Léon Brunschvicg, who had turned their attention to the conceptual development of science. Neo-Kantianism in the historiography of philosophy had stressed the sympathetic understanding of the *sui generis* inner coherence and rationality of earlier systems of thought, the historical transformation and "progress" of certain concepts across such systems, and the ways in which categorical systems shape experience. Koyré held that the development of modern science depended upon a revolution in ideas, involving the establishment of a new metaphysics or set of deep conceptual presuppositions, which in turn shaped theorizing and experience in the emerging fields of modern science, especially classical mechanics and Copernican astronomy. By the mid-1950s, Koyré had become, in Anglo-American circles, the leading exponent of such "internalist" historiography of science, posed in opposition to Marxist or materialist "externalist" approaches.[8]

Galileo's revolutionary constitution of classical mechanics was Koyré's exemplary case of the emergence of modern science.[9] According to Koyré, Galileo had no need to apply some putatively universal and efficacious method, because research procedures and strategies always follow from within one's particular metaphysics. Belief in a universal, transferable, and workable scientific method is, according to Koyré, a myth.[10] Galileo succeeded in founding the first version of classical

[7] Kuhn gave early expression to his appreciation of Koyré's contribution to the maturation of the history of science profession in Kuhn (1977b, e). These essays are cited as reprinted in Kuhn (1977a). They were each originally produced in 1968. When Kuhn addressed each year's crop of new history of science graduate students, he would make a point of bringing in his well-worn, pre-World War II copy of Koyré's *Études galiléennes* [the English translation only appeared in 1978]. He would intone, "Nobody is leaving here until they have read all of this." Presumably he would know from one's work whether the exercise had been done.

[8] The best discussion of the "internalist vs. externalist" debate is Shapin (1992); see also Schuster (2000).

[9] For the emerging Anglophone profession of history of science following World War II, Koyré's treatment of Galileo (Koyré 1939, 1978), then only available in its original prewar French edition, became the exemplar of how to practice the history of science.

[10] Koyré (1956). As we shall see, this principle was followed and deepened by Kuhn, especially through his insistence that a large segment of any living scientific research tradition was passed onto apprentices and applied by them, in the form of tacit, craftsperson-like "knowledge."

mechanics because he worked within the correct sort of metaphysical framework, a kind of nonmystical "Platonism," a conviction that the basic furniture of the world consists in mathematical objects, moved according to simple mathematical laws. If Galileo experimented (a proposition Koyré often appeared to doubt, with the exception of thought experiments), the experiments were shaped by cognition and action themselves constrained by and constructed within this metaphysics.[11] For Koyré, the sort of Platonic metaphysics he attributed to Galileo was the only viable framework for scientific advance. Other frameworks might have virtues, but not scientific ones. Thus, Aristotelian natural philosophy, itself coherent as a categorical framework, could never structure experience and reasoning so as to produce modern mathematical physics. It was too closely enmeshed with the categories of natural language and everyday life (Koyré 1939, 1978).

Koyré's subordination of practice to metaphysics meant that he explained all his key facets of the Scientific Revolution—Galileo's mechanics, Copernican Revolution and Newtonian synthesis—on the same basis. There was no account of the dynamics of everyday practice within a scientific tradition nor of how traditions of scientific practice interact nor, especially for the internalist Koyré, an account of how traditions relate to contexts.[12] That, at least, is how the matter must appear to today's readers of Koyré. But, the situation was different when, following World War II, Koyré's influence was at its height. To understand this seemingly paradoxical point, one must turn to Koyré's detailed practice as a historian, rather than to his grand internalist historiographical pronouncements.

As a working historian of science, Koyré showed by example that scrupulous, critical explication of primary sources resides at the center of any serious understanding of the history of science. Koyré insisted that at a textual level, the conceptual structure, and faults, of a scientific thinker should be laid bare, setting aside anything that thinker might have said on a meta-level about "method" or program. One must avoid what Kuhn, following Koyré, always termed "preface history" and the simplistic "island hopping" among unproblematically linked sequences of correct scientific ideas, which, for Koyré and Kuhn alike, constituted the always to be avoided "Whig" history.[13] Close textual analysis, studying the primary texts "with all senses open", as Kuhn often said, not only meant eliciting the conceptual structures in play but also, tellingly, examining the ambiguities and errors of the author. These, Koyré stressed, showed as much or more about the inner texture of the actor's

[11] There is more to say about Koyré on Galileo's experimentation, which we defer to Sect. 19.5.

[12] Whether and how Koyré's historiography handles the contexts of his great revolutionary figures is a more complex issue than it may appear at first sight. See Barnes 1974, Chapter 4 for a brilliant and suggestive early post-Kuhnian discussion. For example, it has been noted, by Barnes and others, that Koyré's notion of the "metaphysical framework" embraces the intellectual and philosophical "contexts" of science. This has prompted the question of whether Koyré was an internalist or externalist. That question however is misplaced. It is just a matter of the location of what we may in post-Kuhnian terminology call the cognitive/social frontier (Schuster 2000, p. 335).

[13] Koyré and Kuhn had their own problem with a different species of Whiggism, which was built into their respective approaches and little noted until the emergence of post-Kuhnian discussions in the 1970s and 1980s, as we shall see later, Sect. 19.10 and Note 44.

thought than those smooth skeins of conceptualization with which we might still fully agree. As Kuhn recognized, Koyré was importing into history of science techniques of textual study and explication previously pioneered in the history of philosophy.[14] This was what had been missing in the amateurish historical writing of superannuated scientists and other enthusiastic hero-worshipping scholars. Such hermeneutical sophistication, combined with Koyré's internalist pronouncements, was judged to be the key to the training of the new generation of professional historians of science.

However, to return to our main point, a slippage occurred just here regarding the understanding of Koyré's work. As we have seen, Koyré lacked a model of scientific process that would bridge the gap between conceptual revolutions, between, say, Galileo and Newton. Nevertheless, it certainly "seemed" that Koyré had provided one. Koyré could supply a sequence of close, textual analyses of secondary figures, making it seem that the actual dynamics of scientific development had been uncovered. This kind of picture emerged in Koyré's most important sustained historical works, such as *The Astronomical Revolution* (1973) or *A Documentary History of the Problem of Fall* (1955). They presented accomplished, sympathetic readings of the theoretical structures of this, then that, scientist. All this occurred in the reader's mind under the sign of Koyré thundering historiographical pronouncements. It looked as though Koyré not only had adduced the blueprint for historiography, but also had described the dynamics of modern science, while providing a map of the great conceptual rupture points.

Writing about the debate at the historiographical level between internalists and externalists in the 1950s and 1960s, Steven Shapin remarked, with characteristic acuity, that on the internalist, mainly Koyréan side, we did not have a serious attempt to understand the living continuity and dynamics of scientific traditions. "A style of research and writing," he wrote, "does not amount to a theory of scientific change" (Shapin 1992, p. 346). That insight is as penetrating today as it was in 1992 when Shapin first said it. But to repeat, at the height of Koyré's acclaim, his dazzling style of textual explication was taken for a model of how sciences proceed. This is where the young Tom Kuhn, critical historian, entered. Kuhn, alone among his contemporaries, understood that there actually was no Koyréan theory of science dynamics and that one was needed. Moreover, as we are about to see, Kuhn's brilliance in this regard resided not so much in providing a more detailed general model of how revolutions unfold, than in offering a model of the dynamics of everyday, continuous, and in the end revolution-triggering scientific research: the model, that is, of "normal science" in any given scientific tradition. Kuhn saw that Koyréan textual analysis was one necessary thing and that an explicit model of what I am calling "the motor" of scientific dynamics was quite another.

[14] In his overview of how history of science had evolved, Kuhn indicated that reading Koyré and other early philosophically acute historians such as Emile Meyerson and Léon Brunschvicg crystallized his ability to sympathize with outmoded structures of thought, such as Aristotle's. They showed him that past philosophical and scientific systems have their own *sui generis* rationality, coherence, and cogency which the historian must penetrate (Kuhn 1977b, p. 11; 1977e, p. 108).

