“Kuhnian lessons for the social epistemology of science”

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Abstract. Kuhn’s analysis of the structure and function of the scientific community has been recently re-interpreted as a seminal contribution to the so-called *social epistemology of science.* Kuhn’s social epistemology should be considered as part of a normative-descriptive philosophical framework in which epistemological, historical, sociological, and psychological elements are interconnected. In this chapter, I will compare Kuhn’s seminal insights with two contemporary approaches to the social epistemology of science, namely: the development of idealised formal models of the scientific community and the use of qualitative studies for philosophical purposes. On the one hand, these contemporary approaches to social epistemology may be regarded as developing some of Kuhn’s views in new and exciting ways. On the other hand, however, it is still not entirely clear which kind of general philosophical ‘image of science’ they are contributing to. This chapter, therefore, aims at illuminating how analysing some of the contemporary debates in social epistemology through the lenses of Kuhn’s philosophy may recast under a new light the issue of the value of the study of the social dimension of scientific research for general philosophy of science.

1. Introduction

Kuhn puts the scientific community at the centre of his historical model of the progress of science he developed in *The Structure of Scientific Revolutions* (Kuhn 1996) and in other writings. Such a model describes the historical development of science as characterised by the alternance of relatively stable periods of cumulative normal science punctuated by occasional scientific revolutions. It is important to stress that what makes normal science possible is the endorsement of a dominant paradigm by the members of a scientific community. Scientific revolutions are also a community affair: they occur when the community’s consensus upon the dominant paradigm cracks and are resolved when the

community accepts a new paradigm and restores the normality of scientific research. The role of the scientific community within the Kuhnian model, therefore, cannot be underestimated. For a long time, however, philosophers failed to appreciate, or even notice, the community-based character of Kuhn's philosophy.

Ironically, the Kuhnian account of the social dimension of science, much dismissed or ignored by philosophers, ended up influencing the development of the sociology of science and STS, from which Kuhn actually distanced himself. He explicitly rejected the most extreme positions of the so-called Strong Programme, to the point of defining them "an example of deconstruction gone mad" (Kuhn 1992, in Kuhn 2000:110). By considering the social dimension of scientific research, in fact, Kuhn was not aiming at reducing scientific knowledge production to social construction, or at discarding the idea of scientific rationality. Mainly because of the deep-seated individualism of traditional philosophy of science, however, this aspect of the Kuhnian project was grossly misunderstood.

Many of the philosophers of that time defined scientific rationality as a set of universal rules that any (ideally) rational individual could have access to. Kuhn aimed at rejecting such an individualistic and 'algorithmic' conception of rationality. In the postscript to the second edition of *Structure*, he claimed that disagreements in science are not solved in ways that resemble mathematical proofs, because "[t]here is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision" (Kuhn 1996:200). In other writings he further elaborated on this point.

> "The search for algorithmic decision procedures has continued for some time and produced both powerful and illuminating results. But those results all presuppose that individual criteria of choice can be unambiguously stated and also that, if more than one proves relevant, an appropriate weight function is at hand for their joint application. Unfortunately, where the choice at issue is between scientific theories, little progress has been made toward the first of these desiderata and none toward the second. Most philosophers of science would, therefore, I think, now regard the sort of algorithm which has traditionally been sought as a not quite attainable ideal. [...] Even an ideal, however, if it is to remain credible, requires some demonstrated relevance to the situations in which it is supposed to apply." (Kuhn 1977:326)

Rather than discarding the idea that science is a rational enterprise, Kuhn sought to formulate a non-algorithmic and context-sensitive scientific rationality. He insisted that his own project was "an attempt to show that existing theories of rationality are not quite right and that we must readjust or change them to explain why science works as it does". He also suggested that to consider such an attempt as "a defence of irrationality in science" would be "not only absurd but vaguely obscene" (Kuhn 1970; in Kuhn 2000:159).

Since Kuhn did not believe that an objective weight function for criteria of choice can be established, so that every rational individual would apply the same criteria in the same ways, he regarded the tendency to "treat groups as individuals writ large or else individuals as groups writ small" (Kuhn 1993, in Kuhn 2000:241) as a category mistake. In his view, the decisions that a scientific community makes are more than aggregations of individual choices. The study of scientific rationality, therefore, ought to be studied at the group level.

Only in the past few years, Kuhn's approach has begun to be characterised as an early version of so-called *social epistemology of science* (Wray, 2011). Social epistemology of science seeks to understand how social structures serve the genuine epistemic ends of a scientific community and of its individual members, and investigates how such structures could be deliberately designed and optimised. This field is rapidly growing, prompted by philosophers' renewed interest in the institutional and social dimension of scientific research, now regarded not as a mere 'external' factor but, rather, as an important element that deserves philosophical scrutiny.

