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Topoi

An International Review of Philosophy

ISSN 0167-7411

Volume 32

Number 1

Topoi (2013) 32:81-89

DOI 10.1007/s11245-012-9137-8



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The *Structure*'s Legacy: Not from Philosophy to Description

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Published online: 23 October 2012
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Abstract In the paper I consider how empirical material, from either history or sociology, features in Kuhn's account of science in *The Structure of Scientific Revolutions* and argue that the study of scientific practice did not offer him data to be used as evidence for defending hypotheses but rather cultivated a sensitivity for detail and difference which helped him undermine an idealized conception of science. Recent attempts in the science studies literature, appealing to Wittgenstein's philosophy, have aimed at reducing philosophy to multifaceted empirical research in relation to science. I discuss how this turn which is at odds with Wittgenstein's philosophy, cannot be a continuation of Kuhn's project which bears similarities to Wittgenstein's.

Keywords Kuhn · Wittgenstein ·
Social studies of science · Sociology of science ·
The Structure of Scientific Revolutions

1 Introduction

The legacy of T. S. Kuhn's *The Structure of Scientific Revolutions*, fifty years after its publication in 1962, is still a puzzle. On the one hand, it is undeniable that the book has exerted a huge influence on a vast variety of disciplines and has even infiltrated general culture. The concepts of 'paradigm' and 'paradigm shift' continue to pepper popular talk and are referred to on occasion by lay persons and scholars alike. However, the academic and scholarly

reception of *Structure* itself was not ever really enthusiastic. It has been assigned as reading, and was read a lot on and off university campuses, but it was commonly invoked in scholarly literature to be criticized, especially in the first couple of decades after its publication, both for what it said (and presumed to imply) and for what it did not say. In philosophical circles the reaction ranged from cautious appreciation, to hostile rejection and, finally, to marginalization.¹ The main concern was that it courted with irrationality in science. In history of science the book was found problematic for multiple reasons: the old guard feared that it undermined the belief in the rational progress of science while the younger generation of historians who had assimilated *Structure*'s perspective found the book inaccurate, too generalizing, but also too internalistic, that is, too confined to theories and ideas rather than sociological or anthropological facts. A major complaint was that it lacked historical detail and documentation. The area which embraced *Structure* heartily, at least initially, was that of the social sciences, part of which eventually morphed into what became science studies.² Professionals in these fields welcomed the foregrounding of their subject matter and the critical stand towards idealizations of science, but quickly resented Kuhn's reluctance to follow them into relativism, constructivism, understanding truth in terms of negotiations or scientific knowledge in terms of

¹ "Kuhn's work has not remained at the centre of the philosophy of science" (Bird 2011).

² Alexander Bird (2011) cites two reasons for this: "First, Kuhn's picture of science appeared to permit a more liberal conception of what science is than hitherto, one that could be taken to include disciplines such as sociology and psychoanalysis. Secondly, Kuhn's rejection of rules as determining scientific outcomes appeared to permit appeal to other factors, external to science, in explaining why a scientific revolution took the course that it did."

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power politics and struggle. The charge, in this case, was that Kuhn was not radical enough.

Nearly everybody found flaws in *Structure*³ and was eager to bypass it or outgrow it. Yet, the concepts the book introduced and the problems it highlighted became common property and formed the background against which contemporary philosophy of science and science studies are practiced.⁴ Even if one rejects, for instance, conceptual incommensurability or the theory-ladenness of observation, one cannot just ignore them, at least in some areas of the relevant fields, as if these issues have never been put on the table. Despite his influence, however, Kuhn cannot be associated with any school or group of philosophers, historians or social scientists, who can be regarded as practicing the type of work he inaugurated.⁵ Very few historians of science may be said to be doing the kind of historical work that he had done and hardly anyone practices what can be found in *Structure*.

But what can be found in *Structure*? What did Kuhn suggest in his book that has or has not been continued in contemporary work regarding science? In the paper I will concentrate on one particular understanding of *Structure*, namely, the one which takes Kuhn to be advancing a factual account of science (and more particularly a sociological one) and consider whether the descriptive studies of science which developed in the wake of Kuhn's *Structure* can be said to be following Kuhn's lead. I will argue that Kuhn's project, although it made use of empirical material, was primarily philosophical in purpose and, therefore, the descriptive works that it may have inspired or fostered do not reflect its spirit and orientation.

2 Kuhn's Project in *Structure*

Kuhn's project, as it was developed in *Structure*, was understood to be descriptive and empirical in many ways. The most common is the one which takes Kuhn to be

³ "The *Structure* is full of holes" says John Heilbron (1998, p. 514) who is trying to understand why, despite this, Kuhn's book had such a great impact.