19.4 Kuhn's Core Premises as Historian: Post-Koyréan and Neo-Koyréan

Three premises stand out in the historical theorizing of the young Kuhn. They are properly historiographical rather than epistemological or philosophical. The first premise is that there is no such thing as Science (capital S). You cannot say "Science began with the Greeks" or "Modern science started in the 17th century." Kuhn is interested in the *histories of the sciences*. He views Science (capital S) as an invention of poor historical thought, preface history if you will.[15] The second premise is that there is no universal, efficacious scientific method. In this, he followed and articulated Koyré's position: Kuhn reinforced Koyré's view by explicitly and consistently holding that the sciences are many and that each science, in any given normal period, has its own disciplinary research culture. This in itself spoke strongly against the myth of a universal method. Kuhn, however, famously reinforced this claim by insisting that a large portion of any such normal research culture consists in tacit knowledge of how to apply the paradigm to certain species of problem and of the criteria for selecting problems and evaluating their solutions.[16] The third premise is that even though the sciences are many, there is a common pattern of development and change displayed in the history of each science. This is the pattern of normal science—problem- or puzzle-solving or "mopping up"—under the aegis of a ruling paradigm, until such time as serious anomalies are recognized in the application of that paradigm, crisis ensues, alternatives are proposed, and eventually an incommensurable new paradigm is installed to govern normal research until the next crisis and rupture.[17]

Leaving aside the difficulties—historical and philosophical—of Kuhn's model of revolution, we should note that Kuhn's model of the common, recurring pattern of

[15] Anyone who has taught entry-level history of science knows import of this premise as well as students' proclivity to slip, even when doing history, into talk about Science, capital S. However, if properly introduced, it transforms their reading of historical and philosophical literatures that miss this point. See Schuster 1995a, Chapter 15, p. 155; 2013b, pp. 284–285.

[16] Kuhn's views on the tacit component of paradigms are usually linked to those of Polanyi (1958), whom he cites in this connection early in *SSR* (44 n. 1). Recently doubt has been cast upon the reliability of Kuhn's recollections about the timing and import of his reading of Polanyi (Jacobs 2009). The earliest and most impressive post-Kuhnian articulation of the theme of scientists' activity as "craftsmen's work" was in Ravetz (Ravetz 1971), who interestingly mainly cites Polanyi in this connection rather than Kuhn (see Ravetz's index entries on Kuhn and Polanyi). It is also important to note that while the Kuhn/Koyré anti-method position is extremely important for the historiography of science, it did not go far enough. That is, they left the issue of method as one of ironic denial. They did not ask why scientists regularly profess to believe in a general scientific method, what political and rhetorical roles such belief plays, and how "method talk" functions as a misleading species of discourse. See Schuster 1986, 2013a, pp. 70–77, pp. 265–273.

[17] For an explication of the phases in Kuhn's model of revolution, its onset, process, and resolution, see further Schuster 1995a, Chapter 16, pp. 161–165. This exposition is aimed to help entry level students of history or sociology of science, not particularly to facilitate participation in the philosophical debate about Kuhn on revolution. Note also that Kuhn's conception of the "first paradigm," founding a new disciplinary tradition, poses its own problems, and we shall deal with this in Sect. 19.10.

change in any given mature scientific tradition accomplishes four key articulations in relation to Koyré. Firstly, it provides a blow-by-blow account of what presumably goes on inside a Koyréan revolution. Koyré had not dissected revolutions. Kuhn now did so. Secondly, and this particularly interests us here, it provides a post-Koyréan model of the dynamics of ordinary, day-to-day, garden-variety scientific practice that eventually conduces to an internally generated crisis and finally overthrow of the very basis of that ordinary practice. Koyré had only wanted to deal with revolutionary genius. As discussed above, if we avoid mistaking textual analysis for a theory of scientific change, we see that Koyré had nothing to say about what goes between moments of grand rupture. In a way completely unknown to Koyréan historiography, Kuhn's model of normal science points us to the continuity and dynamics of any given scientific tradition, between (now explicated) Koyréan ruptural moments. Thirdly, Kuhn achieved some serious mapping of the macro history of science: just as Kuhn was providing, de novo versus Koyré, a model of the dynamics of normal research, so too he was offering a detailed if preliminary map of the history of the sciences, especially the physical sciences, where Koyré had offered instances of revolution. Once you have decided that the history of science is the history of multiple scientific traditions/disciplines and once you have also provided a generic pattern of change applying to each tradition over time, you have implied a map of the history of the sciences. The younger Kuhn firmly grasped these points and, as we discuss below in Sects.19.7, 19.8, 19.9, 19.10 and 19.11, pursued them in detail.

Finally, Kuhn introduced the possibility of metaphysical pluralism. No longer is there only one metaphysical stance—Platonism of the Koyréan genre—that can stand behind genuine "science." *SSR* teaches that every single paradigm in the history of the various sciences has possessed a particular metaphysics, which need not have been of Platonic type. Thus, Aristotelianism recovers its status as a possible metaphysical core for arguably genuine science.[18] However, the necessary presence of a metaphysical dimension in every scientific tradition, during each and every of its normal science periods, remains a Koyréan element. Similarly, as we shall see below, Kuhn's paradigms retain a destiny of fulfillment, due to the presumed cognitive pressure of their respective metaphysical dimensions—a Koyréan legacy in Kuhn's thought.

19.5 The Historical Problem of Experiment and Experimental Hardware

Koyré's idealist and metaphysically driven vision of knowledge meant he minimized the role of experiment and experimental hardware. This stance comported with his anti-Marxist agenda in historiography of science. Marxist historians of early modern science stressed the causative role of technics, craftsmen, and

[18] This is apparent in the status accorded to Aristotelian-backed Ptolemaic astronomy as a competitive paradigm. For a textbook treatment of how Kuhn released the constraints on what might be construed as the metaphysics of a given paradigm, see Schuster 1995a, Chapter 11, Chapter15, p. 157 and Schuster 2013b, pp. 193–208, pp. 290–291.

instrumental intervention against the background of rising commercial capitalism, state formation, and military competition. Koyré's contrasting account denied such considerations. Conceding experiment a subsidiary, uncreative role in confirming the results of predictions deduced from mathematicized theory, he also granted importance to Galileo's thought experiments, as a way of detecting contradictions in Aristotelian and Scholastic physics.[19]

Kuhn had been trained as a professional physicist. He also sympathized with Robert Merton's attempt to demonstrate—with a non-Marxist emphasis—the importance of technics and men of practice in the rise of experimental, Baconian science in seventeenth-century England (Merton 1970). Kuhn broadened the question, asking how and why new experimental sciences emerged in the eighteenth century. For Kuhn to entertain the importance of experiment and experimental sciences was a large step away from Koyré's program. To that end, Kuhn offered several fruitful initiatives about the role and nature of experiment. Because we shall later focus on the rise of new experimental sciences, as part of our study of Kuhn's larger mapping and motor modeling project, we briefly mention these issues here and recur to some of them later.

[1] Toward a Politics of Testing: In his brilliant paper, "The Function of Measurement in Modern Physical Science," originally published in *Isis* in 1961, Kuhn took the notion of confirmatory experiment and historicized it. Deploying his emerging concept of normal science, he focused on scientists' judgments about the significance of small discrepancies of "fit" between prediction and test results. Training into the ruling theory transfers to apprentice practitioners a sense of the standard of accuracy expected in their particular field. Research problems exist where there are challenges to bring "fit" within acceptable limits and to extend the adequately accurate predictions to new domains of test data. In extreme cases, the value placed on increasing precision might justify a change of ruling theory. In Kuhn's account, experiment undertaken at the research front takes on a new cast: not as providing simple confirmation or refutation of ruling theory, but rather as an activity where the goodness and meaning of results can be expertly debated. This was a far cry from Koyré, who expected a good theory, like Galileo's, to be confirmed (or not even submitted to test).[20] It did not take much imagination to see that in the search for improved "fit," a research community could renegotiate the ruling theory and hence its predictions and/or that the understanding of experimental hardware

[19] "Galilean epistemology […] is both *a priorist* and experimentalist at one and the same time (one could even say that it is the latter because it is the former) [….] [Galileo's] experiments…are designed on a theoretical basis and of which the function is to confirm or refute the application to reality of laws deduced from principles which themselves have a quite different basis" (Koyré 1978, p. 106).

