Book of Abstracts

"Thomas Kuhn and the 21st Century Philosophy of Science"

The Kuhn Centennial Conference

13 – 15 July 2022

Department of Philosophy and Centre for Reasoning

University of Kent, UK

Sponsored by the Aristotelian Society and British Society for the Philosophy of Science

Contents

Interpreting *Structure*

Alexander Bird (University of Cambridge, UK)

In this talk I examine three interpretations of Kuhn's philosophy of science, in particular as it is found in *The Structure of Scientific Revolutions*. These three are the Kantian interpretation, the Wittgensteinian interpretation, and the naturalistic interpretation. I examine how successful these are, both as interpretations of Kuhn and as philosophies of science in their own right.

Kuhnian Lessons for the Social Epistemology of Science

Vincenzo Politi (University of Oslo, Norway)

Kuhn's famous model of scientific progress was not his only contribution to philosophy. Kuhn also established a new way of practising philosophy of science, which draws philosophical norms from the description of historical case studies as well as from several findings in experimental psychology and sociology (Hoyningen-Huene 1992).

Kuhn's 'historical philosophy' has led to a number of discussions about the relation between history and philosophy (Kindi 2005; Roth 2013; Mladenovic 2007, 2017). Some of the recent debates on the methodology of historiography of science (i.e. Kinzel 2015) could be considered, in a sense, as having a 'Kuhnian root'.

Kuhn also anticipated many elements of so-called 'social epistemology of science' (Wray 2011). In his view, science is not the product of an individual mind, but an activity carried on by the members of a scientific community. Being the locus of the production of science, understanding the dynamics of the scientific community becomes crucial. In various works throughout his career, Kuhn explains that: the members of a scientific community possess a special knowledge (they are the experts); such a community is distinguished, or even isolated, from the non-expert public; and membership to the scientific community is acquired through a special training. While much of Kuhn's claims about the scientific community come from his personal experiences, the use of the recently developed tools for the study social groups (i.e. Agent Based Models, network systems, and big data analysis) in social epistemology could expand and refine his original philosophical project (DeLanghe 2013).

I argue that, in the same way in which Kuhn's historical philosophy has lead to consider the meta-philosophical problem of the relation between historiography and philosophy, so his view on the necessity of studying the internal dynamics of the scientific community should invite us to reflect upon the methods and tools employed by social epistemologists, as well as their value for philosophy.

In the first part of this talk, I show how some of the major socio-epistemological formal models of the scientific community represent a (more or less explicit) development of some of Kuhn's original ideas. In the second part, however, I explain in which way these models diverge from the Kuhnian approach due to two different but interrelated assumptions, namely: (a) traditional Rational Choice Theory, and (b) methodological individualism. In the third part, I examine the Kuhnian roots of the so-called 'qualitative approach' to philosophy of science (Wagenknecht, Nersessian and Andersen 2015) and I discuss how such an

approach may complement the development of formal models in social epistemology. In the fourth and final part of the talk I suggest how to further extend the qualitative social epistemology of science.

Rationality and Normativity in Kuhn's Conception of 'Normal Science'

Samuel Schindler (Aarhus University, Denmark)

Kuhn's philosophy of science was often received as promoting 'irrationality' by his contemporaries (and long after) (see Lakatos and Musgrave 1970). Kuhn strongly disliked and rejected this characterization (Kuhn 1970a, b). Although it is hard to deny that the critics were right in some respects (with regards to, e.g., paradigm change and incommensurability of paradigms, in particular), there are clearly aspects of Kuhn's work for which this criticism is unfair. For example, there is clearly a normative aspect to Kuhn's concept of 'normal science': scientists would get little (puzzle-solving) work done if they were to reject a paradigm every time they were to encounter an anomaly. So it is perfectly rational for scientists to pursue those puzzles that do not resist a solution with the means provided by the paradigm (Godfrey-Smith 2009, Schindler 2013).

In fact, Kuhn saw himself as engaged in making rational sense of science. As he put it: "I am no less concerned with rational reconstruction, with the discovery of essentials than are philosophers of science" (Kuhn 1970a, 236, see also Kuhn 1970b). Kuhn was keen to stress that science is "the best example of rationality that we have" (Kuhn 1970b, 144) and that it would "open the door to cloud-cuckoo land" to suppose that we could possess criteria of rationality entirely independently of our understanding of the "essentials of the scientific process" (Kuhn 1970a, 264). He therefore thought that philosophers' notions of rationality should be open to revision upon learning that "the development of science depends essentially on behavior that we have previously thought to be irrational" (Kuhn 1970b, 144).

Lakatos once famously decried Kuhn's view of scientific revolutions as "irrational, a matter of mob psychology" (Lakatos 1978, 178). Lakatos offered an account of the development of science in terms of his 'methodology of research programmes', which is often seen as rational alternative to Kuhn's account (Godfrey-Smith 2009). But is it? Kuhn himself pointed out several analogies between the two accounts and argued that Lakatos' account was in fact no better off in terms of rationality (Kuhn 1970b).

In this paper I will further expound Kuhn's view of how the history of science could help us develop and change our views of what's rational. Kuhn's concept of normal science is a prime example of this, but there are also others. I will also critique the common view that paradigms – which normal science activity presupposes – require a 'dogmatic' mindset in scientists (Popper 1970, Godfrey-Smith 2009) and argue that the acceptance of a paradigm is justified by its exemplars, and therefore can very well be rational. Finally I will probe Kuhn's claim that his notion of paradigm change wasn't less rational than Lakatos' view on the change of research programmes.