⁴ "Tom's contributions to the field over the last four decades have been absolutely formative" (Fox Keller 1998, p. 15).

⁵ "[T]here is no characteristically Kuhnian school that carries on his positive work." (Bird 2011). John Ziman, however, thought that in science studies, they try to follow Kuhn's example. In introducing Kuhn as the recipient of the John Desmond Bernal Award in 1983, Ziman announced that "We are all Kuhnians" (Ziman 1983, p. 24). What he meant was that they try to bring together various disciplines in the manner that Kuhn did it in his work. He calls Kuhn a unifier in opposition to being a revolutionary. "The deep message of *The Structure of Scientific Revolutions* was that these jurisdictional disputes [of the different disciplines] were futile. A scientific theory can only be grasped metascientifically as an entity with intertwined philosophical, historical, and sociological characteristics" (ibid.).

forming a general account of science and its development, based on evidence drawn from history. The pattern of *paradigm/normal science/anomaly/crisis/revolution/paradigm/etc.* is supposed to be found in and founded on history and checked as a hypothesis against it. Alexander Bird calls this "theoretical history"⁶ and takes it to be involving a descriptive and an explanatory element much like natural sciences operate. Another way of understanding *Structure* as empirical is to bring into relief the social factors that *Structure* points to as pertaining to scientific practice. In this respect the focus is on sociological considerations, such as the structure of scientific community, the role and nature of scientific education, scientific controversies and means of persuasion.⁷

Kuhn himself may have encouraged this understanding of his work. In the Preface to *Structure*, he said that historical and sociological research is fruitful for the view of science developed in the book while in the "Postscript" to the second edition of *Structure* he contended that "[i]f this book were being rewritten, it would ... open with a discussion of the community structure of science, a topic that has recently become a significant subject of sociological research" (1970, p. 176). He then proceeded to mention several sociological works in an effort to show how this topic can be further professionally investigated (ibid.). Much later, in an interview to Giovanna Borradori, first published in Italian in 1991, he repeated the claim about the sociological character of *Structure*: "Today I would consider it [*Structure*] part of a discipline that at that time did not even exist: the sociology of knowledge" (1994, p. 157).

Are the interpretations that take *Structure* to be empirical in a historical or sociological sense correct? I have argued elsewhere (Kindi 2005) why Kuhn's *Structure* was not based on historical evidence the way a hypothesis is supported by facts and I have criticized Bird's view. I have claimed that Kuhn used historical examples as anti-essentialist Wittgensteinian 'reminders' to expose a variegated landscape in the development of science. But what about Kuhn's claim that his work is sociological? Is it correct and how should we understand it?

An explicit attempt to explain it can be found in the remarks Kuhn made in 1983 on receiving the

⁶ Bird's 'theoretical history' ought to be distinguished from speculative historical accounts of the type offered by Hegel. Feyerabend, however, credited Kuhn with a Hegelian-like philosophy of history (Hoyningen-Huene 1995, p. 353).

⁷ Bird (forthcoming) combines the two empirical approaches and calls this merging the historical-sociological strand of Kuhn's naturalism. He identifies another naturalistic strand, namely the cognitive-psychological, since Kuhn makes references to psychological findings and experiments that are brought to bear on revolutionary scientific change.

“John Desmond Bernal Award”, published with an addendum in the *4S Review*, *Journal of the Society for Social Studies of Science*:

Structure is sociological in that it emphasizes the existence of scientific communities, insists that they be viewed as the producers of a special product, scientific knowledge, and suggests that the nature of that product can be understood in terms of what is special in the training and values of those groups (Kuhn 1983, p. 28).

Kuhn was clearly interested in finding out what makes science special and turned to scientific communities. It may seem that what he suggested was an empirical investigation of scientific education and scientific practice very similar to what contemporary science studies professionals undertake. Kuhn, however, was very critical of the descriptive works of the social studies of science. He repeatedly expressed his reservations and regrets in his later papers, especially in “The Trouble with the Historical Philosophy of Science” (Kuhn 2000b, p. 110) where he characterized the strong program and the micro-sociological studies of science that followed it as “damagingly mistaken”, “absurd”, and “scarcely satisfactory”. How can we reconcile Kuhn’s claim that his book is sociological with his critical stance towards the actual sociological studies of science? And what is more, how does his view regarding the sociological character of his work fit his later express claim that his model is derived and can be derived from first principles (Kuhn 2000b, pp. 111–112)?