[20] Kuhn 1977h, Schuster 1979, pp. 305–306. Koyré of course knew that "strict agreement" between experimental results and mathematically mediated theoretical predictions is "strictly impossible" (Koyré , 1978, p. 107). Rather, Galileo, who according to Koyré knew this as well, "[…] was not at all looking to found his theory [of motion] on facts gained in the realm of experience: he knew perfectly well that this is impossible. […]. Experiment can confirm that [a theoretical assumption] is a good assumption. It can do this within its limited means; or rather, within the limits of our means" (ibidem).

could be consensually modified to alter the resulting data toward better "fit." Within a decade and a half the micro-sociological dimensions of such activities became a topic of early SSK research.[21]

[2] Theory Loading of Instruments: Kuhn, perhaps taking on board his reading of Popper (1959) or Hanson (1958), was adamant that experimental hardware and procedures were "loaded" by the theoretical commitments of the ruling paradigm. This was a good start, but, of course, it has been shown by later historical and sociological research to be too simple: instruments are internally complex and may bear, especially in black-boxed fashion, the loading of older theories which are no longer in play. Theory-loaded instrumental practices from other realms of science may be used, relatively unproblematically, as tools of exploratory experimental work in another field.

[3] Thought Experiment as Creative: Kuhn, like Koyré, emphasized cases of thought experiment, but he worked out a more sophisticated account, by considering some of the psychological and cognitive issues involved. Kuhn argued that thought experiments do not merely reveal inconsistencies in previously held conceptual schemes. They must be understood to concern the relation of concepts to nature. They can "alter one's knowledge of the world." Accordingly, in these cases, the results are not a catastrophic "Gestalt shifts" but rather conceptual "reforms" or "modifications."[22] The processes of conceptual change developed in this paper do not fit the more extreme formulations in *SSR* about ruptural conceptual change. Kuhn virtually admits that he missed the significance of these ideas, conceding that this paper had "little influence" on *SSR* (Kuhn 1977a, Preface, p. xx).[23]

As with Kuhn's potential politicization of testing [1], this articulation opened the possibility of seeing normal, work-a-day research as having feedback consequences for small but significant modification of the ruling paradigm. However, this potentially fruitful gambit was swallowed up by his official *SSR* model of problem-solving normal science within a more or less frozen paradigmatic frame.[24]

[21] In effect, Kuhn started from Koyré's skepticism about the possibility of strict agreement of data with prediction and opened a realm of social and historical inquiry into exactly what the level of expectation about theory/data "gap" was in a given discipline at a given moment. Post-Kuhnians then focused on the continual possibility of renegotiating that level in the course of consequential work (i.e., creative normal science, possibly leading to paradigm-modifying "discoveries"). Thus, in SSK, the two strands of Kuhnian insight—which Kuhn had relegated to the margin of his thought—about discovery [which we discuss in the next section] and about reasonable agreement in experiment were woven into a broader vision of studying the micro-politics of testing and negotiation of the significance of claimed "results." The works of Collins (Collins 1975) and Pinch (Pinch 1985) are canonical in this regard. See Schuster 1995b, Chapter 6, for a textbook treatment of the politics of testing, devised in the wake of Collins' and Pinch's work.

[22] Kuhn 1977i, originally published in 1964, especially, p. 251.

[23] Again, these ideas of significant conceptual change in the course of normal science accord with Kuhn's ideas about experimental testing (Kuhn 1977h) and his pre-*SSR* view of significant discovery within normal research (Kuhn 1977g) to be discussed below. Taken together, these three themes foreshadow more the subsequent development of SSK than the contents of *SSR*.

[24] There was one other area where the younger Kuhn innovated about experiment, beyond Koyré: following Popper, Kuhn saw that test results were often offered as "proof" of the falsity of one paradigm and truth of its competitor. But Kuhn had looked sufficiently closely at experiment as an

It is worth noting, finally, that although Kuhn vastly extended the empire of experiment within a basically Koyréan historiography, he failed to cash out the more radical implications of his approach for developing a micro-politics of experiment and testing in science. This was accomplished in the brilliant, and often misunderstood, efflorescence of early, or what I term "classical," post-Kuhnian sociology of scientific knowledge. Two things need to be noted about these developments. Firstly, once the early SSK writings were assimilated, they tended to occlude the significance of Kuhn's early moves about experiment. Secondly, as we shall see in the next section, Kuhn's official statement of his motor model, his account of normal science in *SSR*, more or less required that the potential growth points of a politics or sociology of experiment were going to be marginalized.

19.6 Modeling the Motor of Traditions: Normal Science—Importance and Limits

19.6.1 Creative and Political Normal Science or Trivial and Dogmatic Normal Science

Here we are going to examine Kuhn's motor of scientific research, his model of normal science, its strengths versus Koyré, and its own possibilities and implications, many of which Kuhn missed because of his Koyréan bearings. An emphasis on living scientific traditions and their dynamics, the continuous process leading to revolutions, had been missing in Koyré, but it was exactly what Kuhn knew about as a practicing physicist and via close study of the history of physics.[25] Kuhn's original intention was somehow to model, in terms of what he dubbed "paradigms," the structure and dynamics of given scientific fields or traditions—how the members went on with work at the coalface and with what resources, to what standards, toward what aims, and under which constraints. The traditional rather abstract and timeless philosopher's questions about the structure of theories, methods, progress, etc. would be deferred in the light of this enterprise, focused on the how's and why's of the ever-moving, working coalfaces of the individual, mature, scientific traditions or fields.

Note, however, that what I have just said might seem a bit overblown—despite Kuhn's clear improvement over Koyré—when we recall that in *SSR* normal science

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expert enterprise and at cases of theory debate, to conclude that "crucial" tests did not in themselves determine the outcome of theory contestations, the Popperian position. Kuhn held instead that "crucial tests" were offered as evidence, among other evidence and argument, by one side in its contest for support against its opponents. Crucial tests were a phenomenon within a wider cognitive and social process of heightened theory debates, not the determinative end points of them.

[25] Kuhn (1977a, Preface pp. 17–18) tells us that his paper on "Function for Measurement" (Kuhn 1977h) was largely finished by spring of 1958. Kuhn says it comes very close to describing what became "normal science," adding, "Though I had recognized for some years that periods governed by one or another traditional mode of practice must necessarily intervene between revolutions, the special nature of that tradition–bound practice had in large part previously escaped me."

is rather uncreative. The "mopping up" or "puzzle-solving" that characterizes normal science is straightjacketed by the ruling paradigm, until and unless some anomaly or anomalies happen to trigger a crisis and the remainder of the flight path of revolution.[26] Clearly there is a tension between the letter of *SSR* on normal science and its "almost" obvious wider possibilities as a sociopolitical model of some degree of creativity and change within a ruling paradigm. This reflects on the nature of the Kuhn/post-Kuhnian dialectic. We can appreciate what Kuhn did and did not achieve concerning normal science by looking at an early post-Kuhnian insight about Kuhn which emphasized the—at the time—paradoxical point that the central achievement of Kuhn was to conceptualize normal science, rather than to produce his highly contentious and disputed account of revolution.

The best and earliest post-Kuhnian exponent of this point of view was Barry Barnes, then a rising star in the Edinburgh Science Studies Unit. Barnes took seriously Kuhn's style of critical history and attempted to integrate it with SSK. Barnes grasped that Kuhn was concerned with understanding traditions of scientific practice, so he stressed instances of Kuhn's historical work rather than his grand modeling in *SSR*. Therefore, at the beginning of his seminal (especially for historians!) Barnes (1982), he started from the rules of exegesis implicit in Kuhn's own historical practice as displayed in his numerous specialist studies: (1) the historian must place a scientist in his subculture or tradition; (2) the historian must treat utterances charitably as internally coherent, where coherence is judged in terms of the actor's relevant contexts and culture; and (3) the only causes asserted of utterances and actions must arguably have been present and active in the contexts in question. Barnes then fleshed out the nature of a tradition of specialized scientific practice in a post-Kuhnian way, viewing *normal practice within a tradition* as a process of social negotiation of conceptual change, possibly small-scale conceptual change.[27]

[26] For an account of what Kuhnian normal science involves, designed for a first or second year introductory course on HPS, see Schuster 1995a, Chapter 15, pp. 157–158 and Schuster 2013b, pp. 291–292.

[27] "The continuation of a form of culture implies mechanisms of socialization and knowledge transmission, procedures for displaying the range of accepted meanings and representations, methods of ratifying acceptable innovations and giving them the stamp of legitimacy. All of these must be kept operative by members of the culture themselves [...]. When there is a continuing form of culture there must be sources of cognitive authority and control. Kuhn was initially almost alone among historians in giving serious attention to these features of science. The result of this attention […] is to display just how profound and pervasive is the significance of the sub-culture in science, and the communal activity of the organized groups of practitioners who sustain it. The culture is far more than the setting for scientific research; it is the research itself [...]. Science is not a set of universal standards... Scientific standards themselves are part of a specific form of culture" (Barnes 1982, pp. 9–10; see also Barnes 1972). The English sociologist M. D. King (1971) had incisively made related points around the same time: addressing specifically Kuhn's paper on the "*Historical Structure of Scientific Discovery*" (Kuhn 1977g) which we are about to discuss, King observed that the paper issued a revolutionary challenge to positivist philosophy of science and to orthodox Mertonian sociology of science. It threatened the latter by hinting that the object of study in the sociology of science is not the cluster of transtheoretical Mertonian social norms of science, but rather the institutional, social, and political processes by which explanatory frames are produced, maintained, and altered by significant (but not catastrophic) discovery.