In this chapter, I quickly summarise how the historical model of science, social epistemology, its underlying theory of collective rationality, and sociological and psychological elements are all interconnected within the Kuhnian philosophical framework. I then discuss two contemporary approaches to social epistemology, namely: the development of idealised formal models of the scientific community and the use of qualitative research methods for philosophical purposes. I will explain what these approaches share with Kuhn's social epistemology, but also under which respects they complement, or even depart from, his original views. Although these contemporary approaches to social epistemology contribute to develop in new and exciting ways some of Kuhn's early insights, it is still not entirely clear which kind of general philosophical 'image of science' they are contributing to. This chapter, therefore, aims at illuminating how analysing some of the contemporary debates in social epistemology through the lenses of Kuhn's philosophy may recast under a new light the issue

of the value of the study of the social dimension of scientific research for general philosophy of science.

2. Social epistemology, collective rationality, and social psychology

Kuhn's interest towards the philosophical study of the scientific community predates the publication of *Structure.* In his (1959), he described the scientific community as endemically pervaded by an *essential tension.* Within the scientific community, the striking majority of scientists have a rather uncritical attitude towards their own research tradition, in the sense that they will engage in the scientific problem-solving activity without questioning its fundamental theoretical and methodological presuppositions. A few of them, however, will display a more divergent, critical, and iconoclastic attitude, which may lead them to explore alternative approaches. As mentioned in the introduction, this divergence often stems from a different application of the same scientific standards: even though every member of the scientific community agrees on the set of minimal requirements of scientificity a theory must meet, not all of them 'weigh' and rank such requirements in the same way (see Kuhn 1977).

The essential tension is linked to the theory of scientific rationality Kuhn was trying to develop. At the individual level, none of the scientists could be said to be 'irrational' for weighing the same standards differently (i.e., by preferring fertility over simplicity, or the other way round) and assessing theories accordingly. Such individual variations maintain several options alive. For a community as a whole, in turn, it would not be rational to 'bet' everything only on one theory. Even the best confirmed and most promising theory may be wrong after all: investing all the community efforts on it is too risky and it may kill off too soon valid alternatives that, if developed, applied, and worked on, may result in success. Through a strategic distribution of its intellectual resources, in other words, a scientific community can 'hedge its bets' in a way that best serves its collective epistemic ends. While it makes sense that the majority of scientists endorse the most promising theory, to reduce the risk of failure it is also necessary that *some* of them work on less developed theories and explore alternatives. In Kuhn's account, in short, the consensus bestowed by a scientific community on a dominant paradigm is not 'strong' or unanimous: internal dissensus is the silent side of the manifest consensus, it guarantees diversity of options and, therefore, the 'survival' of a scientific community. This is, in a nutshell, the so-called *risk spreading argument* (Kuhn 1977:332; see also Rueger 1996; D'Agostino 2005, 2010), which represents the normative element of Kuhn's description of the scientific community as well as the

backbone of the theory of collective rationality that he used as a reaction against the Received View.

Kuhn reinforced the descriptive element of his model by relying on the findings of some empirical sciences. That sociological or psychological descriptions could play any role whatsoever in epistemology puzzled Kuhn's contemporaries, who were used to what Kitcher (1992) defines as a 'post-Fregean' methodology, which reduces epistemological problems to questions about conceptual and logical analysis. By contrast, Kuhn and others established a way of conducting epistemological investigations in a more naturalistic way. In Kitcher's reconstruction, naturalistic epistemology is not even a novelty emerged in the second half of the XX century but, rather, the revival of the philosophical approach followed by modern thinkers such as Bacon, Descartes, Locke, Hume, or Kant. These pre-Fregean philosophers grounded their normative theories of knowledge and rationality on the psychological and anthropological studies of their times, and embedded epistemology within a general view of knowing subjects and of the social and cultural world in which they interact.

Many of Kuhn's views, as presented in *Structure*, followed such a naturalistic and 'post-Fregean' methodology and were in fact informed by the experiments on perception conducted by Bruner and Postman (1979), as well as by Piaget's developmental cognitivism and by so-called Gestalt psychology. Later on in his career, however, Kuhn rejected his early use of psychological theories about *individual minds* when talking about *group dynamics*. Clearly, scientific communities are constituted by individuals and individual psychology might be enough to explain the existence of diversity of articulations of the same paradigm, or the difference of weighting of evaluative standards due to personal preferences. What individual psychology cannot do, however, is explain how a scientific community remains relatively cohesive *despite* such individual differences and how fruitful exchange may persist among individual scientists with different preferences.

Since it amounts to a *social* epistemology, Kuhn probably realised that his view would be best grounded on *social* psychology.

> "My recourse has been exclusively to social psychology (...), a field quite different from individual psychology reiterated *n* times. Correspondingly, my unit for purposes of explanation is the normal (i.e., non-pathological) scientific group,

account being taken of the fact that its members differ but not of what makes any given individual unique". (Kuhn 1970, in Kuhn 2000:133–134)[1]

Social psychology is the study of how individuals' behaviours and choices are influenced by other individuals, as well as by their social and cultural context: following the simple formula established by Lewin, Heider, and Heider (1936), 'behaviour' is equal to the product of the influence of a 'social situation' over an individual. It must be noticed that, in this equation, an individual is as important a variable as the social situation, which means that behaviour is not fully determined by social forces alone. This leaves ample room for individual behavioural variations within the same social context.