In Kindi (2005) I address this second question in relation to the alleged historical character of Kuhn’s work. I argue in that paper that Kuhn may draw on historical examples and cases but does not form his model by amassing historical facts or by checking hypotheses against them. His arguments are mostly philosophical and his account, I propose, is transcendental. By that I mean that Kuhn gives us the historicized conditions of the possibility of science. These comprise dogmatic training, puzzle solving, adherence to the values of accuracy, empirical adequacy and the like. The whole Kuhnian model subsequently follows: exemplars or paradigms lay down rules which establish the normalcy and normativity of the practice, meaning is provided by rules and not independently from the world or experience, radical change or revolution is associated with change of meaning which results in incommensurability. The concrete expression, however, of the conditions for the possibility of science, may vary with circumstances and the historical period. This is how history enters Kuhn’s a priori model. His a priori considerations regarding the logical connections between concepts are supplemented by a glance at history which offers Kuhn not only a dynamic conception of science as he surmises (Kuhn 2000a, p. 95), but also the variability of the

ways science had been practiced in different historical settings. In this regard I compare Kuhn’s resort to historical cases to Wittgenstein’s consideration of examples of language use. Wittgenstein reminds us of facts of our natural history to combat an essentialist idea of meaning (PI 415) and Kuhn reminds us of facts of ‘scientific history’ to combat an essentialist idea of science.

Can the same case be made regarding the sociological character of Kuhn’s work? Can it be said that Kuhn’s sociological considerations serve a philosophical purpose, and that, therefore, the purely descriptive studies of science do not continue his work? In what follows I will examine more closely how Kuhn understands the sociology of science and argue that, indeed, his account is different from merely empirical investigations of the social aspects of scientific practice.

3 Kuhn’s Sociology

In the passage cited from Kuhn’s comments upon receiving the “John Desmond Bernal Award”, it seems clear that scientific communities are taken to be the producers of a *special* product, i.e., scientific knowledge, and that sociology is expected to explain the special epistemic status of science. In the social studies that emerged later he finds no such concern. Instead, practitioners of the new field lay emphasis on socio-economic interests eradicating, thus, according to Kuhn, any difference between external and internal sociology of science and, more importantly, any special status of the scientific product as such. Kuhn never gave up on his concern with the cognitive part of science and thought it was, in his words, a “disaster” (ibid., p. 30) to treat science as any other practice.

But, what kind of sociology did Kuhn expect? What kind of sociology did he practice and recommend? Again in his remarks on receiving the Bernal Award he says that he came to realize, reading Koyré’s letter to him, that *Structure* was a “gap filler”. By concentrating on scientific communities, the *Structure* intervened between history of science, on the one hand (internal history of science), and social history on the other (Kuhn 1983, p. 27).⁸ Scientific

⁸ Kuhn makes virtually the same point in his long autobiographical interview (Kuhn 2000a, p. 286). Kuhn remembers that Koyré, shortly before he died, sent him a letter in which he said “you have brought the internal and external histories of science, which in the past have been very far apart, together.” Kuhn says in the interview that “he had not seen this coming” but when he thought about it he understood that Koyré was right. When one of the interviewers asked him how he failed to realize it, Kuhn replied: “I hadn’t thought of it [*Structure*] as doing that. I mean, I saw what he [Koyré] meant ... I thought of it as pretty straight internalist” (ibid., p. 287). Cf. Kuhn (1983, p. 27): “My principal efforts have, that is, been directed towards what I have sometimes called ‘dynamic interrelationships of pure ideas’”.

communities seem particularly suitable for this mediating role since they belong both to science proper, that is, to an internal realm, and to the society at large as they consist of real people who are not cut off from the theories they produce as the typical normative philosophy of science had it.⁹ But how did Kuhn manage to fill the gap between the internal and external approach to science? How did sociology which features in his work manage this feat? Kuhn says that he drew “sociological conclusions from the developing cognitive practices and products of scientists” and he expected sociology “to traverse the path in the reverse direction”, i.e., to relate the results of sociological inquiry to the content of science explaining also how these contents change with time (ibid., p. 28). This ambition, he contends, “remains to be fulfilled”.

How did Kuhn draw sociological conclusions from the developing cognitive practices? Here is what he says about sociology in the same paper:

Having insisted upon those points, ... [the points emphasizing the role of scientific communities in *Structure* with their values and norms] I proceeded to *make up the sociology of such communities* as I went along, or rather to draw it from my experience with the interpretation of scientific texts supplemented by my experience as a student of physics. That is an abominable way to do sociology, and it did not occur to me that its outcome would, *qua* sociology, have a claim on the attention of members of that profession. They might, I suppose, learn something useful from my book, but that something could not be sociology [emphasis added] (ibid.).