Barnes helped shape the emergent SSK consensus that Kuhn's stark differentiation between normal and revolutionary phases in the history of a tradition of scientific practice was too strong.[28] This view considers "normal research" paradigms as constantly subject to partial renegotiation and modification. If a problem can be solved only by advocating a shift in some aspect of the paradigm, however so slight, then one can say that the problem solution involves feedback alterations to the paradigm. Such alterations are carried over into the next rounds of problem-solving where further alterations may be suggested. Of course, such bids to alter the paradigm slightly must be accepted by the relevant community. We may term a noticeable alteration of the paradigm, which has been negotiated into place, as a "discovery." Moreover, given this new suggestion that normal science lives by producing significant discoveries, then "revolutionary science" need not be so wild as Kuhn thought, and indeed it need not exist at all. Perhaps a revolution is just a case of relatively large modification of a paradigm in which the innovators legitimate their actions with a "rhetoric" of revolutionary overthrow of the bad old theory.

With these sorts of insights, one was in new territory. The focus shifted onto the micro-politics and organizational dynamics of mature scientific fields, a step that further discomfited anti-Kuhn scholars not used to thinking this way about how science works. But clearly, if one wished to be a post-Kuhnian historian, there was now no alternative. Critically minded historians of science of my generation grasped this eagerly and passed it on, at least to their first generation of students. By this point, the debate, at least among those interested in being historians rather than philosophers, had suddenly and gratifyingly slipped into a post-Kuhnian key. The strict theory of *SSR* was well left behind, and indeed so was Kuhn, who for many years showed little sympathy for these new "sociologists," even though they were arguably legitimate heirs of Kuhnianism. The capping irony is that the younger Kuhn, when practicing as a critical historian, had hit upon elements of the later post-Kuhnian view of normal science, only to overwrite them with the grand pronouncements of *SSR*.

19.6.2 The Young Kuhn on Creative Normal Science and the Process of Significant Discovery

In late 1961, on the eve of publication of *SSR*, Kuhn finished his dazzling paper on the "Historical structure of scientific discovery." The core ideas in it predate his famous paper on energy conservation of 1957 (Kuhn 1977a, Preface, pp. xvi–xvii)—so the paper reflects very early theoretical ideas prior to *SSR*.[29]

[28] This view appeared early in the SSK tradition: Jerry Ravetz (1971) articulated Kuhn at length in this way; I later suggested such an SSK articulation of Kuhn (Schuster 1979). And, of course, this is what Barnes (1982) did so brilliantly. Barnes also devoted a chapter to exposition of this emerging post-Kuhnian conception of discovery, which per force entailed erosion of the stark normal versus revolutionary science dichotomy.

[29] The paper in question (Kuhn 1977g) is the first in the "meta-history" part of *The Essential Tension*.

What interested Kuhn was the idea that "significant discoveries" are not simple "events" in which a new fact or law is slotted cumulatively into a growing edifice of scientific knowledge. Significant discoveries arise from complex historical processes, and they ecologically alter the structure of knowledge through which they are produced, rather than simply adding to it. First, a striking difficulty must stand out against a well-developed framework of theory, theory-guided technique, and expectation. "Anomalies," Kuhn was already writing:

> […] do not emerge from the normal course of scientific research until both instruments and concepts have developed sufficiently to make their emergence likely and to make the anomaly which results recognizable as a violation of expectation. (Kuhn 1977g, pp. 173–174)

There follows a struggle—or negotiation as we would say—both theoretical and experimental, to render the anomaly lawlike and to fit it into the accepted categories of explanation. This work issues in what we might term a "feedback" effect upon the web of techniques and concepts against which the anomaly arose (Kuhn 1977g, p. 175ff). This is the process and the product known colloquially as "discovery." The upshot can be part of an "upheaval" of established "theory and practice," such as the discovery of oxygen; or it can be subtle, like the effect of the discovery of Uranus upon the expectation that similar patterns of anomaly would henceforth best be handled by postulating additional planets.

Kuhn here saw significant discovery as a theory-bound and theory-altering process involving subtle or not so subtle ecological renegotiations of the preceding frame of concept and practice. He came very close to what was later taken as the dynamic, and creative, nature of normal science, characteristic of post-Kuhnian thinking. What Kuhn's vision lacked was the later sense of the competitive character of the normal scientific goings-on, and, *a fortiori*, the insight that what one needs to study are the organizational dynamics and micro-politics of the expert field. Kuhn never realized the potential of the notion of significant discovery—and it was precisely his work on revolutionary change, growing from normal science as puzzle-solving and "mopping up," that occluded and marginalized it.[30] Significant discovery cuts across the black and white categorization of normal and revolution-

[30] We see this happening in *SSR*, especially Chapter 6 'Anomaly and the Emergence of Scientific Discoveries." Here an elaborated version of Kuhn's (1977g) account of discovery is offered [mainly] as a moment in the account of anomaly and the emergence of new paradigms. He offers the oxygen/phlogiston case, as in Kuhn (1977g), but now of course it subserves a model of revolution; he offers the case of the Leyden jar, which subserves an account of emergence of a "first paradigm" in electrical science [cf. Sects. 19.10 and 19.11], and he offers the case of Roentgen and x-rays, an example where paradigm change was not immediately in the offing. There is a vestige of the original sense of discovery at Kuhn (1970, pp. 52–53): "[The process of discovery] then continues with a more or less extended exploration of the area of anomaly. And it closes only when the paradigm theory has been adjusted so that the anomalous has become the expected." Readers concentrating on the stock Kuhnian theory of revolution may well read right through that passage, as I did many times, before I reread it in the light of Kuhn (1977g) and the emergence of the SSK approach to discovery. It is then clear that Kuhn is slipping back toward a view of potentially creative normal science. But the opportunity is lost as the chapter and book flow on with the model of revolution (the next chapter is titled "Crisis and the Emergence of Scientific Theories" and the one after that "The Response to Crisis").

ary science. Kuhn never seriously thought through the idea that scientific actors are skilled interpreters, negotiators, and indeed hermeneuts, always involved in the competitive making and breaking of paradigm altering "significant discoveries."[31] In short, Kuhn had his embryonic conception of significant discovery before he had worked out most of *SSR*. But *SSR*—very much influenced by and reacting to Koyré's historiography of ruptures played out under determining metaphysical umbrellas—marginalized significant discovery in favor of the interplay of excessively routine normal science and excessively ruptural revolutionary science.[32]

19.7 Kuhn's Mapping: The Scientific Revolution, Classical and Baconian Sciences

We have surveyed Kuhn's relation to Koyré on the issue of the "motor" of practice in the sciences and reflected on what happened to Kuhn's mechanics of normal science in the hands of early SSK, observing the irony that Kuhn, had he been less Koyréan, might have come closer to the post-Kuhnian position.[33] We now turn to the mapping side Kuhn's early critical history project. To the extent that Koyré left a map at all, it was rather unexplicated, concentrated on the Scientific Revolution, and consisted in locating three temporally splayed sub-revolutions: the Copernican in astronomy, the Galilean in classical mechanics, and the final Newtonian synthesis and transformation. These played out against the background of a "nonscientific" Scholastic Aristotelianism. It was a map of high points, with little attention paid to the interrelations, temporal and cognitive, among the three towering moments, offering no continuous narrative of change and transformation.

In contrast, Kuhn stepped in with a detailed and challenging map of the period and beyond. Kuhn's map resembled in detail none of the other contemporary sketches of "the Scientific Revolution," in part because it was a map meant to deploy and further articulate his model of tradition dynamics. Thus, it amounted to a brilliant correction of Koyré. However, it also harbored serious problems: it was hindered by lack of mobilization of a notion of significant discovery and creative normal science, by failure to understand what natural philosophy was, and by ambiguities and hesitations about the origins of new disciplines and the nature of initial

[31] Kuhn lacked the interest in micro-sociology or to be precise Schutzian phenomenological sociology, to go further with this. Many of the early SSK scholars drew upon Schutz in particular (Schutz 1970; Schutz and Luckmann 1973). Kuhn always seemed devoted to Parsonian/Mertonian structural/functional, large-scale normative and consensual views of groups and institutions.