Although the first systematic studies of social psychology were conducted between the end of 19th and the beginning of the 20th century, the whole field regained momentum with the analyses of conformity aimed at explaining the rise of nazi-fascistic regimes in Europe (Sherif 1936, Ash 1952). While the analyses on conformity quickly led to a number of studies on obedience and aggression, in the final decades of the 20th century, and thanks to the emergence of cognitive psychology, social psychology began to focus more on the influence of social factors onto cognition. What social-cognitive psychologists aimed at was a finer understanding of how social structures influence not only behaviour or attitudes, but also the very ways of perceiving the world and reasoning about it. Social psychology, in other words, unveiled the impact of social influences on information selection and processing, memory, and judgement. Exemplary, under this respect, is the work of psychologists Kahneman and Tversky (1979), who demonstrated how actual people's decision-making processes are substantially different from the idealised 'perfectly rational agents' postulated by so-called expected utility theory, and often run against the basic rules of logic and statistics. Incidentally, psychological research such as Kahneman and Tversky provides arguments in favour of the idea of modifying or even abandoning idealised theories of rationality, but without abandoning the idea of rationality itself.

Kuhn's social epistemology, in a sense, can be grounded on many of these different trends of social psychology. It is worth noticing, for example, that in 1961, a year before the

[1] In the original passage, Kuhn claims to actually prefer the term 'sociology' to 'social psychology'. It is not known whether this was due to a matter of personal preferences over terminology, or whether, instead, some deeper methodological concern was going on. It may be the case that Kuhn actually relied on various elements drawn from *both* sociology *and* social psychology. While the influence that social scientists like Max Weber may have had on Kuhn has been recently highlighted by Mladenović (2017), other parts of his framework could be illuminated and better explained by social psychology.

publication of the first edition of *Structure*, Kuhn delivered a talk in which he described the commitment to a dominant theory as a 'dogma' that scientists working in a mature science adhere to. The talk was eventually published a year after *Structure* (see Kuhn 1963), although Kuhn decided to retract his 'paradigm-as-a-dogma' definition. As argued by Reisch (2019), while such a retraction was a response to the numerous criticisms received after the talk, what Kuhn gave up was only a controversial term (namely, 'dogma'), but not its underlying idea: numerous passages of *Structure*, in fact, define the concept of a paradigm as something akin to a dogmatic ideology.

The 'paradigm-as-a-dogma', however, does not influence only scientists' attitudes and behaviour, but also the very way in which they perceive the world. Kuhn was one of the first philosophers to acknowledge how a dominant 'ideology' rules the research activity of a scientific community and, for this reason, he was accused of irrationalism and mob-psychology. While social cognitive psychology has uncovered the social and cultural mechanisms driving prejudices, fallacies and bias, many feminist philosophers of science have persuasively argued, and without being charged of 'irrationalism', that the same mechanisms may influence how scientists collect and interpret data, thus creating prejudices and bias in the scientific community. It is even possible to regard Kuhn's work as providing a methodological and conceptual basis for the later developments of feminist philosophy science (Longino 2003), which was capable of recognising the full potential of social cognitive psychology for a theory of science.

Finally, it is crucial to highlight how Kuhn's social epistemology, collective theory of rationality, and socio-psychological approach relate to his historical model of scientific progress. Clearly, scientists with a dogmatic attitude are normal scientists, whereas divergent thinkers become more prominent during times of crises and revolution. As already specified, however, the essential tension between the two kinds of scientists is a feature of epistemically well functioning scientific communities at every stage of scientific development, that is both during periods of normal science and during revolutions. By not questioning many of its fundamental aspects, normal scientists articulate and refine the dominant paradigm in order to apply it towards the resolution of a wider class of problems. By questioning the paradigm, the divergent scientists help the community to hedge its bets, work on the development of potential alternatives, and guarantee a 'reserve tank' of theories and methods to be used in cases of crisis, when the consensus towards the dominant paradigm begins to crack and new solutions start to be seeked out. Without the work of the iconoclasts, it would be difficult to

understand where a new paradigm comes from. Kuhn's social epistemology provides an answer to this worry: a new paradigm comes from *within* the scientific community itself.

For Hoyningen-Huene (1992), within the Kuhnian framework, historical, epistemological, and sociological factors are all interconnected: Kuhn showed how "the history of science already determines, among other things, the realm of questions that can, in a sociological or philosophical perspective, be sensibly asked with respect to science" (Hoyningen-Huene 1992:490). The interrelation of historical, epistemological, and socio-psychological elements in Kuhn's philosophy could also be described in terms of *explanatory relations*. By looking at history, Kuhn discovered that science does not grow in a cumulative way and that long periods of normal science are punctuated by revolutionary breaks, in which a new paradigm replaces the other. How such changes are possible and where new paradigms come from are issues explained by the model of the scientific community internally governed by an essential tension. The existence of individual variations that allow the emergence of the essential tension, in turn, can be explained *via* social psychology. In short: historical phenomena are explained by social epistemology and the theory of collective rationality, which are in turn explained by social psychology.