This is a striking and puzzling passage. Kuhn has judged *Structure* to be sociological in character and yet he says that it has no value for sociologists since what he has supplied is only “made up”, drawn from his personal experience as a student of physics and as an interpreter of scientific texts. One might think that by saying that he “made up” the sociology of scientific communities, Kuhn simply meant that he had just sketched or prepared such an account. But the judgment that this is an “abominable” way of doing sociology rules out, I think, this option. The reference to a “made up” sociology is clearly derogatory and is

⁹ C. P. Snow in his essay “The Two Cultures: A Second Look” also assigns the social sciences (social history, sociology, demography, political science, economics, government, psychology, medicine and social arts) a mediating role but, this time, between the sciences and the humanities. Unlike Kuhn who is interested in understanding science in a rich way and not solely in terms of idealized scientific theories, C.P. Snow is interested in establishing contact and communication between the practitioners of the two fields. He does not, however, explain how the social sciences can play this role of intervening to improve intelligibility between the divided cultures (Snow 1998, pp. 69–71).

particularly interesting given that Kuhn has been accused of also “making up” history. Steve Fuller (2000, p. 195), for instance, has alleged that Kuhn invented mythical constructs, or at best ideal types, instead of relying on accurate historical data while Robert Westman (1994, p. 82) noted that Kuhn made use of a “fictive speech” and of a “hypothetical Copernican convert” (emphasis in the original, ibid., p. 85, n.14), instead of providing the words of historical figures when he argued for the radical changes in “seeing” after a revolution.¹⁰ How should we understand this use of “made up” history or sociology?

A “made up” account may be taken to mean that Kuhn simply failed in his work and that his sociology is just arbitrary and unfounded. Commentators have actually blamed Kuhn for bad science -bad history to be exact. It has been maintained that his account is “inaccurate” (Bird 2000) or thinly supported (Kourany 1979), while Sharrock and Read (2002), having a different objective, found it “largely unevidenced” and said that “we have mostly simply to take it [Kuhn’s grasp of the actualities of normal science] on trust from him” (p. 107).¹¹ Did Kuhn make up his account after failing to properly ground it? In my view, Kuhn is not guilty of bad science and, certainly, he did not deceptively fabricate some spurious sociology *in lieu* of an accurate one. I believe that Kuhn’s claim about a made up sociology modestly distinguishes his own efforts to understand the sociological dimension of scientific practice from the proper investigations of professional sociologists. What he meant to say is that his sociology does not qualify as sociology proper because it was not done following the sophisticated methods of professional sociologists. His sociological reflections were drawn from his own experience as a physicist and an historian and were supposed to serve philosophical concerns. His primary aim was to understand what science was all about and in his effort to explain its peculiarities he was driven to think about the conditions that make it possible. It might be feared that what we are offered are just idiosyncratic observations, a mere subjective opinion, a personal take on things. This understanding of what Kuhn does would take his account

¹⁰ It should be noted in fairness to R. Westman that his references to a fictive speech and a *hypothetical* character in Kuhn’s *The Copernican Revolution* (1957), as well as his claims that certain of Kuhn’s theses are at odds with historical fact (Westman 1994, p. 82), are not meant to reflect negatively on Kuhn. Westman’s point was that *The Copernican Revolution* and the *Structure* had different orientations, the latter being more theoretical, and therefore, more philosophical, rather than historical or anthropological (ibid., p. 114).

¹¹ Sharrock and Read wanted to show that Kuhn was not acting as a scientist but rather as a philosopher of a Wittgensteinian therapeutic spirit. They argued that Kuhn was not in the business of offering empirical generalizations but rather in the business of deflating, of bringing down the received image of science dominant in standard normative philosophy of science.

to be an empirical hypothesis and a very weak at that, given that it is based solely on what he himself knows.

There is, however, another way to see what Kuhn is doing. Under this more charitable interpretation, Kuhn does not lay out a hypothesis to be confirmed, but speaks confidently as a competent participant of the practice of science. He speaks authoritatively about his own experience, not because he has unassailable proof, not because he may not be wrong, but because he knows what he is talking about. He does not need to gather evidence in order to say what is going on in the sciences; he is the source of that evidence. This is a point made by Stanley Cavell (2002, p. 4) and Martin Gustafson (2005, pp. 365–368) when they talk of the method of ordinary language philosophy. When the ordinary language philosopher, Austin, for instance, reminds us of what we say in language, evidence talk is irrelevant. The philosopher speaks definitively not because he may not be making a mistake but because he is entitled to speak with self-reliance. One may take it that one who speaks like this is just voicing his or her own subjective opinion which is to be checked for correctness. It is true that it is often the case that we check the correctness of what we are reporting about language by consulting a dictionary, an authority or our fellow native speakers. But that does not mean that we usually go wrong. There need to be some certainty and confidence in the use of language in order to even raise the question of correctness. If we were often mistaken, if we were often to find ourselves in disagreement with others, we would not really be real participants in the practice under consideration. And there wouldn't be a practice at all if speakers were often in disagreement among themselves as to what is the right way to say or to do things.