[32] Moreover, late in his career, after years of rejecting the work of SSK, Kuhn wrote a piece that again seems to return to that earlier point of accepting what had been his own proto-SSK position. He reflects on the intervening development, but then leaves his position up in the air, not cashing out the conclusions, such as others had already done by the early 1980s! (Kuhn 2000).

[33] This is not to say that SSK devotees were broadly committed to developing general historical models of motors and applying them to large mappings and narratives in the history of scientific practice. Barnes and Shapin were, but many were not.

paradigm-founding achievements. As several of Kuhn's essays show, he saw the Scientific Revolution as having advanced on two loosely connected fronts (Kuhn 1977b, f, pp. 136–137; 1977h, pp. 213–221).

1. Radical change in the preexisting "classical sciences," such as the mathematically oriented ones of geometrical astronomy, optics, mathematics, and the study of motion, as well as in the "biomedical" ones of medical theory/physiology and anatomy
2. The initial development of some experimentally oriented "Baconian sciences," the fields that, in *SSR*, mature to their first paradigms at various stages in the eighteenth century: electricity and magnetism, optics (from Newton), heat theory, and chemistry

We shall examine two disciplinary case studies in this mapping: in Sect. 19.8, the Copernican Revolution from (1) and, in Sect. 19.10, case of the birth of electrical science from (2).

19.8 Mapping Test Case I: The Copernican Revolution

Let's consider a potential mapping in the area of Kuhn's classical mathematical sciences—the case of the Copernican Revolution. I am not talking about the pre-*SSR* story in Kuhn (1957). We are dealing strictly with the mapping inherent in *SSR*, a revolutionary change: the Copernican system defeated and displaced the Ptolemaic system. If we take *SSR* seriously, we must envision a showdown between two set-piece, finished and "incommensurable" paradigms in competition, backed by their respective teams.[34] This conforms to popular and even professional readings of Kuhn, but it is not a picture of Copernicanism supported by the slightest reflection about the debates among the key players. Nor is it a view that comports with the findings of SSK about the concept of significant, negotiated discoveries within a continuous tradition of practice. The following four paragraphs sketch the way I approach a correction of Kuhn in undergraduate teaching, so that Kuhn can be better understood, even as students can move beyond Kuhn in terms of SSK understandings and historiographical sophistication.[35]

One begins by realizing that there never was a single, agreed Copernican system—not one to be fleshed out and not one to be worked toward. What we have is a

[34] Here I take "incommensurability" to mean "*there is no single, agreed, and overriding criterion of goodness accepted by both sides*," rather than "*there are no criteria at all available to the two sides*," thereby defusing philosophical anxieties and allowing us to get on with historiographical discussion. On how to interpret "incommensurability" in Kuhnian theory without falling into complete irrationality, see Schuster 1995a, Chapter 16, pp. 164–165; 2013b, pp. 304–305. On the "competing teams" conceit and a figural representation thereof, see Schuster 1995a, Chapter 25, pp. 236–237, and figure 4, p. 240; Schuster 2013b, pp. 482–483.

[35] Schuster 1995a, Chapter 25, p. 237–238, and figures 5–7, pp. 240–241; Schuster 2013b, pp. 484–486, and figures 26.5–26.7.

ramifying series of variants—conflicting and in part contradictory variants. Copernicus' Copernicanism is not the Copernicanism of any other Copernican and so on—down to Newton. Even within the group that strict *SSR* theory would account a Copernican paradigm "team," what we see are competing bids for individual hegemony. Each player fashioned a significantly different claim at the coalface of astronomical debate, depending upon his judgment of the state of play; his personal cognitive, technical and material resources, skills and investments, as well as his judgment of how best to defeat, co-opt or marginalize competing versions. Kepler and Galileo did not simply accept some straitjacket offered by Copernicus in 1543.[36] Galileo did not even practice mathematical astronomy; and his version of Copernicanism is not a repeat of Copernicus' theory. Nor did Kepler march shoulder to shoulder with Galileo as a straitjacketed follower of Copernicus. His version of Copernicus did not parallel Galileo's version — in fact their versions were in competition. These men were creative players and negotiators, battling to get the best advantage for themselves, and so they had consequentially different versions of "Copernicanism."

For a while—1590 to 1630—the most successful move proved to have been played by Tycho Brahe. He was not a pure Ptolemaist, and he was not a Copernican at the level of self-accounting. Rather, he was a clever professional negotiator, saying in effect that he wanted to preserve as much as possible of the basic Aristotelian system while accommodating, indeed co-opting, the best parts of the Copernican bid —especially Copernicus' claimed finding of several "harmonies of cosmic structure" that are used to support the physical truth of the system. The Tychonic system is in a way both geocentric and heliocentric and houses Copernicus' harmonies of cosmic structure with equal mathematical ease. Tycho did not thereby produce a timid or unimaginative compromise, but rather a brilliantly constructed gambit. One can view him as the most radical Ptolemaist of the day or the most conservative Copernican. He himself, of course, preferred the idea that his was a significantly different, third, and true theory. But that does not mean Tycho had a third, straightjacketed paradigm, perhaps missed in *SSR*! Tycho's bid was within the same field of discourse as Copernicus', and we can further tie his claims back to that evolving field. After all, Kepler would go out on a limb for elliptical planetary orbits, because he preferred to stay within the improved error limits established by Tycho's data. Kepler and Tycho were maneuvering in a common, evolving field.[37] And the same point applies to the "revolutionary" Copernicus: just as Tycho was in part a Copernican astronomer, so Copernicus was almost entirely a Ptolemaic astronomer. Almost everything that Copernicus did was in that tradition, except for the uncovering of the cosmic harmonies. Copernicus can be seen as a radical version of a Ptolemaic astronomer.[38]

So, there were no opposing paradigms with backing teams—only a historical process of negotiation, revision, and alteration in one tradition and field of competition.

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[36] Schuster 1995a, Chapter 25, p. 237, and figure 5, p. 240; Schuster 2013b, p. 484 (figure 26.5).

[37] Schuster 1995a, Chapter 25, pp. 237–238, figure 6, p. 240; Schuster 2013b, p. 485 (figure 26.6).

[38] Schuster 1995a, Chapter 25, p. 238, figure 7, p. 241; Schuster 2013b, p. 486 (figures 26.7).

The only important thing to understand is the process of bidding, counter-bidding, and negotiating in attempts to establish the longevity of one's own claims. There was no emergent essence "Copernicanism," no final goal given in advance—the entire history displays a dynamic process within one complexly evolving tradition.

Thus, one can achieve a revision of the Koyréan Kuhn by going more deeply into the issue of the nature of a tradition and its dynamics and by employing the "lost" Kuhnian concept of "significant discovery." The remaining issue is whether so far we have conceptualized the tradition of astronomical practice in historically acceptable terms, thus paving the way for an improved mapping. And the answer is "not yet." There is still more to be said, which again bears directly on Kuhn's model, his mapping, and his deeply seated Koyréan orientations: this is the problem of the historical category of natural philosophy.

19.9 Forming the Category of Natural Philosophy, Modeling Its Dynamics, and Re-mapping the Copernican Revolution

Kuhn, like Koyré before him, failed to recognize that in order to understand the Scientific Revolution, one must take serious historical cognizance of the then competing varieties of natural philosophizing. He endemically ignored the role in the history of the sciences of the great families of natural philosophies—of mechanism, neo-Platonism, and Aristotelianism, within which families' particular variants flourished and competed. Post-Kuhnians have come to realize that conflict among instances of such systems defined the rhythm and moments in the Scientific Revolution.[39] But, according to Kuhn, such philosophies of nature variously provided only the "metaphysical" elements of the sciences—"new intellectual ingredients," as he calls them, of the now being revolutionized classical mathematical sciences and of the about to emerge Baconian experimental sciences (Kuhn 1977c, p. 53). Kuhn occluded the historical problem of natural philosophy and its varieties within his own neo-Koyréan conception of multiple and various metaphysical umbrellas for the various existing and nascent sciences. He thereby missed a major element in any mapping of the history of the sciences in the period, including the Copernican Revolution.

