

Shifting Paradigms

Thomas S. Kuhn and the History of Science

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

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Chapter 8

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

Stefano Gattei

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

Sextus Empiricus

The Historical Turn in Twentieth-Century Philosophy of Science

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

This fact is extremely significant, not only from a purely philosophical point of view, but also from the wider cultural perspective. And while more than one philosopher contributed to this important shift of focus, Thomas Kuhn undoubtedly played a major role. From the historical point of view, this mere fact makes Kuhn one of the most significant philosophers of the past century, and if we think of his influence on such diverse and far-away fields, our consideration of his con-

tribution grows further. Indeed, few philosophers (and even fewer historians) of science have influenced as many readers as Kuhn; whether one agrees or disagrees with him, no one can deny that the key notions of his philosophy (“normal science,” “revolution” or “incommensurability,” for instance) and some of the terms he introduced (most notably, “paradigm” and its derivatives, such as “paradigm shift”) have been at the very center of the heated philosophical controversies that characterized the last decades of the past century. Kuhn's 1962 seminal work, *The Structure of Scientific Revolutions*, has become a modern classic, used (and misused) by different people in diverse contexts as the token in various ongoing disputes. Providing a common reference for cross-disciplinary discussions, it has affected debates across fields as different as historiography, sociology, politics, economics, psychology, theology, literature, feminism, cultural studies, art, education and more. Half a century after the publication of *The Structure of Scientific Revolutions*, Kuhn's shadow hangs over almost every field of intellectual inquiry.

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

Paradigms and Truth

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

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

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

A scientific theory is usually felt to be better than its predecessors not only in the sense that it is a better instrument for discovering and solving puzzles, but also because it is somehow a better representation of what nature is really like. One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. Apparently generalizations like that refer not to the puzzle-solutions and the concrete predictions derived from a theory but rather to its ontology, to the match, that is, between the entities with which the theory populates nature and what is “really there”.

Perhaps there is some other way of salvaging the notion of “truth” for application to whole theories, but this one will not do. There is, I think, no theory-independent way to reconstruct phrases like “really there”; the notion of a match between the ontology of a theory and its “real” counterpart in nature now seems to me illusive in principle. Besides, as a historian, I am impressed by the implausibility of the view. I do not doubt, for example, that Newton's mechanics improves on Aristotle's and that Einstein's improves on Newton's as instruments for puzzle-solving. But I can see in their succession no coherent direction of ontological development. On the contrary, in some important respects, though by no means in all, Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's. (Kuhn 1970b, 206–207; see also 1970a, 205–207)

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

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

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

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

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

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

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

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

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

From Paradigms to Lexicons

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

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

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

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

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

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

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

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

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

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

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

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

Coherence Theory and Correspondence Theory

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

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

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

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

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

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

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

The second step involved a change of view concerning the formal structure of the system of scientific statements. It consisted in loosening the verificationist conception of meaning typical of Wittgenstein's thought: in so doing universal statements, such as scientific hypotheses, can be regarded as meaningful even if they do not receive a logically conclusive verification by singular statements. Furthermore, Hempel remarked, also several propositions that appear to be singular in form possess a logical, hypothetical form. The singular statements we adopt depend upon which formal system we choose. Thus, also a second fundamental principle of the *Tractatus Logico-Philosophicus* must be abandoned; it

is no longer possible to define the truth or falsehood of certain basic statements, whether or not they may be atomic statements or protocol statements, or other kinds of singular statements. "So," Hempel wrote, "the refined analysis of the formal structure of the systems of statements involves an essential loosening or softening of the concept of truth; [...] In science a statement is adopted as true if it is sufficiently supported by protocol statements" (Hempel 1935, 13).

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

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

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

tion is the basis for scientific beliefs, not even it is free from revision in the light of theoretical change.

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

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

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

"Furthermore," Carnap continues, "the formulation in terms of 'comparison,' in speaking of 'facts' or 'realities,' easily tempts one into the absolutistic

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

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

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

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

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

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

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

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

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

Consolations for the Specialists

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

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

⁸See Bartley (1990, chaps. 14–15).

⁹By no accident in the closing pages of *The Structure of Scientific Revolutions* Kuhn speaks of “dis-solution” rather than “solution” of the problem of induction: see above footnote 1.

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

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

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

Kuhn's Unfinished Historiographical Revolution

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

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

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

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

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

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