3. What do formal models of the scientific community model?

Several social epistemologists borrow methods and techniques from the social sciences to develop formal models of the scientific community. The first formal models of social groups were developed in disciplines such as economics and relied on analytical and mathematical tools, such as the game-theoretical framework. Contemporary social scientists often make use of Agent-Based Models (ABMs), computer-based simulations that explain social macro-phenomena as emerging from the micro-level consisting of collections of individual agents, the rules governing their behaviour, and their interactions (Axelrod 1997; Gilbert and Terna 2000). One the first and most influential ABM in social science, for instance, explained how, in choosing a residence, incentives and perceptions at the individual level lead to the phenomenon of urban ethnic segregation at the collective level (Schelling 1971).

The evolution of formal modelling in social epistemology mirrors the evolution of formal modelling in the social sciences, going from the use of analytical-mathematical frameworks to computer-based simulations. One of the first formal socio-epistemological models, for instance, was an analytical framework developed by Kitcher (1990). Kitcher aimed at correcting a fatal limitation of Kuhn's essential tension, namely the "mismatch between the demands of individual rationality and those of collective (or community) rationality" (Kicher

1990:6). While the scientific community as a whole ought to maintain the essential tension and spread the risk, being an divergent thinker and working at the fringes of the scientific community, with the risk of remaining unacknowledged or even ostracised, would be undesirable and just irrational for any individual scientist. In Kitcher's model, scientists are represented as aiming not only towards their community's epistemic ends, but also at 'making a personal profit' (i.e., gaining recognition, prestige, and career advancement). The model shows how, sometimes, the chances of an individual scientist's expected utility gets higher when choosing a research strategy with a lower probability of success: in a less crowded and less competitive field, it could be easier to make a relevant contribution after all. This way of dividing the cognitive labour among credit-seeking individuals allows the community as a whole to maintain internal pluralism and, therefore, to preserve the essential tension.

More recently, philosophers have begun to employ computer-based agent simulations not only to reframe the issue of the division of cognitive labour (Weisberg & Muldoon 2009), but also to study the communication structures of epistemic communities (Zollman 2007, 2010), the mechanism of knowledge acquisition (Borg et al 2018, 2019; Grim 2009; Kummerfeld and Zollman, 2016), and various other topics. These idealised models of the scientific community also aim at providing normative claims on how to design epistemically optimal scientific communities. Such an approach resembles Kuhn's descriptive-prescriptive philosophy. Indeed, as pointed out by Politi (2021), an ideal argumentative line connects Kuhn's generalised description to Kitcher's analytical framework and some other recent socio-epistemological ABMs, with each new model attempting to solve some of the problems raised by the previous ones.

Despite the shared interest towards the study of scientific communities, there is an important difference between the Kuhnian approach and the formal modelling in contemporary social epistemology. Namely, the reliance of the latter on so-called *methodological individualism* (MI), that is doctrine for which social phenomena are explained as a consequence of individual actions.

MI is particularly evident in the analytical frameworks used in classical economics, in which every single agent follows the rules of traditional Rational Choice Theory. ABMs, by contrast, introduce heterogeneous agents (i.e., agents that follow different rules) and may therefore account for more complex dynamics. However, this does not make them any less reliant on MI, inasmuch as the macro-level of the social phenomena they represent is completely reduced to the micro-level of the individual agent. By doing so, some argue, ABMs risk leaving out factors that are relevant for the kind of systems they aim to model

(Epstein 2011). Indeed, one of the current foundational debates in the social sciences is about whether the computer-based simulations of social phenomena are necessarily connected to MI. The debate is far from a resolution: while some argue that the link between ABMs and MI is neither necessary nor desirable (Marchionni and Ylikoski 2013; Zahle and Kincaid 2020; Kincaid and Zahle 2022), others argue that such a link is indeed inescapable and fruitful (Bulle and Phan 2017; Di Iorio and Chen 2019). As Manzo (2020) notes, part of the problem with this debate is that its terms are not always clear, with different people holding a different interpretation of MI and assessing its relations with formal modelling accordingly. Indeed, MI may be defined in different ways and its supporters may end up holding any of its many weaker or stronger versions (Udehn 2001).

The version of MI that might be of particular interest for a discussion about the socio-epistemological models is the 'trans-situationality', or 'context-free' character, of their agents' rationality. As Bulle and Phan (2017) explain, the agents modelled by computer simulations make their choices by always following the same set of abstract rules that have little or nothing to do with how real people behave in a social context. ABMs, in other words, only provide formal and 'empty' mechanistic/individualistic explanations of social phenomena. This means that "from the point of view of the phenomenal world [the symbolic system of an ABM] 'speaks' of nothing" (Bulle and Phan 2017:332). In their view, however, this is not a limitation of ABMs. For Bulle and Phan, the 'trans-situational' character of ABMs allow for their generalizability and transferability. The mistake is to believe that the full and once-and-for-all explanation of social phenomena is exhausted by the simulations run on an ABM. Rather, the 'empty' general mechanisms provided by ABMs are useful insofar as they are complemented by further context-dependent 'interpretations'.