Revolutionary Analogies

Francesco Nappo (Politecnico di Milano, Italy)

One of Kuhn's central themes in *The Structure of Scientific Revolutions* is the role that analogy plays in scientific research. Kuhn considers this theme specifically in relation to his discussion of 'normal science'. Scientific training under a paradigm, according to Kuhn, comes with a capacity to extend the paradigm to new problem situations (1963:37). Such extensions are often not rule-based. They depend, in other words, not on the algorithmic application of some formalizable principle, but on a more visceral and hard-to-analyze capacity to recognize similarities between cases already covered by the paradigm and the ones not yet covered. This essentially analogical way of proceeding (which Kuhn often compares with the case of learning to solve the exercises at the end of a textbook) is at the heart of scientific research processes during normal science.

A theme that is less prominent in *The Structure,* and that this paper aims to analyze, is the role of analogy in 'revolutionary' times. J. Clerk Maxwell's analogy between hydrodynamics and electricity, which gave rise to the new 'field' conception of electromagnetic phenomena, and Darwin's analogy between breeding and nature, which gave rise to the hypothesis of natural selection, are two notable examples. In both cases, the analogical connections drawn by the scientists eventually became paradigms for, respectively, electromagnetism and biology.

Two conclusions emerge from considering revolutionary analogies in science – one more in line with Kuhnian philosophy and the other less so. Both are exemplified by the case-study of Maxwell's (1890) early electromagnetic work, which the paper will analyze in detail.

On the one hand, the fact that so many analogies became paradigms is partly explained by the fact that, as Kuhn (1963:43) stressed, they contained immediate indications as to their own extensions. As will be discussed, Maxwell was particularly aware of the need for new theories to come in the form of "physical analogies" for this reason. He criticized a purely mathematical approach to physics, claiming that, by casting an electromagnetic theory in terms of unembodied mathematical symbols, "we entirely lose sight of the phenomena to be explained [and] can never obtain more extended views of the connexions of the subject" (1890:155). Through an analogy with fluids, instead, the theory could display its 'extensibility' insofar as "the connexions of very different orders of phenomena may be clearly placed before the mathematical mind" (1890:156). These ideas are very much in line with Kuhn's emphasis on the fruitfulness of paradigms.

On the other hand, it is apparent that revolutionary analogies achieved the status of paradigm partly as the result of drawing from a common background of experience and training – one that rival scientific parties shared. For instance, hydrodynamics was well-known to defenders of the rival approach to electromagnetism (the one that eventually lost), based on action-at-a-distance. This fact puts pressure on Kuhn's idea that the choice between paradigms is (as the result of under-determination) often based on merely 'personal' or 'aesthetic' considerations, perhaps merged with vague criteria of theory choice (Kuhn 1972:98). As Maxwell's work in electro-magnetism exemplifies, some scientific works had greater success partly as the result of being capable of giving reasons that were *accessible* to the opposing factions – thereby 'bridging', so to speak, the incommensurability gap. Thus, an in-depth analysis of the role of

analogy in revolutionary times puts pressure on Kuhn's depiction of the emergence of paradigms as the result of a mere 'fight' between factions with incompatible standpoints. The upshot is that the distinction between 'revolutionary' and 'normal' science stands in need of a reformulation.

Kuhn and Logical Empiricism: Opposition or Continuity?

Joseph Bentley (University of Manchester UK)

Kuhn is one of the most influential and important philosophers of science of the twentieth century, in some ways eclipsing much of what came before him. This is in large part because of his theoretical innovations. But his significance is also a consequence of his historical role in the reorientation of philosophy of science away from Logical Empiricism, and towards a more historically and sociologically informed approach. Kuhn is often framed as the killer of Logical Empiricism. He invokes the specter of the Logical Empiricists as that which his new thoughts are a response to and rejection of. And it is true that Kuhn's historical methodology stands in marked contrast to some of the more technically-minded and formalistic examples of Logical Empiricist philosophy of science in the post-War anglophone world. However, this version of Logical Empiricism is not characteristic of the entire movement.

Within the Vienna Circle the more radical left-wing, in particular Otto Neurath and Philipp Frank, were heavily influenced by the works of French conventionalists like Duhem and Poincare, and especially Ernst Mach's historical studies of science. With this background, they pursued an historically and empirically informed approach to the philosophy of science which both methodologically and theoretically anticipates positions later made much more famous by Kuhn. My emphasis is on the question of theory-change, a topic that Kuhn and Frank both dealt with extensively. I will demonstrate not only a continuity in general approach, but some specific and noteworthy theoretical agreements. In particular, I emphasize the role of values and decision-making in the process of theory choice, as opposed to the desire for the application of an objective decision-calculus, and the instrumentalist conception of scientific theorizing that underlies it. I will also show how Neurath's response to Karl Popper involves similar the thoughts to those that Kuhn would later deploy to the same ends; that normal science is incompatible with falsificationism.