[39] I first emphasized this point in Schuster (1990) and Schuster and Watchirs (1990). Much earlier Robert Lenoble (Lenoble 1943) put the conflict of varieties of natural philosophy onto the map of the Scientific Revolution. Kuhn owned a first edition of this work—as I learned when I borrowed his copy, since it was not in Princeton's Firestone Library. Lenoble was followed in the 1960s by P. M Rattansi (Rattansi 1964). Later Easlea (Easlea 1980) and Ravetz (Ravetz 1975) tried to popularize this view. My early work followed from these initiatives and from discussions with my then Cambridge colleague Andrew Cunningham, circa 1978–1979. Later attempts to delineate the category of natural philosophy include Andrew Cunningham (Cunningham 1988, 1991), Cunningham and Williams (Cunningham and Williams 1993), Peter Dear (Dear 2001), and Peter Harrison (Harrison 2000, 2002).

In recent work, I have argued that one must go beyond even the conception of jostling instances of the general types of natural philosophizing. One must look at natural philosophizing as an institution and cultural enterprise (Schuster 2013a, pp. 10–13, pp. 37–70, pp. 77–88, 2013c, pp. 21–28). That is, one must appreciate that natural philosophy was not just the Scholastic Aristotelianism of the universities, so that the eclipse of Aristotelianism did not mark the death of natural philosophy per se. It was an entire subculture and field of contestation. When one "natural philosophized," one tried systematically to explain the nature of matter, the cosmological structuring of that matter, the principles of causation, and the techniques for acquiring or justifying such natural knowledge. All natural philosophers and natural philosophies constituted one subculture in dynamic process over time. Early modern natural philosophers learned the rules of natural philosophizing at university while studying hegemonic neo-Scholastic Aristotelianism. Even alternative systems—neo-Platonic, chemical, mechanistic, and later Newtonian—followed the rules of this game. The Scientific Revolution, in its most turbulent phase, in the early and mid-seventeenth century, was a set of transformations, a civil war, inside the seething, contested culture of natural philosophizing. That culture then continued to evolve under internal contestation, and external drivers, and eventually fragmented into more modern-looking, science-like disciplines and domain**s**, plural, over a period of 150 years from 1650 (Schuster and Taylor 1997).

Along with the post-Kuhnian study of early modern natural philosophy has come attention to those disciplines then thought to be subordinate to it, such as the traditional "mixed mathematical sciences" of hydrostatics, statics, geometrical optics, geometrical astronomy, and harmonics. These of course are precisely Kuhn's classical mathematical sciences. If we are to understand Kuhn's claimed revolutions in these domains, including the Copernican Revolution, we need to grasp the relations between the dynamics of these fields and that of the supervening field of natural philosophy. Strict Aristotelians insisted that the mixed mathematical sciences were of instrumental value only: they could not touch upon questions of matter and cause, natural philosophical categories. But, this "rule" was increasingly bent and questioned, and nowhere more importantly than in the debate over realist Copernicanism.

In my model of the game of natural philosophizing, I use the term "articulation" to denote moves made by a natural philosopher to bring at least the higher theoretical levels of a subordinate science, such as geometrical optics or astronomy, into coordination with the matter, cause, and cosmic structure elements of his own natural philosophy (Schuster 2013a, pp. 42–43, pp. 51–55, 2013c, pp. 21–23). In the case of geometrical astronomy, virtually all natural philosophical contenders tried to articulate a preferred version of astronomy—Ptolemaic, Tychonic, or Copernican—to their preferred natural philosophy.[40] Such an articulation amounted

[40] It is important to note that such articulation existed in the relation even of Aristotelianism to Ptolemaic astronomy, regardless of what the strictest Aristotelians might have said. The fine details and elaborate geometrical tools of Ptolemaic astronomy fell outside any plausible realistic interpretation, offered merely appearance-saving geometrical models, and could not provide natural philosophical explanations in terms of matter and cause. However, the fundamental concepts of

to specifying the cosmological element of the natural philosophy in question. But of course, only part of the selected version of astronomical theory was "articulated upon," since no natural philosophy ever presumed to explain the full scope of astronomical practice and the elaborate geometrical detail of planetary models.

The macro history of astronomy in the period depends upon the concatenation of the ways it was articulated upon, and thus practiced, under competing natural philosophical claims. This allows further refinement of our previous correction of Kuhn on the Copernican revolution. Not only was astronomy, as we have just seen, a single agonistic tradition of practice—rather than a site of paradigm versus paradigm combat—but we can now affirm that astronomy existed in complex articulation relations with that other encompassing discipline and field of contention, natural philosophy. Hence, the Copernican debate was not about astronomy alone nor about astronomy on the one hand and a separate domain of "world views" on the other. It was precisely a battle about articulations of varieties of natural philosophy onto Copernicanism, or not. It was about the direct challenge in the field of natural philosophizing posed by realist Copernicanism—because realist Copernicanism only existed in articulations of non-Aristotelian natural philosophies onto Copernicanism. This is why the Copernican debate was a "hot spot" in the desperate natural philosophical struggle of the early seventeenth century, and in turn it helped fuel that crisis.[41] The supporters of *realist* Copernicanism needed to adduce a framework of non-Aristotelian natural philosophy, a new theory of matter and cause, adequate to explaining the heliocentric cosmos. The entire late sixteenth- and early seventeenth-century debate over realist Copernicanism (culminating in the emergence inside Kepler's and Descartes' respective philosophies of nature of a discourse of "celestial physics") was a phenomenon of competition at a now inflamed site within the natural philosophical field—no realist Copernicanism, no inflammation (Schuster 2013a; Schuster and Brody 2013). But why be a realist Copernican, unless you intend a quite radical overhaul of Aristotelian natural philosophy (and its rules) as such?

This sketch of an alternative mapping of Kuhn's terrain of the Copernican Revolution (as we must read the latter from *SSR*) begins to illustrate the price paid by Kuhn for not recognizing the role of systematic natural philosophies per se in shaping the contents and directions of the sciences, both existing and nascent.[42] Our case study of Kuhn on the Copernican Revolution also shows, on the one hand, the complex dialectic involved in his modifications of Koyré, which were executed while he was still conditioned by allegiance to Koyré, and, on the other hand, the vast distance separating our own best practice historiography, mapping, and notions

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Ptolemaic astronomy were shaped by Aristotelian natural philosophy: the finite Earth-centered cosmos, the distinction between the celestial and the terrestrial realms, and the primacy of uniform circular motion.

[41] "Hot spot" like "articulation" is a term of art in my model of the dynamics natural philosophy. Schuster 2013a, Chapter 2, section 2.5.4.

[42] For example, Kuhn speaks of atomism as a "new intellectual ingredient" providing the metaphysics of the cluster of classical sciences.

of tradition dynamics, from those of the younger Kuhn, post-Koyréan historical theorist. Finally, note that all these matters—emergent from our approach to the younger Kuhn as a historical theorist—are far from those usually canvassed in philosophical explications and criticisms of Kuhn.

19.10 Mapping Test Case II: Kuhn on the Rise of New Experimental Sciences

We can now turn to the second branch of Kuhn's map, dealing with the emergence of new experimental sciences in the wake of the seventeenth century Scientific Revolution. Here again, Kuhn filled a gap left by Koyré's historiography in a manner partially shaped by his own Koyréan leanings. Additionally, Kuhn's effort left problems and ambiguities, some of which he himself could have detected. Most importantly, he left two different accounts of how the new sciences arose.

19.10.1 Kuhn's Version One: Baconian Sciences Born in Ruptural Emergence of a First Paradigm

In *SSR*, Kuhn attacked the problem of the rise of new Baconian experimental sciences by invoking what may be termed a "two-place" historiography. Kuhn identifies the genesis of an experimental science with the emergence of a first "paradigm," against a field of "pre-paradigmatic" enterprises in the relevant domain of research. He seeks points of rupture, where such sciences are born, placing such events in the context of the preexistence of what is "not science" (Schuster and Watchirs 1990, pp. 3–7). At the beginning of *SSR*, Kuhn deploys the case of electrostatics to illustrate this theory of paradigm formation (Kuhn 1970, pp. 13–15). The pre-paradigm stage of electrical research was dominated by competing "schools," with differing "metaphysical" commitments (e.g., Cartesian, Newtonian) and differing preferred problems and starting points for surveying the burgeoning field of known electrical phenomena (e.g., stressing attraction/repulsion phenomena first and foremost or the phenomena of conduction, flow, and sparking). For Kuhn, the founder of the first real electrical paradigm was Benjamin Franklin, initially an adherent of the pre-paradigm school concerned mainly with conduction effects. This school "tended to speak of electricity as a 'fluid' that could run through conductors, rather than as an effluvium that emanated from non-conductors." They could deal with simple conduction effects, but not very well with the known attraction/repulsion effects. The work of Franklin and his successors, however, issued in a successful first paradigm:

> [...] that could account with something like equal facility for very nearly all these effects and therefore (provided) a subsequent generation of electricians with a common paradigm for its research. (Kuhn 1970, p. 15)

The "exemplar" of the Franklinists, their exemplary problem solution, out of which their full paradigm was articulated, was their "successful" explanation of the Leyden jar, an early capacitor. According to Kuhn, the basis of Franklin's exemplary, paradigm-forming problem solution was the possibility of conceptualizing electrostatic induction and hence the possibility of distinguishing between, on the one hand, the flow and conduction of electrical fluid and, on the other hand, its exertion (upon itself) of repulsion at a distance.