Such an interpretation is what is missing in formal models of the scientific community. These models represent scientists as following the abstract and, indeed, 'trans-situational' rules of Rational Choice Theory, without complementing such a theoretical background with further interpretations on how actual scientists may make context-dependent choices. This is evident in Kitcher's MCR model, clearly borrowed from classical economics, in which profit-seeking individual agents make choices with the aim of maximising their personal gains, while the system as a whole will eventually reach equilibrium through an 'invisible hand'. Zollman's 'epistemic network' too is inspired by economics, most notably by the decision-making model based on past experiences and intersubjective communication proposed by Bala and Goyal (1998). The difference between the two models is that, since Zollman's agents are epistemically pure, the 'return' they attempt to maximise is not their

own personal gain, as in Kitcher's framework, but the reaching of the epistemic objectives of the scientific community. In the end, however, the *modus operandi* of the agents of both models is the same: maximising profit, being it epistemic or non-epistemic, by following a set of context-independent abstract rules.

To varying degrees, the same holds for the agents of more sophisticated socio-epistemological ABMs. What is common to many of them is that they represent the scientific community as composed by a collection of agents pursuing the same objectives. On the one hand, it is true that an ABM may have different kinds of agents, who adopt different strategies and pursue the same objective in different ways, and who may also be 'disturbed' by a number of factors (the spread of false information, 'biassed agents', and so on). Yet, on the other hand, their differences and variations notwithstanding, the models presuppose that all the agents agree on the objective to be pursued, on how to pursue it, and on how to recognise it when it has been reached.[2] Rather than being unproblematically assumed, such an agreement would deserve to be questioned and investigated further. Behind the apparent consensus among the members of the same scientific community, even among those who make the same decisions, there can be ample room for non-trivial individual differences in assessing and evaluating the aim of research and how it is conducted. Through time such differences may even contribute to the further development of science.[3]

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[2] To make an example, the ABM developed by Weisberg and Muldoon (2009) represent scientists as agents exploring an 'epistemic landscape' with the objective of discovering its peaks of maximum significance. Although different kinds of agents adopt different exploratory styles (for instance, 'mavericks' take more risks and explore the less known paths, whereas 'followers' prefer to work on consolidated grounds) they all agree on what counts as epistemically significant in their research. The authors do not find this particularly problematic: "An important and foundational debate in philosophy of science concerns the source of scientific significance. A classical perspective holds that some facts have intrinsic scientific significance. A radical alternative holds that all judgments of scientific significance are merely the result of dominant ideologies and other political and social forces that influence scientists and scientific consumers as much as anyone else. Moderate positions acknowledge both the social origin of much of what we take to be important in scientific knowledge, but also that some questions and answers have significance internal to the goals and structures of science. Our model makes no commitment about the source of significance judgments. It only requires that the community of scientists working on the same topic would make the same or nearly the same judgments about significance" (Weisberg and Muldoon 2009:229). However, even when scientists seem to make the same or 'nearly the same' judgments, they may do so on the basis of different interpretations and motivations. Such internal differences may remain tacit during the routine work of normal science, but they may nevertheless play a crucial role for the development of science. How consensus emerges in spite of these differences and when and how the internal diversity of the scientific community leads to a break in the consensus are important elements of science that are not represented in a model like Weisberg's and Muldoon's.

[3] Two observations are in order. The first is that not every agent-based model may face the problems discussed here (see, for example, Epstein 2014). In this chapter, however, I am not talking about ABMs 'in general', but about the ABMs *used by social epistemologists.* This leads me to the second observation. To my knowledge, only Kitcher explicitly acknowledged the methodological individualism of his model. He defended it by presupposing its validity instead of demonstrating it (see Kitcher 2000:S39). The subsequent development of more sophisticated socio-epistemological models has not led to a more sophisticated discussion about their fundations. The general impression is that formal social epistemologists often borrow such tools from the social sciences and apply them to social epistemology without reflecting upon the philosophical and methodological

The irony is that while Kuhn adopted a social epistemological perspective to reject an individualistic, rule-based, and context-independent theory of scientific rationality, current socio-epistemological formal models appear to support precisely the view Kuhn was arguing against. One could argue that this would be a problem only if fidelity with the Kuhnian perspective is sought. Of course, the argument that formal models of the scientific community must be faithful in spirit to Kuhn can hardly be made. The more general issue at stake here is whether these models represent how actual scientists reason. The discussion of the theory of rationality underlying the MI at the basis of the formal models of the scientific community therefore leads to the problem of the descriptive and representational power of such models.