The intention is not to demonstrate any as-yet unrecognized influence of the Vienna Circle on Kuhn (although one potential link is discussed), but rather to suggest that the historical framing of Kuhn's relationship to Logical Empiricism is incomplete. Whilst Kuhn considered himself an opponent of the dominant strand of Logical Empiricism in the anglosphere of the 1950s, he was also working (if unbeknownst to him) in continuity with an earlier and ultimately less dominant strain of the Logical Empiricist movement. My claim then is that there is far greater continuity between the positivist and post-positivist approaches to philosophy of science, and that Kuhn is a clear example of this. To frame Kuhn as the killer of Logical Empiricism therefore perpetuates an unhelpful historical narrative.

Philipp Frank and Thomas Kuhn: From Worldviews to Paradigms in the Political Arena

Adam Tamas Tuboly and George Reisch (Hungarian Academy of Sciences, Hungary)

Thomas Kuhn used a variety of (unreflected) sources when writing Structure of Scientific Revolutions. One of his less noted sources was Philipp Frank: their personal and intellectual relationship is still a highly neglected theme among historians of philosophy of science.

Sustained attention to the sociology and history of scientific knowledge in Frank's American work will legitimately lead to comparisons with Kuhn's influential book, traditionally credited with inaugurating interest in the historical, sociological, and psychological study of science. This comparison is not abstract for Frank's mature philosophy of science, his activities on behalf of the unity of science movement, and Frank himself—the friendly, talkative fixture in and around Harvard Yard—belonged to the intellectual landscape in which Kuhn became a historian of science and began to write Structure at the first place.

Sharing a background in physics, an interest in the historical and social dimension of knowledge, a fatherly figure in Harvard president James Conant, an agenda as a public intellectual, and a commonly inherited Cold War atmosphere, Frank and Kuhn surely exchanged ideas and could be seen as differing mirrors of a slowly changing era. As both taught in Conant's General Education scheme for years, both saw the rise and fall of it. General Education at first was a reaction to various totalitarian regimes, to anti-intellectualist sweepings in Europe, a form of liberal education. The idea was to show students the intrinsic human and liberal values of the sciences, and thus citizens will be armored by such a synthetic perspective that empower them for a democratic life in "a free society". But after the Korean and during the Vietnam War, the political and educational life underwent substantial changes. Many students doubted the "liberal and free" picture of America, raised important doubts of governments—something had to be changed and the old humanistic catchwords lost their attractiveness. The revolutionary generation of the 1960s cannot be convinced to first immerse into the humanistic picture of science and society and then move on to radicalize.

In that atmosphere, whatever Kuhn's original aims and intentions were, Structure was read as enhancing a more radical and political understanding of the sciences and why members of the scientific community resisted the change. As American culture and politics embraced Kuhn's revolutions and paradigms, they lost contact with Frank's (and Conant's) more general, worldview-related schemes of humanization in the liberal arts-driven program of General Education. One might just wonder how much Paul Feyerabend (influenced heavily by Frank and Kuhn) intentionally played with his title "Science in a Free Society" with Conant's 1945 ideal of "General Education in a Free Society".

This talk will present all the available biographical data from the archives about the Frank-Kuhn relation that would substantiate this enterprise at the first place. Secondly, from a more systematic point of view, Frank and Kuhn will be interpreted within Conant's general education program as it evolved and changed in the political arena during their lifetime.

Kuhn, Popper, the Military-Industrial Complex and the Techno-Scientific Revolution

Daniele Cozzoli (Pompeu Fabra University, Spain)

This essay focuses on the political dimension of the debate between Thomas Kuhn and Karl R. Popper. While Popper's analysis of the development of science was coupled with his political philosophy, Kuhn tended to conceal the political implications of his views. Popper, Lakatos and Watkins stressed the political implications of any view on the scientific development, whereas Kuhn tried to avoid them.

The essay explains the difference between the two approaches in the light of the different cultural climates in the USA and in Britain. In the USA, philosophers of science (consciously or not) tended to eliminate the political commitment of their philosophical views. On the contrary, in post-war Europe liberal intellectuals struggled with Marxist scholars for the hegemony in the progressive camp. At that time Soviet scholars often talked of a 'technological and scientific revolution' and debated on how the scientific and technological breakthroughs could be used for the welfare of citizens and the transition from capitalism to socialism. In Britain, it was J. D. Bernal who defended the idea that science and technology had to be applied in planned economies for the wealth of all citizens.

The essay analyses the discussion between Popper and Bernal and stresses how Popper and Lakatos constructed their philosophical perspectives also with a view to contrast Bernal's ideas, which were strongly influencing the politics of the Labour Party in the 1960s. The essay also analyses the debate in Soviet Union and its possible echo in Britain by focusing on the papers of the Science and Technology subcommittee of the British Communist Party and Bernal papers.

Popper saw in Einstein's attitude of looking for crucial tests and not for confirmation of one's own theories the antidote against any kind of dogmatism, although his main concern was Marxism. In his view, Kuhn was undermining the rationality of scientific progress and, therefore, also of the 'open society.' Kuhn, on the contrary, tried to carefully avoid any political extrapolation of his philosophy of science. It is not, therefore, surprising that Kuhn did not take into accounts the technological, industrial and military dimension of scientific research. His work as an historian dealt with the conceptual dimension of the sixteenth and seventeenth century Scientific Revolution and on the birth of Quantum mechanics. He acknowledged his debt to Alexandre Koyré's historiography, but he never engaged with the work of those historians such as Olschki, Zilsel and Merton, who stressed the relations between science and technics in the early modern period. This fact has always puzzled historians, provided that Kuhn had worked on military-directed research on radar and that Structure came out the year after Eisenhower's farewell speech in which he mentioned the danger for democracy of 'military-industrial complex'. This essay argues that Kuhn's focusing on Koyre's perspective was influenced by the cultural climate of Cold War America, when most intellectuals were led to keep their work far from any political implication.