For Kuhn, these insights constituted the essence of the subsequent Franklinian paradigm, as he makes quite clear by conflating the establishment of Franklin's paradigm with the widespread comprehension and acceptance of the critical clarification of inductive effects (and of conduction versus distance effects): by packing these understandings of what we might term "the condenser as such" into the paradigm from the point of its "assimilation," Kuhn endows Franklin's paradigm with a destiny of unproblematically maturing, through articulation of its own pre-given conceptual and technical resources.[43] That is, once the rupture to this first genuinely scientific paradigm has occurred over against the field of pre-scientific/pre-paradigmatic "schools," a kind of epistemological predestination seems to hover around Franklin's paradigm. Assuming, as Kuhn does, that Franklin was quite clear about distinguishing induction from conduction, and, in general, flow effects of the electrical fluid from its distance effects, then Kuhnian historiography can gaze almost Whiggishly at the cumulative articulation of Franklin's paradigm. Kuhn sees this maturation and articulation of Franklin's paradigm as consisting essentially in the progressive mathematization of electrostatics later in the works of Coulomb, Cavendish, and Poisson. In neo-Koyréan fashion, Kuhn assumes that development from the ruptural moment of finding the "first paradigm" will consist in virtually preprogrammed moves, the central process being the all but inevitable drive to mathematicize the field (and so achieve in the experimental science in question the realization of the Koyréan goal of mathematization).[44]

19.10.2 Kuhn's Version Two: Baconian Sciences Born from Continuous Process (of "Scientificity")

Interestingly, in his later essay on "Mathematical Versus Experimental Traditions in the Development of Physical Science" (Kuhn 1977c), Kuhn provided from the same sources a model based more on continuity and process. This theoretical ambivalence and confusion can be traced back to Kuhn's grappling with his Koyréan

[43] Kuhn 1970, p. 21, pp. 28–29; 1977c, pp. 47–8; 1963, pp. 356–357. The latter paper, known as perhaps the most radical of Kuhn's historiographical efforts, was pointedly *not* included in Kuhn 1977a.

[44] I say "almost Whiggishly" because what we have here is a specifically Koyréan/Kuhnian variant of Whiggism. Kuhn and Koyré each rightly claimed that they did not practice the form of (inductivist and often "method-based") Whiggish narrative which they often criticized in others. Nevertheless, where Koyré had invoked a kind of destiny toward mature, comprehensive mathematization under his preferred ruling "Platonic" metaphysics, Kuhn played a Koyréan variation, with each first paradigm in an experimental field harboring a destiny toward mature and comprehensive mathematization.

heritage and with the demand to be more historically accurate. The upshot is a model quite at odds with the vision of normal science in *SSR*.[45]

Kuhn's continuity story of the Baconian sciences starts from the idea that in the Renaissance and seventeenth century, there existed in addition to the classical mathematical and biomedical sciences a heterogeneous collection of partially overlapping areas of what we should now term physical and chemical inquiry into the nature of matter, light, heat, magnetism, chemical reactions, and the like. Facts and experiments about such matters were not pursued within any of the existing, mature classical sciences, but were partially and variously the subject matter of alchemy, natural magic, and the crafts (Kuhn 1977c, pp. 32–33, p. 36, p. 46). From about 1650, these domains began to be reformed and recrystallized under the aegis of the triumphant rhetoric of Baconian method and experiment (Kuhn 1977c, pp. 41–46). The characteristic products of these emergent Baconian sciences were natural or experimental "histories," in which corpuscular-mechanism-guided experiment only loosely if at all and, correspondingly, corpuscular-mechanism was largely unaffected in detail by experimental outcomes.

According to Kuhn, the Baconian sciences, largely subsisting independently of the cluster of classical sciences, underwent a three-stage process of maturation over the next century and a half. Initially, from the late seventeenth century to well into the eighteenth century, the pattern of Baconian compilation of "histories" dominated. The Baconian sciences "remain underdeveloped," if we take the criterion of development to be "the possession of a body of consistent theory capable of producing refined predictions" (Kuhn 1977c, p. 47). But, toward the middle of the eighteenth century, more systematic forms of experiment developed in these fields, focused upon revealing sets of phenomena to which corpuscular and cognate explanatory concepts were increasingly applied. The theories in these domains remained mainly qualitative, but they could be matched up to specific experiments with degrees of conformity not seen earlier in the century (ibid.). This second stage therefore corresponds to that identified with the acquisition of first paradigms in the model of *SSR*. Finally, in the last third of the eighteenth century, some portions of these relatively mature fields became more quantitative and were subjected to sophisticated mathematical articulation (Kuhn 1977c, pp. 47–48).

In sum, Kuhn's second story of the Baconian sciences is a *continuity*-oriented narrative of the "pre- and post-paradigm" states of these sciences. His account veers away from the official categories and two-place historiography of *SSR*. Hence, Kuhn is assuming that these sciences really existed from their initial seventeenth-century points of crystallization and that, in general, there were less mature sciences in the pre-paradigm stage and more mature sciences in the post-paradigm stage. Kuhn's continuity, therefore, can only be a continuity of some kind of scientificity.

Evidence of Kuhn's difficulties with the pre-paradigm/paradigm distinction appears in his description (mentioned earlier) of the competing pre-paradigm schools of electricians, each characterized by its own metaphysical commitments and preferred domain of problems and phenomena (Kuhn 1970, pp. 14–15). This, of

[45] Material in the next five paragraphs derives from Schuster and Watchirs (1990).

course, makes the pre-paradigm schools seem very paradigm- or science-like, and it is a description holding for the pre-paradigm stages of other fields as well.[46] Replying to critics in his 1970 "Postscript" to the second edition of *SSR*, Kuhn responded in the spirit of his earlier ambivalent assertion that pre-paradigm researchers "were scientists, [but produced] something less than science" (Kuhn 1970, p. 13), arguing that the transition to maturity of a science "need not be associated with the first acquisition of a paradigm," for the members of the schools of the pre-paradigm period of a field "share the sorts of elements which I have collectively labeled a paradigm" (Kuhn 1970, p. 179). What changes, Kuhn now suggested, is not "the presence of a paradigm but rather its nature. Only after the change is normal puzzle-solving research possible" (ibidem).[47]

This certainly seems a distinction without much of a difference, when we recall Kuhn's views on the effectiveness and maturity of the fluid school of electricians even before Franklin.[48] Kuhn's difficulty in grounding his two-place historiography in the relevant pre-eighteenth-century facts led him in the direction of these vague intimations of "continuity." But there was a deeper cause of his problem. It had to do with the issue of proper historiographical categories. That is, the underlying cause of Kuhn's problem resides in his failure, again, to deploy a category of natural philosophy. We saw how the absence of this category—shaped by his Koyréan proclivities—vitiated much of his understanding of the Scientific Revolution. Now we shall see how that absence permitted (and demanded) his use of the term "Baconian sciences" to articulate the continuity that looms through his failure to enforce his own pre-paradigm/paradigm distinction.

19.11 Articulating the Category of Natural Philosophy: Rethinking the Motor and Map of the Emergent Experimental Sciences

Our argument in this section has three steps. First, recall that rather than taking seriously the historical reality of natural philosophizing as an institution and activity, Kuhn spoke of "intellectual ingredients," neo-Koyréan metaphysical groundings for

[46] Similarly, in discussing pre-paradigm schools in physical optics, Kuhn (1970, pp.12–13), even observes that they each had different respective "paradigmatic observations."