Some philosophers have already argued that current formal socio-epistemological models lack an empirical calibration, which makes them not only inadequate to represent their target object, namely the actual scientific communities, but also insufficient as a basis for advancing normative claims (Martini & Fernández Pinto 2017; Thicke 2019). It must be wondered, however, whether ABMs and other kinds of formal models of social phenomena need to be more connected with their empirical targets after all. Many ABMs in the social sciences are not empirically validated either, but this is not always regarded as problematic because the function of such highly idealised models is not to provide *actual* causal explanations of complex social phenomena, but only to explore theoretically *possible* causal mechanisms. For example, the famous Schelling model showed how unproblematic individual choices taken on the basis of preferences and potential rewards may lead to ethnic segregation at the collective level. This does not exclude that, in actual cases, ethnic segregation is caused by clearer and more blatant racial prejudices. Similarly, it may be argued that the function of socio-epistemological models is not to describe how actual scientists in actual scientific communities make decisions, but only to provide 'how-possibly explanations' (Rosenstock, O'Connor & Bruner 2017).

To say that idealised models of the scientific community do not aim at actual explanations but only to how-possibly explanations, on the one hand, rescues them from the problem of their descriptive inadequacy. On the other hand, however, it poses the problems of

presuppositions of ABMs and formal modelling. Yet the presuppositions behind the construction of a formal model of the scientific community may become part of the philosophical theory that that model is contributing to develop (in this case, the MI of formal models of the scientific community may lead to an individualistic theory of scientific rationality). In short, the problem is not that socio-epistemological models rely on MI and therefore do not conform to the Kuhnian framework. The problem is that the MI at the basis of socio-epistemological models, and therefore the general perspective it provides to social epistemology, is not even analysed and discussed.

understanding, exactly, what how-possibly explanations are good for and, in case, of how to distinguish between relevant and irrelevant how-possibly explanations. For instance, Frey and Šešelja (2018) suggest that socio-epistemological formal models should not be regarded as the 'true' representation of actual scientific communities, but rather as formal tools to supplement and enrich some debates in the history and philosophy of science. Their function, therefore, is neither descriptive nor prescriptive, but it is rather 'exploratory', in the sense that they help with the exploration of theoretical issues. But even in this case, Frey and Šešelja argue, some sort of calibration and changes in initial parameters are still needed. This is so because current socio-epistemological formal models are far too abstract to be used in HPS debates and, when they attempt to explain some episodes in the history of science, they actually misrepresent the very case-studies they aim at explaining. More detailed historical reconstructions may offer insights about the actual size of a scientific community at a particular time, about its actual communicative structure, and so on. All these elements could be then parameterized in order to enrich the highly idealised ABMs.

While the history of science could help developing more refined ABMs, in the next section I explore a recent approach to social epistemology that may provide formal models with the element for a more realistic theory of scientific rationality.

4. Qualitative research methods for philosophy of science

A different approach to the social epistemology of science consists in the use of qualitative studies of research communities for philosophical purposes. From a methodological point of view, this is not a novelty. Sociologists and STS scholars have already established a strong tradition of 'laboratory studies', ethnographic analyses of the 'shared culture' of research labs (see, among the others, Latour and Woolgar 1986 [1979]; Lynch 1985; Knorr-Cetina 1995, 1999). Laboratory studies were conducted, to varying degrees, from a constructionist perspective. Their interpretive background theory is the idea that the validity of scientific results is shaped and determined only by contextual, cultural, and sociological forces (i.e. power-relations, political and economic interests, negotiations, and so on), with little or no role for genuine epistemic factors.

The real novelty consists in the emergence of a new approach in philosophy of science, which relies on the use of qualitative research methods for epistemological purposes and without constructionist connotations. As Nersessian and MacLeod (2022) argue, the fact that STS scholars of a social constructionist persuasion have used qualitative research methods

does not imply that the use of such methods by itself supports social constructionism. Philosophers may employ some of the same methods of STS scholars, but without necessarily sharing their relativistic and constructionist stance, and with the aim of providing novel empirical inputs to reframe epistemological issues. Such an approach may represent the missing link between highly idealised philosophical theories and the complicated reality of actual scientific research (Hangel & ChoGlueck 2023). The use qualitative research methods for philosophical purposes, therefore, represents a new development in the converging interactions between STS and philosophy of science, as well as a further step in the use of empirical methods within a naturalised philosophical framework (Wagenknecht, Nersessian, and Andersen 2015).

An example of this approach is the corpus of in-depth ethnographic studies conducted by Nersessian and her collaborators on a number of scientific laboratories over two decades. Nersessian's 'socio-cognitive approach' challenges the traditional social-cognitive divide, by showing how contextual and situational features (i.e. psychological, material, and social elements) shape properly epistemic and knowledge-producing practices. In particular, these studies uncovered the contextual, methodological, and cognitive obstacles to interdisciplinarity, as well as some of scientists' cognitive strategies to overcome them (see, as a synthesis of the work conducted by her team, Nersessian 2022). Another example is provided by Wagenknecht (2016), who has conducted a comparative analysis of two laboratories, in order to inform and refine philosophical theories about the division of labour, epistemic dependence, trust, and other factors necessary to group cooperation in science.