Kuhn and Feyerabend in Berkeley: The Birth of Pluralism in Modern Philosophy of Science

Karim Bschir (University of St Galen, Switzerland)

In the early 1960s, Thomas Kuhn and Paul Feyerabend were both at the University of California Berkeley where they engaged in an extensive discussion of an early draft of Kuhn's Structure of Scientific Revolutions. In this talk, I will reconstruct the relevance and impact of this critical exchange between two of the most important figures in 20th century philosophy of science.

I will show that it was Feyerabend's critique of Kuhn, and in particular of Kuhn's concept of normal science, that led him to think for the first time about the role and importance of theoretical and methodological alternatives in the development of science. Although his criticism had little impact on Kuhn, Feyerabend's critical engagement with Kuhn's ideas was crucially important for his own philosophical development and as such, as I claim, for the history of philosophy of science in the 20th century more generally.

My talk will consist of three parts. In the first part, I will carve out the main lines of Feyerabend's criticism of Structure using (among other sources) the detailed written correspondence between Feyerabend and Kuhn from the early 1960s. In the second part, I will show how the ideas that emerged in Feyerabend's criticism of Kuhn became the cornerstone of his own pluralist philosophy, which he developed further in the following years, and which culminated in his 1975 book Against Method.

In the second part, I will briefly highlight the specificities of Feyerabend's pluralism in such a way that its anti-Kuhnian roots become visible. I will also point out that, contrary to common belief and contrary to his own narrative, Feyerabend's position should not be seen as primarily anti-Popperian, but rather as the result of his critical engagement with Kuhn's Structure. In the third part, I will elaborate on the importance of Feyerabend's pluralism and its influence on contemporary pluralist accounts of science.

Ultimately, the early Kuhn-Feyerabend-exchange provides a good example for the inspiring effect Kuhn's Structure had on other thinkers as well as for its profound impact on the history and philosophy of science in the 20th century.

Kuhn and Philosophy

Vasso Kindi (University of Athens, Greece)

Thomas S. Kuhn is usually referred to as a historian of science, but he himself has said that he was a "a physicist who became a historian for philosophical purposes". In the paper, I want to argue, firstly, that Kuhn's most famous work, which has been largely read as historiographical, was predominantly philosophical and was meant as a contribution to philosophy. While The Structure of Scientific Revolutions draws upon material from the history of science, still, it engages with philosophical theories (such as, logical empiricism), addresses philosophical issues (for instance, how concepts are formed and changed), gives philosophical arguments for its major claims (e.g., that scientific development is discontinuous), and raises new philosophical questions (for example, relating to incommensurability). Secondly, I want to examine Kuhn's legacy in philosophy in general,

which has been significant despite the fact that his work has been stereotyped, scarcely or not carefully read, and belittled as philosophically naïve and incognizant. Although philosophers avoided to directly engage with his work and even mention his name, still they had to confront the issues that he raised and discussed. The demise of logical positivism that he helped to bring about, and the challenges that his model laid out, advanced the turn to realism and motivated research in response to fears of relativism and idealism, even outside philosophy of science proper. Bernard Williams' "absolute conception" was a case in point. Incommensurability has also been a constant matter of concern, especially in practical philosophy. Finally, regardless of the dismissive attitude towards his philosophy, many of Kuhn's views that in the second half of the twentieth century were thought to be radical and extreme, are now part of the variegated background against which we continue to conduct philosophical research.

Kuhn and the Prospects of Philosophical History of Science

Theodore Arabatzis (University of Athens, Greece)

Kuhn's classic *The Structure of Scientific Revolutions* opened up a spectrum of possibilities for integrating history and philosophy of science (HPS). In the subsequent history of HPS, only some of those possibilities have been realized, namely those related to historically informed philosophical reflection on the dynamics of scientific development. HPS has been, for the most part, a philosophers' project, where history of science is brought to bear upon philosophical theorizing about science. In this talk I will draw upon Kuhn's work to suggest ways of overcoming this imbalance and rethink HPS from a historian's point of view. I will argue that some of Kuhn's insights into scientific practice have been overshadowed by his grand narrative of scientific development. Those insights can still be fruitfully deployed and developed so as to enrich the historical interpretation of the sciences.

Kuhn's Wrong Turn or Bird's Mistaken Characterization?

Hyundeuk Cheon (Seoul National University, South Korea)

This talk aims to critically examine Bird's characterization and assessment of Kuhn's linguistic turn. It is widely believed that after the linguistic turn, Kuhn gave up the naturalistic argument based on history of science and empirical psychology and turned to more a prior, philosophical argument depending on language learning and translation. I will show that this is not the correct characterization of Kuhn's linguistic turn. According to Bird, Kuhn's philosophical legacy seems paradoxical, which can be accounted for mainly by Kuhn's wrong turning. Bird claimed that Kuhn made a wrong turn, and by doing so, he failed to develop the most insightful contribution, which is paradigm as exemplar, and instead proposed the unfortunate thesis of incommensurability that is not well received among philosophers.