[47] He continues, "Many of the attributes of a developed science which I have associated with the acquisition of a paradigm I would therefore now discuss as consequences of the acquisition of the sort of paradigm that identifies challenging puzzles, supplies clues to their solution, and guarantees that the truly clever practitioner will succeed" (ibidem).

[48] In effect, Kuhn still needed a rupture, a transition to maturity, but he was left with two weak indicators of its supposed occurrence: relatively greater consensus—the end of interschool debate—apparently dependent upon relatively greater puzzle-defining and puzzle-solving power (which also instills confidence). But, given the paradigm-like virtues of the pre-paradigm schools, it is still not possible to see, in Kuhn's terms, why and how such points of superiority are made out and enforced upon the debating parties.

various scientific pursuits. In his continuity story of the maturation of the Baconian experimental domains between the late sixteenth and mid-eighteenth century, Kuhn granted varied roles to such "ingredients": Hermeticism promoted interest in electricity and magnetism; Paracelsianism and Helmontianism promoted the status of "chemistry"; and Baconianism, with its rhetoric of utility and of experiment sanitized of magic and alchemy, thereby effectively crystallized the experimental "sciences" and rendered them fit for the injection of the ingredient of corpuscular-mechanism at the hands of Boyle, Hooke, and others. Corpuscular-mechanism, as an ingredient, had equivocal effects upon the Baconian sciences: it completed the purge of magic, but its explanatory resources were only beneficial where they were "adapted" to the needs of "particular areas of experimental research" (Kuhn 1977c, p. 47, pp. 53–54). So, Kuhn shifted his "intellectual ingredients" around to help explain moments in the trajectory of maturation of *already existing* Baconian sciences.

The second step is to conceptualize how theoretical concepts are embodied within experimental hardware and expressed in their outputs.[49] The evolution of best practice in history and sociology of science since Kuhn indicates the way forward: we follow Gaston Bachelard's (Bachelard 1938, 1949) monumental insight that the phenomena or objects of inquiry of experimental sciences are manufactured in and by experimental hardware, because such hardware are themselves developed, used, and understood as materializations of conceptual structures—what he called *phénoméno-techniques*. Bachelard insisted that embodied concepts can only be mathematical and noncontingent, but post-Kuhnian sociology of scientific knowledge scholars have stressed the fluidity and negotiability of meanings embodied in hardware (Collins 1975; Pinch 1985; Shapin 1982; Barnes 1982).This allows us to speak not only of couplings of *mathematical theory* and hardware, but of *discourse-hardware couples*, which are contingent products, outcomes of social processes of closure, and subject to possible renegotiation of the elements on either side of the couple.[50] This "softening" of Bachelard in recognition of continuous processes of construction and negotiation brings out the *historical* character of experimental practices and allows them to be related to our model of natural philosophical processes.

Third, one can ask the following: If early experimental scientists, or better natural philosophers, coupled discourse to hardware, where did the discourse come from?[51] The answer obviously is that in early experimentalism, the discourse at stake in materializations was of natural philosophical provenance. This is trivially obvious: couplings of hardware and discourse *without* use of natural philosophical

[49] Material on Bachelard in this paragraph evolved from findings first put forward in Schuster and Watchirs (1990, pp. 7–11, pp. 21–25). A fully articulated, pedagogical model of how thus to deal with the issue of the theory loading of experimental hardware is presented in my textbooks: Schuster 1995a, Chapter 14, pp. 145–146 and Schuster 2013b, pp. 269–77.

[50] All this goes well beyond the insights of Popper, Hanson, and then Kuhn about "theory loading" of experience and experiment. Arguably, it was Bachelard, not Kuhn or the others, who specifically stimulated the efflorescence of SSK inquiry into experiment.

[51] Material in the remainder of this section was first presented in Schuster and Watchirs 1990, pp. 14–29.

discourse were called crafts and practical arts. From the early seventeenth century, the natural philosophical field was increasingly characterized by an imperative to articulate competing natural philosophical utterances onto hardware, in order that the reports of the outputs of hardware could serve to support other natural philosophical claims.[52]

All this permits us to sketch how experimental sciences crystallized out of the dynamics of the field of experimental natural philosophy. The imperative to articulate competing natural philosophical utterances onto hardware exerted subtle pressure toward creating domains of relative specialization, that is, sets of couples increasingly articulated in terms of more localized and specific swathes of originally natural philosophical discourse: increasingly, clusters of hardware-discourse couples emerged within the natural philosophical field—constituting such experimental domains as electricity, magnetism, and heat. (Kuhn read this process toward domain formation as eruption of "first paradigms" in the experimental sciences.) As this happened, natural philosophers increasingly abandoned the aim of systematic natural philosophical discourse to the requirements of engaging the flow of practice in these narrower spaces. More local explanatory "dialects" emerged out of the originally wider natural philosophical field of discourse.[53] This model avoids Kuhn's penchant for ruptures, but also rationalizes his unconsummated intimation of continuity, by explaining the formation of experimental sciences out of the dynamics of the already existing natural philosophical culture. Discipline formation was an unintended process within the natural philosophical field and helped to fragment and eventually evaporate it.

Kuhn played himself into difficulties by failing to theorize natural philosophy as a historical category. He neglected to consider whether the dynamics of natural philosophy had anything to do with the Baconian sciences and their long gestation process to "full maturity." Kuhn had defined his pre-paradigm schools primarily in terms of metaphysical predilections—in my terms essentially natural philosophical commitments. Kuhn had trouble factoring such schools into his strict *SSR* model: on the one hand, he had to depict some schools as too "paradigm-like," while, on the other hand, he had to introduce into the schools a suitable degree of "Baconian" fact gathering, so that they would remain pre-paradigmatic. The model of domain and dialect formation cuts across Kuhn's tortured variety of "schools." When Kuhn sought science-making breakthroughs in one or another school, he was looking for the wrong thing in the wrong place. His alternative strategy of tracing the long-term maturation of the "Baconian sciences" also missed the mark, misconceiving the object continuously in play—experimental natural philosophy—and its peculiar dynamics. Slightly improving upon Koyré, by multiplying the possible metaphysical bases of various "scientific" traditions, meant continuing Koyré's avoidance of the presumably nonscientific and not important realm of natural philosophy.

[52] This aspect of the model being offered was developed in Schuster (2002, 2013a, c) and Schuster and Taylor (1997, pp. 519–24).

[53] For case of electro-statics up to, through and beyond Franklin, Schuster and Watchirs (1990, pp. 30–36).

19.12 Conclusion

We have analyzed the younger Kuhn as a "critical historian," who desired to understand the motor or dynamics of change in the sciences and to produce an initial map of how those changes had played out over time. As a critical historian, Kuhn was trying both to articulate sympathetically and revise radically what he took as Koyré's riding orders for the history of science profession. The younger Kuhn was the way he was as a historian largely because of his complex grappling with what he saw as the historiographical heritage of Koyré. It would have been easy simply to list the similarities and differences between Koyré and the younger Kuhn. But, the real issues come out when one looks—as we have—in greater depth at exactly what was going on in Kuhn's mapping and motor exercises. That allowed us to identify key areas—concerning discovery, experiment, and natural philosophy—where Koyré and Kuhn each underplayed or missed opportunities that were later picked up during early post-Kuhnian developments in sociology of knowledge and Scientific Revolution studies.

It is worth underscoring the "history-centric" nature of our inquiry and our findings. All the issues we have dealt with—Kuhn's relations to Koyré and the relations of post-Kuhnian thought to Kuhn himself—reside squarely in the realm of historiographical theorizing. We have not examined issues about Kuhn doing philosophy. Nor have we had to concern ourselves with philosophical evaluation of Kuhn's work. All the matters we have considered arose when a critical historian of science, the young Tom Kuhn, engaged in category and model formation and modification, endeavoring, as we said at the beginning, to be his own purveyor of key historiographical categories.

Of course, such critical history did not concern the older Kuhn. From the time of his being philosophically ambushed by Popper and the Lakatosians at the Bedford College Conference in 1965 (Lakatos and Musgrave 1970), Kuhn began to retreat from creative thought about historical process, slowly sinking into a maelstrom of unending and unwinnable debate with philosophers about rationality and method. Kuhn's occluded and aborted trajectory in critical history was taken up by younger historians and sociologists of science, who often knew their philosophy of science, but who wished to advance post-Kuhnian critical history of science/sociology of science without first having to try to legitimate themselves in philosophical circles, as the older Kuhn increasingly did.

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