The use of qualitative studies for philosophical purposes poses foundational and methodological issues. One of the perceived issues with this approach is that, focused as it is on 'local' analyses, its results are not statistically valid. It is therefore unclear how context-specific qualitative studies may be used to support general conclusions. Qualitative philosophers respond by noticing that this problem is similar to the one raised by the use of historical case studies in philosophy of science. That historical case studies are not generalizable *strictu sensu* does not prevent philosophers from using them. They do however spark interesting debates on the historiographic methodology and on the meta-philosophical issue of the relation between history and philosophy (see, for example, Burian 2001, Chang 2012a, Kinzel 2015). Following the view of Chang (2012b) on the relation between particular episodes in the history of science and general philosophical claims, Masneures and Wagenknecht (2015) argue that qualitative case studies and philosophical theories are not in a particular-to-general relation but, rather, in a concrete-to-abstract relation. In other words, the

function of case studies is not to provide inductive support to a theory, but rather to show how the abstract theory works in an actual context. In any case, instead of simply borrowing empirical methods from other disciplines, supporters of the use of qualitative studies for philosophical purposes stress the need of reflecting upon the foundational issues of the methodology of social research.

In a nutshell: the use of qualitative research methods in philosophy of science is part of *descriptive* enterprise, but the descriptions may nevertheless play a *heuristic role* (in the sense that they may lead to the discovery of new philosophically interesting facts about scientific practice) and even a *normative role* (in the sense that they may challenge some of the existing normative theories and lead to the examination of other norms that are 'implicit' in the actual practice).

Even though he developed the model of the essential tension from occasional and unsystematic personal observations, Kuhn may not have been against the use of qualitative methods for the study of the scientific community. To explain how the many elements making up the disciplinary matrix of a scientific community could be uncovered and interpreted, he even described the activity of a philosopher of science through the Quinean metaphor of the anthropologist learning the language of a foreign community (see the 'Discussion' following Kuhn 1974, in Suppe 1974: 516-517). Empirical philosophers of science who use qualitative methods just take a step forward: they do not imagine themselves as ideal anthropologists, they just go directly into the field trying to interpret scientists' language and actions. Unlike formal models of the scientific community, therefore, this approach does not presuppose a theory of (ideal) scientific rationality. Rather, it looks at the *in situ* scientific activity with the aim of uncovering and explaining how actual scientists reason. In order to interpret what is observed in the lab or in the research group, philosophical ethnographies often rely on methods from empirical psychology. The use of methods from social cognitive psychology may be regarded as a concrete application of Kuhn's (sparse) invitations to recur to social psychology to reframe problems in social epistemology.

Another difference between formal modelling and the philosophy based on qualitative research is the 'size' of the object under study. Formal models provide idealised representations of so-called 'specialty-communities', with their agents standing for scientists working in the same research field or subfield, but who may be based in research institutions all across the world. Because of its local and contextual character, qualitative philosophy looks at much smaller units of scientific production, such as laboratories and research groups. This focus on smaller communities of scientists is consistent with Kuhn's view. Although he

is often regarded as being concerned only with much bigger specialty-communities, Kuhn acknowledged that scientific communities exist at different 'levels of generality' (i.e., the level of the community of physicists and biologists, the level of specialties and sub-specialties, down to the level of labs, departments, and research groups; see Kuhn 1996: 177).

Despite the differences in methods and in the size of the object of study, perhaps social epistemology in general would benefit from a stronger interaction, or even a convergence, between formal modelling and qualitative studies. On the one hand, although the results of qualitative case-studies could be used as part of the basis for making general claims, in and by themselves they cannot be generalised. On the other hand, as explained at the end of the previous section, one of the philosophical shortcomings of the formal models of the scientific community is their presupposing the validity of abstract and context-free theories of rationality. Qualitative studies may therefore inform the formal models of the scientific community with new and more realistic theories of rationality. In this way, the empirically-informed computer-based models of the scientific community would systematise the results of qualitative studies within a more general and formal explanation.

5. Kuhnian lessons for social epistemology

Kuhn developed a philosophical framework in which history determines the philosophical and sociological questions about science. Contemporary social epistemology has evolved into a number of different approaches and debates. If there is a Kuhnian lesson to be learned is that, instead of developing them in relative isolation with one another, these different strands of social epistemology may interact in fruitful ways. For instance, drawing from more detailed historical reconstructions, as suggested by Frey and Šešelja (2018), and from qualitative studies of actual scientific practice, as proposed in the previous section, may improve some of the current formal models of the scientific community, thus strengthening their epistemic functions. In turn, these improved models may allow the further exploration of theoretical and 'what-if' questions.