First, I argue that Bird's description of what motivated Kuhn make such a turn is wrong-headed. According to Bird, Kuhn was inspired to be accepted as a good philosopher because he was not trained as a philosopher. Consequently, Bird suggested, Kuhn inherited the legacy of logical positivism, which was destined to fail. I claim that Kuhn's ideas can not be

regarded as positivistic, and Bird's interpretations ignore that incommensurability stemmed from Kuhn's experience as a historian, not from philosophical theories of meaning.

Second, I claim that the so-called linguistic turn should not be characterized as an anti-naturalistic turn. When Kuhn talked of language, he had in mind the empirical literature in cognitive psychology and developmental psychology. Thus, the language metaphor he used should not be treated as a priori. Furthermore, Kuhn said that what is meant by semantic incommensurability or lexicon can be better understood in terms of pre-linguistic conceptual modules, which can be investigated empirically. Then, Kuhn can be viewed as a naturalist even after the linguistic turn. Kuhn never abandoned the naturalistic approach.

Third, Bird demonstrated that Kuhn's insight on exemplars could be vindicated by recent research on analogical thinking, cases-based reasoning, and pattern recognition. Depending on empirical works from psychology and cognitive science, Bird suggested that working with exemplars shapes our cognitive habits, or what he called 'quasi-intuitive connections.' While this is impressive, I will point out that his way of naturalizing incommensurability is not satisfactory. Bird suggested that world-changes and incommensurability can be best understood as psychological. However, given that changes in cognitive habits seem gradual and continuous, it is not clear which changes in the quasi-intuitive connection are constitutive of the revolutionary change and which are not. In addition, if worlds are characterized as psychological, there is no gap between the mind and the world.

Something Happened to Me that Changed My Understanding of Aristotle's Physics. Did I just Experience a Gestalt Switch?

Margaretha Hendrickx

Could the concept of the gestalt switch (GS) be the key to unlock and reveal the significance of The Structure of Scientific Revolutions (SSR), the book in which Thomas Kuhn brought this concept to our attention with (Wittgenstein's) visual metaphor of the duck/rabbit?

My aim is to counter scholars like Abbott (2016:176) who dismiss gestalt arguments as dated and more a reflection of the intellectual environment in which SSR was received than a sign of its core content and possible significance. I intend to revive the GS concept and demonstrate its fertility, turning it into something that deserves further study by using Stephen Pepper's (1942) World Hypotheses as the backdrop for my arguments.

Pepper (1891-1972), was Mills professor of philosophy at UC Berkeley where he taught from 1918 to 1953, and he was still active when Kuhn joined its faculty as a junior member. Quite likely, Pepper and Kuhn talked with one another about their work and, indeed, Fuller (2001:398) commented on the similarities between the concept of a world hypothesis and the one of a Kuhnian paradigm.

But it seems that Fuller did not dig deep enough, possibly inducing other Kuhnian scholars to dismiss the Pepper-Kuhn link or treat it as a closed chapter. That is, I did not find any references to it published post 2001.

The Pepper-Kuhn link may be much more important than we have considered to date given that Pepper's World Hypotheses gives a rather unique insight into Kuhn's preoccupation with

his gestalt switch – as documented in detail by Reisch (2016). World Hypotheses indicates the following:

(1) The so-called gestalt-switch in Kuhn's head -- his "Aristotle moment" -- on that fateful day in 1947 happened given that Kuhn used an explicitly non-mechanistic background against which to evaluate Aristotle's writings on motion. He had stepped outside Pepper's world hypothesis of mechanism into the one of contextualism which focuses on the switching from one background/foreground to another one. Kuhn himself had switched world hypotheses!

(2) Next, Kuhn wondered why his time at Harvard where he obtained a BSc, Msc, and a PhD in physics had not prepared him for that experience. In Pepper's language, "Why did my science education not prepare me to work with root metaphors? Why was I not taught as a student that a competent scientist, under no circumstances, confuses and substitutes his root metaphors (language usage) for the unobservable reality that s/he is attempting to catch and concretize with the aid of metaphors?"

My reading of SSR, summarized above in (1) and (2), supports the work of scholars who argue that SSR was written deliberately to put the GS concept on the map. It adds to that work that Kuhn's intention was to alert us to the importance of maintaining control over the metaphors with which scientists inevitably work when they probe the unobservable reality by keeping a laser-sharp focus on these metaphors' limitations while still appreciating and recognizing their strengths. In this light, further study of SSR is necessary.

The Philosophical Framework of the Kuhnian Historiography: The Conflict between Naturalism, Neo-Kantianism and Nonrealism

Thodoris Dimitrakos (University of Patras, Greece)

From the historiographical point of view, being a Kuhnian is a matter of embracing the so-called 'cyclical pattern' of scientific change (Bird 2015). From a philosophical point of view, on the other hand, being a Kuhnian is not such a straightforward affair. It is widely accepted that Kuhn was committed to -at least some version of- naturalism (Kuhn 19962/1990;1970, Bird 2012, Shapin 2015); that he defended the autonomy of scientific rationality by embracing a dynamical version of a priori and thus a version of Neo-Kantianism (Kuhn 1993;2000a; 2000c, Friedman 2001;2002); and that he rejected the correspondence theory of truth, i.e. the traditional realistic conception of the world's mind-independence (Kuhn 2000a; Devlin 2015;2021; Kukkanen 2021). Consequently, one may conclude that being a Kuhnian implies the philosophical commitment to naturalism, neo-Kantianism and anti-realism (or nonrealism). In this paper, I argue that these three philosophical perspectives form an uneasy triangle, for while it is possible to coherently defend each of them separately or two of them combined, holding all three of them leads to incoherence. Hence, the only option is to reject one of them in a way that is both compatible with the Kuhnian historiography and philosophically fruitful.