Kuhn did not practice ordinary language philosophy. Yet, I suggest that his sociology, “made up” solely from his own experience, be seen under this light. He calls it “made up”, not because he fabricated it, but simply because it does not meet the standards of the profession in the sense that it is not based on well researched empirical data collected, for instance, by using questionnaires or statistical models.¹² Kuhn needs to speak authoritatively of the connections between science and what makes it possible and he speaks of his personal experience as a physicist and historian in order, not to inform us of something we do not know, something that a disengaged social scientist (a sociologist or an anthropologist, for instance) might have done, but to remind us of similar experiences that we might have had of what science is like. This is the

¹² In fact, Sharrock and Read in their book (p. 107) wonder “why sociologists did not demand of Kuhn at least as high a standard as they would demand from any piece of work submitted to one of their own professional journals?”.

reason, I think, that his description resonates so well with scientists and with whoever has had some experience of the practice.¹³ Kuhn does not develop an empirical theory, but brings into relief what is familiar to whoever has some acquaintance with science: how textbooks function, what kind of training scientists receive, how research proceeds, what kind of problems scientists encounter. One may take his assessment of scientific practice as the statement of his subjective personal opinion but that would be to misunderstand the character of his testimony. He is not venturing a judgment as an answer to a question posed from without to be put next to other personal views in an empirical inquiry. He is offering his expert understanding of the field as someone who has taken part in the practice and has studied it closely.¹⁴

What does this interpretation tell us about Kuhn's project in *Structure*? I think it tells us that it is not empirical in the sense used in the social sciences. Kuhn had philosophical concerns, and by considering the phenomenology of scientific life (the type of education scientists get, the textbooks they use, the research papers and books they publish, the consensus they exhibit, the disagreements they have, etc.), he tried to account for the distinctiveness of the product of science, i.e., scientific knowledge. He spoke authoritatively as a practitioner of science and offered reminders of the scientists' experience, for instance, the type of training they have, in an effort to find out what it is that undergirds and sustains scientific practice and makes scientific knowledge possible. This approach resembles a situated/historicized transcendental analysis (cf. Kindi 2005), but is also reminiscent of Wittgenstein's assembling of reminders for a particular purpose (PI §127). The purpose served in the case of Kuhn is the demolition of the standard image of science and the better understanding of what science is (Kuhn 2000b, p. 108).

¹³ *The Structure of Scientific Revolutions* was very well received by scientists (see Kuhn 2000a, p. 282) in comparison to standard philosophy of science texts.

¹⁴ In an essay entitled “Scientific experiences of a European scholar in America” (1968) Adorno made a pertinent comment. Recounting his days in the United States, he remembers that certain judgments of his about music and musical preferences were taken by his colleagues in relevant sociological research projects to be “unproven”, “idle speculations”. “I provoked the objection”, he says, “that I was not to hear for the last time: ‘Where is the evidence?’” Adorno's judgments were relegated to mere subjective reactions to the stimulus of music, which was the object of the investigation, or treated as ingenious prophecies that were miraculously confirmed (Adorno 1968, pp. 349–350). “My very friendly colleague”, said Adorno remembering one occasion where he judged correctly that jazz fans are more commonly found in the city rather than in the country, “preferred to regard me as a medicine man rather than make room for something that lay under the taboo of speculation” (ibid., 350).

Wittgenstein's method of assembling reminders, that is, of collecting and discussing examples of language use, actual, but also fictitious, in the effort to inspect the possibilities of phenomena (PI 90), has very much influenced research in the social studies of science. The suggestion made, however, in that area was to forget about possibilities and concentrate on actual description. Sociology, anthropology or ethnomethodology could provide accurate accounts of phenomena which would replace the ones invented. Could the same be suggested in relation to Kuhn's 'made up' sociology? Could we recommend that science studies substitute an accurate sociology for the 'made up'? And could this be a continuation of Kuhn's project in *Structure*? In the next section I will consider the analogy between Wittgenstein and Kuhn in relation to this issue.