Of course, this is a purely speculative suggestion, in the sense that it is not always clear whether and how qualitative data about scientists' reasoning and collaborative strategies obtained through ethnographic studies could be modelled into a computer simulation. A reflection on the potential technical limitations of formal modelling should also be part of social epistemology. Nor should it be thought that qualitative studies supply everything formal models miss. As already discussed, ethnographic studies of science are conducted in

'small-size' scientific communities, such as laboratories and research. Because of their focus on *in situ* problem-solving practices, they focus on collaboration and coordination, rather than on rational disagreement. One may wonder whether something like an essential tension between dogmatists and divergent thinkers has actually been *observed* (or even whether it is *observable*) in small-size research contexts. Evidence for the existence of such an essential tension, perhaps, may be drawn from the study of larger specialty communities, usually conducted through the quantitative (rather than qualitative) methods followed by bibliometric analysis of the scientific literature.[4]

There is another Kuhnian lesson to consider. At the very beginning of *Structure*, Kuhn famously stated that "[h]istory, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed" (Kuhn 1996:1). Similarly, the analysis of the social dimension of science, which he regarded as a repository for more than 'external' factors, helped him challenge some received conceptions about scientific rationality. Even assuming that all the relevant factors of the actual scientific group activity could be drawn from historical, qualitative and quantitative studies, and then fed into a formal model of the scientific community, it remains to explicate which image of science such a model challenges, or else supports. This is the meta-philosophical issue of the relation between empirical descriptions, being them historical or sociological, and philosophy.

Such a relation became problematic for the same Kuhn who, later on in his career, seemed to abandon his early naturalistic outlook in favour of the analysis of the linguistic structure of scientific theories. Such a 'linguistic turn' has been deemed as regressive by some philosophers, such as Bird (2002). Others, by contrast, argue that this was not a 'bad' turn after all, or even that there was not even a 'turn' to begin with. Talking about his use of history in philosophy, for example, Kindi (2015) argues that Kuhn's aim was never to develop an empirical historical philosophy of science: all he needed was to take a look at some historical facts, not to learn all the little historical details, but to acquire the 'historical

[4] Interestingly enough, Kuhn would also agree with the use of bibliometric or scientometric methods for the study of the scientific community. Facing the problem of avoiding circularity in individuating a scientific community without a prior recourse on the paradigm that defines it, in the *Postscript* Kuhn suggested to use empirical methods to analyse the "formal and informal communication networks including those discovered in correspondence and in the linkages among citations" (Kuhn 1996:178). Kuhn referred to methods and techniques that were still not sufficiently refined and established and his proposal was criticised by the philosophers of his time, most notably by Musgrave (1971). For reasons of space I cannot delve into a discussion of scientometric studies, which however can be regarded as constituting yet another branch of contemporary social epistemology of science.

perspective' necessary to discard the received image of science and to develop an alternative philosophical model (see Kuhn 1992). Kindi's argument could be extended to the empirical studies of the scientific community: one needs to just take a look at actual communities of scientists, not to learn all the little social and psychological facts, but to acquire the 'social perspective' necessary to discard the received individualistic theory of scientific rationality. This would mean, however, to give up almost entirely the descriptive and empirical element that made Kuhn's 'descriptive-normative' approach so exciting in the first place. If philosophers of science only look at sociology and psychology to find what they need for their own 'perspective', then they could stop looking too quickly and focus exclusively on the development of their own abstract philosophical systems. Yet it is true that if they limit themselves to looking at sociology and psychology, then they would fall prey to the naturalistic fallacy and risk not developing any philosophical perspective altogether.

In my view, Kuhn's intellectual trajectory is neither characterised by two main periods separated by a sudden 'turn', nor by a strong a priori philosophical outlook with the descriptive and empirical part only playing an ancillary role. Rather, I regard Kuhn's mature focus on philosophical questions as an attempt to solve some of the issues arising from his early empirical study of science. For instance, if the historical records support a particular non-cumulative image of science, which kind of theory of meaning would be compatible with such an image? This is the sort of question Kuhn asked himself in his mature years. Similarly, if contemporary social cognitive psychology supports a particular image of scientific rationality, would a methodologically individualistic computer-based simulation best represent it? In short, rather than focussing only on the technical issues of the formal models of the scientific community, such as their robustness, contemporary social epistemology should also reflect upon the kind of philosophical framework such models contribute to establish.

Kuhn developed a model of the progress of science through revolutions and, at the same time, adopted a new way of practising philosophy. Contemporary social epistemology is not meant to confirm all the details of the Kuhnian model, but it may learn some lessons from the Kuhnian overall methodology. In particular, it may appreciate the fruitfulness of establishing links and interactions among its different branches and it may also expand the meta-philosophical reflection about the relation between empirical and formal studies of the scientific community and philosophy. In the end, however, contemporary social epistemology may also provide a valuable lesson to the Kuhnian scholarship. Kuhn relied on the empirical findings of his time to develop a social epistemology of science. Being acquainted with the

most recent developments of social epistemology, in turn, may provide a way to further develop Kuhn's legacy.

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