The incoherence takes the form of an inevitable dilemma on what Kuhn (2000b: 112) takes to be the fundamental issue for the 'philosopher who adopts the historical perspective', that is how to make the changes of (scientific) belief intelligible. Given the Constitution Thesis

(Psillos 2009: 32), which is inherently Kuhnian, the objects of scientific inquiry are constituted by the conceptual schemes of the scientific theories. Thus, radical belief changes amount to changes in the conceptual framework (CF) which constitutes scientific experience. Therefore, the question is how to make the CF changes intelligible. Given nonrealism, truth cannot be explanatory of those changes. Thus, we face the following dilemma: either CF changes are explained exclusively by the empirical research of the various social or psychological (or other contingent) factors (naturalism) or they are made intelligible by appealing to (relatively) autonomous rational processes (Neo-Kantianism). That is why some self-proclaimed Kuhnians, even pace Kuhn, had to abandon either the defense of the autonomy of scientific rationality (e.g., Strong Program) or naturalism (Friedman's neo-Kantianism).

I suggest that there is an alternative path which avoids both naturalistic relativism and Neo-Kantian anti-naturalism. This path goes through the adoption of realism which allows to retain both naturalism and the autonomy of scientific rationality. In particular, I argue in favor of the following interconnected positions: 1) Given the role of anomalies in Kuhn's historiographical model, the latter satisfies the two fundamental conditions for acknowledging the mind-independence of the world: the 'irreducibility condition' and the 'objectivity condition' (Psillos 2017). 2). Kuhn's (2000b) rejection of the notion of an 'Archimedean platform' does not entail, as he seemed to think, that the truth-value of beliefs doesn't play a role in the explanation of their change. It only presupposes that the 'subject-side components' (Hoyningen-Huene 2021) of scientific knowledge are constantly attuned by the object-side elements. 3) Adopting realism is the only way to defend the autonomy of scientific rationality within a naturalistic philosophical framework. However, adopting realism leads to a liberal rather than an eliminative version of naturalism.

My purpose in this paper is not primarily exegetical. I don't intent to present Kuhn's actual philosophical commitments. My ambition is to sketch a coherent philosophical framework which does justice to Kuhn's historiographical model for scientific change and avoids the philosophical costs of relativism and anti-naturalism.

Meaning-Constitutive Generalizations: An Investigation on "Nomic" Concepts

Paulo Pirozelli (University of São Paulo, Brazil)

The nature of scientific terms appears as a major concern in Kuhn's later writings. According to him, kind terms are organized in hierarchical structures or "taxonomies", which are learned through acts of ostension. Later, in one of his last papers, Kuhn (1993) comes to distinguish a second sort of kind term, which he names "nomic". Those terms are learned with the use of natural laws, and their meaning is intimately connected to them. As examples of nomic terms, Kuhn mentions the concepts of "force" and "mass", which are learned together with Newton's second law.

According to Kuhn, the different learning process of nomic terms, compared to taxonomic (or normic) ones, has implications to their relation to generalizations. Nomic terms are learned with the use of some generalizations, and their meaning is partly determined by them; in

contrast, no specific generalization is necessarily connected to a normic term, and its meaning is only determined by the appropriate selection of correct instances of the class.

Kuhn, unfortunately, was not able to develop this distinction further, but a deeper consideration can highlight problems with it. I defend that all concepts exhibit a nomic component (a doubt already raised by Andersen & Nersessian (2000)). From an inferentialist perspective (Brandom, 1994, 2000), I claim that the meaning of a concept is related to the set of generalizations in which it may appear, and that a change in this generalization set implies a change in the concept's meaning. In other words, all concepts have some "definitional dimension" attached to them. The distinction between taxonomic and normic terms, therefore, is based on a subjective distinction between terms attached to generalizations that have obvious implications to their meaning, and terms that have less conspicuous connections to generalizations.

Although Kuhn sometimes acknowledges this nomic dimension (e.g., 1993), his focus on taxonomic terms tends to eclipse this aspect of word meaning. Kuhn's notion of nomic terms, however, can, and should, be extended to scientific concepts in general. This perspective has consequences for important aspects of Kuhn's view on meaning, in particular those on intensionality and extensionality. For him, taxonomic terms have both an extensional as an intentional aspect (1983). Nonetheless, due to the contingent nature of generalizations connected to taxonomic terms, Kuhn tended to treat this intensionality as a mere tool for determining a term's extension (1989), with only rare implications for the communication of members in scientific communities. I believe that expanding the notion of nomic terms may enrich the conception of "intensionality" associated with kind terms. Contrary to Kuhn's view on intensionality, I argue that the meaning of a term is always connected to some socially accepted generalizations: understanding the meaning of a scientific kind is not only being able to select its referent and distinguish it from those of contrasting classes, but also being able to extract appropriate linguistic consequences of it. This change may also have implications to our understanding of non-cumulative transitions — as conjectured by Kuhn in (1974, p. 304, n. 14).