4 Substituting a Disciplined Sociology of Science for the "Made Up"

In this section, I want to examine what it would mean to substitute a disciplined, "properly" conducted professional sociology of science for the "made up" supplied by Kuhn. As noted, a similar substitution has been suggested in the science studies literature in relation to Wittgenstein: the substitution of Wittgenstein's fictitious natural history by an accurate one provided by sociology or anthropology. I consider this substitution relevant to the issue discussed here because I take Kuhn's use of examples from the history of science to be similar to Wittgenstein's use of actual and imaginary examples of natural history. In (Kindi 2005) I argue that just as Wittgenstein attacks an essentialist idea of meaning by bringing forward the multiplicity of ways language is and can be used, Kuhn attacks an essentialist idea of science by bringing forward the multiplicity of ways science has been and can be practiced.

Obviously, there are differences between the two thinkers. First, unlike Wittgenstein who often made use of imaginary examples of alternative practices, Kuhn uses examples from his historical research. His recourse to fictive cases, which is very limited compared to Wittgenstein's, only shows, I think, that he does not treat history as evidence for his model. Second, as I pointed out earlier, Kuhn's "made up" sociology is not actually invented -as it is the case with some of Wittgenstein's examples-, but based solely on his personal experience (so, "made up" should be taken in the sense of not professionally conducted). In this respect it resembles more Wittgenstein's reminders and less his imaginary examples. Third, Kuhn's examples, even when fictive, are not as bizarre as Wittgenstein's. The reason, I think, is that Kuhn's aim of showing the differences in the practice

of science would not have been served by extravagant examples of strange practices. For one, he would have to show that these strange practices relate to science. And second, the difficult thing for Kuhn was to show that what is taken to be familiar from the history of science is actually very different. Recourse to strange imaginative examples would not have contributed to shaking the deeply and solidly entrenched belief that science was always a monolithic, cumulative enterprise. Despite these differences, however, there remains the similarity of referring to particular cases and of not treating them as empirical data supporting a hypothesis. It would be relevant then, I think, to consider what has been discussed in relation to the substitution of Wittgenstein's fictitious natural history by an accurate one in order to consider what it would mean to substitute a professional and actual sociology for the made up.

One place where Wittgenstein refers to natural history and fictitious natural history is in the second part of the *Investigations*: "We are not engaged in natural science, and not even in natural history—since we can also surely provide fictitious natural history for our purposes (PI II, xii, §365). Wittgenstein's use of fictitious natural history and imaginary examples of deviant behavior and exotic tribes which range from the slightly different to the bizarre, has been variously interpreted. In David Bloor's view (1983), Wittgenstein's use of such history is just unfortunate. He finds Wittgenstein's project of showing the embeddedness of our concepts in our social practices badly executed: fragmentary, speculative and incomplete, and he assumes the responsibility of amending the situation. He intends to supply the "badly needed empirical material" (Bloor 1983, p. 49), knowingly going against Wittgenstein's own express repudiation of such an empirical project. Wittgenstein's concerns, he says, "are not only amenable to empirical study, but positively cry out for such an approach" (ibid., 182).

Similarly, Michael Lynch, in his paper "Extending Wittgenstein: The Pivotal Move from Epistemology to the Sociology of Science" (1992), juxtaposes ethnomethodological studies of work in the sciences (ESW) to Bloor's sociology of scientific knowledge in order to recommend the former as a more adequate empirical extension of Wittgenstein's later philosophy. Ethnomethodological studies, Lynch says, "is not a move into empirical sociology so much as an attempt to rediscover the sense of epistemology's central concepts and themes" (ibid., pp. 257–258). This means, practically, that ethnomethodologists study and describe how certain terms like proof, giving reasons, following instructions, observing or explaining, feature in concrete epistemic activities. Lynch mentions Garfinkel and Sacks whose projects involve the so-called troublemaking exercises (ibid., 257), i.e. disrupting the ordinary work of

scientists with questions in order to bring out what is going on.

Lynch does not want Wittgenstein's "imaginary ethnography" to be replaced by an empirical one à la Bloor. Yet, he thinks there is room for empirical studies that extend Wittgenstein's philosophy. These empirical studies which investigate key epistemological concepts, like rationality, rules, agency, etc., as being integrated in mundane practical activities of scientists, are seen as helping to realize the Wittgensteinian aim of perspicuous representation. According to Lynch, Wittgenstein devised real and imaginary cases to articulate a perspicuous representation of our grammar and ethnomethodological studies could be taken to be, in Garfinkel's terminology, "aids to a sluggish imagination" (ibid., 257). He believes that we cannot always give perspicuous representations of certain concepts so, by conducting certain experiments—like Garfinkel's for instance-, we bring into relief aspects of the phenomena that remained invisible. Lynch, just like Bloor, admits that these empirical projects are not in line with Wittgenstein's express claims, but nevertheless derives their legitimacy from Wittgenstein's philosophy and insists that they legitimately extend it.