Normal Science and Normalization: Divergence in the Adherence to Tradition in West and East Asia

M. A. Mujeeb Khan (University of Utah, USA)

This paper focuses on normal science, an often-overlooked contribution of Kuhn. Kuhn's discussion of normal science and its function in intellectual development situates transformation not in paradigm shifts identified by incommensurability but rather in the continuation and preservation of traditions either before or after such shifts. This represents a different paradigmatic shift, one of continuation and preservation. Kuhn outlines this in his discussion of textbooks and the normalization of new knowledge, which he terms normal science. In particular, Kuhn describes how new, generally theoretical, knowledge is consumed as accepted and established by subsequent generations. It is this generational transformation of knowledge in normal science that reveals how efforts to continue and preserve an existing tradition simultaneously produce something new, which, while seemingly identical to present participants, would appear considerably different to an earlier generation. The implications of this for a linear analysis of any discipline might be obvious

but these insights of Kuhn into normal science are also insightful and instructive in the examination of cross-cultural traditions. To explore the value of these points, this paper takes the early Islamicate and Japanese medical traditions. Historically situated in the global medieval period, the medical traditions of both societies were the result of cross-cultural transfer that resulted in new local knowledge by the 10th century, a product of the Greco-Syriac-Arabic translation movement in the former and, in the latter, that of the Sinitic cultural sphere's flow of Sino-Korean knowledge. In order to investigate normal science, this paper takes from the Islamicate world, Abū Bakr al-Rāzī (d. ca. 925/932), an Abbasid state-appointed physician in Baghdad, and from Japan, Tanba no Yasuyori (d. 995), a Heian medical bureaucrat. Each composed an encyclopedic medical compendium, sourcing contemporary, earlier, and imported knowledge. For this reason, this paper employs these texts, al-Rāzī's "Kitāb al-Ḥāwī fī al-Ṭibb" (The Comprehensive Book of Medicine) and Yasuyori's "Ishinpō" (Essential Medical Methods) to demonstrate how Kuhn's principles are applicable in these pre-modern, non-Western, cross-cultural medical traditions. It will show how imported foreign medical knowledge and locally produced knowledge functioned in each society just as textbooks do in Kuhnian normal science. In addition, this paper will also identify the transformative shift between generations described by Kuhn and discernible within these traditions by conducting a comparative examination of al-Rāzī's and Yasuyori's works with their predecessors, both those within their traditions and those within the imported traditions. In the case of al-Rāzī, this includes early works ranging from the Hippocratic Corpus (ca. 4th BCE) to Galen (d. ca. 216) to authors in the Arabic tradition such as Thābit ibn Qurra (d. 901). In that of Yasuyori, this consists of continental works such as early as the "Huangdi Neijing" (Yellow Emperor's Classic of Medicine) corpus (ca. 4th BCE) to later writers like Chao Yuanfang (d. 618) and local Japanese works. The paper also hopes to shed light on how the application, to these new contexts, of the normative standard of a separate discursive tradition, i.e., of the imported tradition, distorts the reality of their intellectual milieu.

Scientific Progress as Evolution in Kuhn's Philosophy of Science: A Fruitful Analogy?

Nicola Bertoldi (Université du Québec à Montréal, Canada) and Silvia de Cesare (Université de Genève, Switzerland)

In The Structure of Scientific Revolutions (SSR), Thomas Kuhn (1962) dismissed the idea that the history of science qua diachronic succession of paradigms embodies scientific progress understood as an ever-closer approximation to reality and absolute truth. If science is "merely" a set of tools for solving practical and intellectual problems and if scientific paradigms are "incommensurable", why should the latest paradigms be regarded as inherently more valid and accurate than those that preceded them? However, Kuhn also rejected all accusations of relativism by defending an alternative view of scientific progress, which is first alluded to in SSR's 13th chapter. This alternative view relies on the evocative analogy expressed by the following sentence: "For me, therefore, scientific development is, like biological evolution, unidirectional and irreversible" (Kuhn 1970, p. 264). In Kuhn's perspective, just as the Darwinian theory of evolution by natural selection allows understanding the appearance of ever-more elaborate, articulate and specialised organisms without positing the existence of a natural (whether immanent or transcendent) goal, his

(meta)theory of scientific paradigms enables conceiving scientific progress as a non-teleological process fuelled by conflicts within and between scientific communities. Therefore, could we argue that the Kuhnian philosophy of science presupposes an evolutionary view of scientific progress?

This paper aims to answer this question by problematising both sides of Kuhn's analogy, i.e. the equation of unidirectional and irreversible evolution with biological progress and the view that the successive branching of paradigms outlined by Kuhn constitutes an evolutionary process. On the one hand, Kuhn seemed to presuppose that greater "articulation" and "specialisation" are linked to the mechanism of natural selection and constitute criteria for determining the, albeit relative, "superiority" of more phylogenetically recent organisms. We will thus illustrate how Kuhn's presupposition is problematic by drawing on the arguments of two evolutionary biologists and philosophers of biology, i.e. Stephen Jay Gould and Daniel McShea. Furthermore, we will also argue that a coherent conception of organic progress compatible with evolutionary theory has yet to be developed. On the other hand, we will problematise another assumption implicit in Kuhn's view of scientific progress, i.e. the idea that the diversification and specialisation of scientific theories can be regarded as a process analogous to evolution by natural selection. To this aim, we will show how this assumption is related to the problematic nature of Darwin's principle of divergence and how its validity hinges upon further premises connecting diversity with fitness as properties of organisms or theories. We will also assess whether such premises could be drawn from attempts to extend niche construction theory to the study of cultural and cognitive niches (Laland & O'Brien 2011).