Two philosophers have dealt explicitly with this replacement of Wittgenstein's fictitious natural history by an actual one: David Cerbone (1994) and Michael Friedman (1998). Both point out that this move constitutes a misinterpretation of Wittgenstein's philosophy acknowledging, though, that those who propose it know this already (see Bloor 1983, p. 5 and Lynch 1992, p. 217 n.4). David Cerbone has a particular understanding of Wittgenstein's imaginary scenarios, namely that they are meant to enlighten us about the character of our own lives and concepts, to make us appreciate their naturalness, make us "recover own familiar concepts as our own and to see the extent to which our (form of) life is inseparable from them" (Cerbone 1994, p. 178). Based on this he claims that Bloor's project is either otiose or misplaced. It is otiose because, in some cases, the demand for replacement can be met but is superfluous. And it is misplaced because, in cases such as the example of the strange wood-sellers, it is unclear what it would mean to replace an indeterminate scenario by an empirically accurate one. Wittgenstein's point, according to Cerbone, is exactly to show that "we are confused in our attempts to envisage such beings" (ibid., 179).

Michael Friedman's concerns are similar. He begins by noting the disparity between Wittgenstein's and SSK's (Sociology of Scientific Knowledge) philosophical agenda. He claims that Wittgenstein "*only gestures* towards [the] 'mundane' facts of practical social life" (emphasis added, Friedman 1998, p. 261), does not "marshal evidential support by instances, as it were, for any kind of generalized

picture of language—whether empirical or philosophical (ibid., p. 253)—and does not advocate any ethnological theorizing because this involves an outsider's (to the cultural system) point of view which cannot account for the normativity of the rules and practices studied (ibid., pp. 263, 265). Friedman insists, *pace* the SSK practitioners, that their philosophical agenda of reducing normativity to social facts is harmful to their empirical achievements and urges that philosophical and empirical investigations be kept apart, the way Hume did it (ibid., p. 268–269). His concern is that by adopting a naturalistic point of view we cannot take reflective responsibility for "the normativity of our most fundamental norms and standards without which we could not think at all" (ibid., p. 265). According to Friedman, the important thing is that we should not adopt an outsider's perspective putting our own cultural system on a par with alternative ones. Once we do that, the internal naturalness and binding normativity of our system dissolves (ibid., p. 263) and we are left with simply comparing, from outside, diverging social conventions. The professions of responsibility that are expressed by the SSK practitioners are of a different nature and "ring hollow" to Friedman's ears (ibid., p. 264). These practitioners may insist that we are responsible for our intellectual standards since we construct them, but this kind of responsibility does not imply commitment since it is pronounced from an external and detached point of view. Friedman, then, is opposed to the replacement of Wittgenstein's fictitious natural history by an empirically accurate one. His view seems to be that philosophers do philosophy from within the practice (preserving normativity, assuming responsibility, etc.) and empirical scientists conduct their research from without. Both he and Cerbone have a particular interpretation of Wittgenstein's use of imaginary examples. They think that these examples are used to explore the limits of language from within.¹⁵ They believe that we are committed to or bound by the language we have and we cannot ask, as an outsider would, -a social scientist, for instance-, why these norms and standards should govern our practice. So, philosophy and social science should be kept apart. Philosophers should refrain from trying to explain normativity and social scientists should avoid trying to deny traditional philosophical concerns such as whether there are context free norms of rationality (ibid., p. 244).

Given this discussion about Wittgenstein, there are two issues as regards Kuhn: one, whether we should substitute a disciplined sociology of science for Kuhn's amateurish one

¹⁵ This interpretation of the use of imaginary examples by Wittgenstein is contested by other scholars who argue that Wittgenstein did not use these examples to show their meaninglessness but rather to show that alternative concepts to the familiar ones are possible (see Forster 2004, p. 159).

in the way thought in relation to Wittgenstein's method, and two, whether the science studies professionals should be seen as continuing Kuhn's project in *Structure*. If we place the first question in the context of Friedman's discussion of the issue, we would have to ask, first, whether Kuhn had a philosophical agenda in *Structure*. If he had, that is, if he was committed to the normativity of the practice of science which is not supposed to be reduced to the social facts that preserve it, then, obviously, substituting empirical studies for what he did would be inappropriate. Friedman is not very clear, however, as to what Kuhn did. On the one hand, he thinks that Kuhn is doing something similar to Carnap, namely, relativizing standards of rationality, but on the other he brings Kuhn closer to the defenders of SSK by saying that "Carnap plus Kuhn equals the philosophical agenda of SSK" (ibid., p. 251). What he means is that Carnap relativized the notions of rationality and correctness and Kuhn offered the emphasis on the social factors responsible for the normative ideals and conventions. Subsequently, the SSK agenda undertook to reduce normative ideals to social factors and thereby combat traditional philosophy of science. If the SSK agenda involves both an empirical and a philosophical component, and Carnap is the pure philosopher, it follows that Kuhn offered the empirical part. Friedman thinks, I take it, that there is no tension between Kuhn and the social studies agenda: the former gave rise to the latter. This may be true genetically speaking, but it does not imply that Kuhn and science studies professionals engage both in empirical research.

In my view, Kuhn's project in *Structure* is motivated by philosophical, and one might say, normative concerns, namely, to properly understand science, and in that sense it cannot be identified with sociological and anthropological accounts in the science studies literature. Kuhn's recourse to empirical facts, either historical or sociological can be seen, as I have explained above, as offering the historicized conditions of the possibility of science. Delving into the intricacies of specific historical and sociological cases can, in addition, be seen as illustrating the different ways science is and has been practiced. We would then have a perspicuous representation of the concept of science but not in the sense that Lynch understands it. We will not, that is, be uncovering some hidden structure using experimental techniques to probe disclosures from the scientists which lead to discoveries (a constructive effort), but we would have the exposition of a variegated, open concept of science (a destructive effort).

My answer, then, to the second question above, *pace* Friedman, is that the science studies literature, so far as it aspires to be scientific, does not continue Kuhn's work. Substituting a professional sociology for Kuhn's 'made-up' would not fit in with what Kuhn was doing. Kuhn's aim

was to reach a better understanding of what makes science possible which would also undermine and bring down an idealized conception of it. Just like Wittgenstein, he was combating a philosophical idol, an essentialist idea of science, and he was doing it by philosophical means trying to preserve standards of normativity. What does this answer imply for the professional sociology of science? Obviously what professional sociologists and ethnomethodologists are doing is extremely valuable but they do not continue Kuhn's project. He recognized that himself when he said that the gap between what he was expecting after the *Structure* and what the social studies of science provided was vast (Kuhn 1983, p. 29). He was looking forward to a sociology of knowledge that would account for its special character and he was getting social studies of science concentrating on material interests and socio-economic situations. He attributed the problem to the background of the social scientists—that they lack the training to study the technical work of scientists- but also to the increasing realization that science is not an autonomous enterprise since it is affected by what is going on in society. Ironically, the problem was aggravated by the fact that the social studies of science that developed partly out of Kuhn's work, endorsed the so-called symmetry principle, according to which all kinds of beliefs, the ones deemed rational and irrational, should be accounted for by the same type of causes. This has obviously eroded the normative, epistemic privilege of scientific knowledge and made it impossible to have what Kuhn expected. Kuhn, just like Friedman, insisted that "the status of knowledge is in no way reduced when knowledge is seen as social" (ibid.) and, so, could not approve of the social studies agenda. Again like Friedman, he thought that his enterprise would fail, if he were to accept the reduction of knowledge to social facts (ibid.).

5 Conclusion

As we saw, Kuhn sketched a sociology of science by considering the cognitive practices of the scientists. It was "made up" by academic standards but still valuable as it was striving to offer insights into what is special about science. Kuhn, subsequently, invited and expected sociologists to reach cognitive conclusions by drawing on sociological data. Kuhn's hope was that by studying the scientific practices, the training of scientists and their values, sociologists would be able to explain the special character and efficacy of scientific knowledge. Questions about the cognitive content of science were always a major concern of his: "Why the special nature of group practice in the sciences has been so strikingly successful in resolving the problems scientists choose? What is it about

what scientists do ... that makes their output knowledge?" (Kuhn 1983, p. 30). The aspiration he had never materialized. The disciplined sociology he was looking forward to did not concern itself with the questions Kuhn had and certainly it could not replace his own approach which, despite its historical and sociological observations, had a clear philosophical bend. In fact, it is not at all clear whether Kuhn's expectation to have sociology account for what is distinct about science could ever be met. As Friedman has pointed out, the complete reduction of the rules that constitute the practice of science to the social facts that underpin it, eradicates the normativity of the practice and what is special about it. Certainly for Kuhn philosophy was not an armchair affair completely detached from empirical research. He made use of empirical material but he subjected it to his philosophical concern of showing, in a non-evidential manner, a diversified picture of science much like Wittgenstein's way in relation to language, i.e., by assembling reminders and surveying a wide range of possibilities. Other philosophical concerns may call for a different use of empirical data but this is a much wider issue that goes beyond the limits of the present paper.

Acknowledgments I would like to thank Rogier De Langhe and the two anonymous referees for their insightful criticism and much valued help.

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