Why Kuhn Doesn't Fit Evolutionary Biology

Andrea Olmo Viola (Università degli studi di Roma La Sapienza, Italy)

Recently there have been attempts to reevaluate Kuhnian epistemology and extend it to the interpretation of the history of evolutionary biology. I argue that these extensions are based on simplifications of the history and Kuhn's epistemology isn't helpful to understand and explain the history, the development and actual debates in evolutionary biology.
Darwin's theory and natural theology weren't incommensurable. Theologists understood the theory and its implication, developed appropriate criticisms, which were seriously addressed by Darwin. Moreover, researchers were able to evaluate the different merits of the theories, comparisons were possible, the relative usefulness of the theories was evaluated in order to expand the research; in the Origin of Species there are continuous comparisons dedicated to different explanatory capabilities of the two rival theories.

There has never been a Darwinian paradigm: from the beginning Darwin's framework was received differently. The majority of naturalists adopted an evolutionary perspective, not the Darwinian explanation. His framework was disassembled and reassorted, by different scholars, who accepted some components, rejected and reformed others. The Darwinian framework itself changed: examining the six editions of Origin of Species we can see how Darwin changed the structure of his theory over time. Other example of different receptions and reforms of the original framework are: T. H. Huxley was not convinced by gradualism, A. Gray interpreted the variation as directional, A. Weismann developed a theory without Lamarckian components. The field was not uniform, always full of heresies: Darwin's framework fostered the evolution of different research paths.

Darwinian theory was plagued by some anomalies, with the emergence of rival frameworks, in the early 20th century, the weight of the anomalies and the successes of the rivals convinced many scholars of the death of Darwin's theory. Hardly explainable is the eclipse and then the convergence that led to the Modern Synthesis. It was a slow, interdisciplinary, collective work of synthesis and integration that lasted from '18 until '37.
The daily work of researchers is not reducible to puzzle-solving. The theory of evolution has been characterized by an interesting heuristic power, which favored progressive empirical expansion and a continuous work of refinement and integration of new theories, which slowly, over time, changed the explanatory structure of the theory. Darwin's theory and today's theory of evolution are different, but there is a genealogical relationship, the development of the second is internal to Darwin's framework and never happened a discontinuity or radical substitution.

A Kuhnian interpretation is also inappropriate to understand the emergence of the Extended Synthesis, which is not an alternative paradigm, but an implementation of the explanatory structure, a reform of the domains of relevance of explanatory mechanisms of the Modern Synthesis.

Kuhn's epistemology has been influent in the lexicon and useful to focalize on certain aspects of the dynamics of scientific research before neglected, but it seems not fit to explain the development of evolutionary biology. The problem might be the universalist ambition of certain interpretative models. We must accept that those models may have a limited range of application or none.

Kuhn and the Challenges of Integrated History and Philosophy of Science

Haosk Chang (University of Cambridge, UK)

Despite his crucial role in promoting the interaction between the history of science and the philosophy of science, Kuhn was profoundly uneasy about the relation between the two. He remarked that one could do both history and philosophy, but not at once. He was particularly critical of the Lakatosian mode of integrated history and philosophy of science (HPS), issuing a scathing review of the Lakatosian historical case-studies collected in the 1976 volume *Method and Appraisal in the Physical Sciences*. Later he took broader aim at what he designated as the "historical philosophy of science", in his Rothschild Lecture at Harvard in 1991. The latter occasion was nothing short of the main instigator of the "historical turn" in the philosophy of science condemning the whole enterprise.

My discussion in this paper has two main aims. One is to reach a better understanding of the ways in which Kuhn practised the history and the philosophy of science, as relevant background for the unease he expressed about integrating the two. His major works in the history of science were done with little explicit framing in terms of the philosophy of science, and almost none at all in his last major historical work, *Black-Body Theory and the Quantum Discontinuity* (1984). He seems to have been entirely comfortable in this line of work, and his concrete output was quite consonant with the methodological articulation he made of the "new internal historiography". His historical work was respected by most of his fellow historians, and largely accepted without controversy. On the other hand, his early

philosophical work, first becoming famous with *The Structure of Scientific Revolutions* (1962), was heavily shot through with historical examples, and would have lacked much of its persuasive power without them. At the same time, this history-based mode of argument was heavily criticised by professional philosophers. Kuhn's wariness about integrated HPS is quite understandable in light of his own experience of comfortable success in non-philosophical history and notoriety and controversy in historical philosophy.

The other main aim of this paper is to propose a way in which practitioners of integrated history and philosophy of science can overcome the obstacles to integration that Kuhn perceived. I believe that the core of the difficulty was Kuhn's (and others') attachment to standard analytic philosophy, particularly the narrow view of knowledge as consisting of propositions. Kuhn rightly perceived that this kind of philosophy was not helpful for his historical work, but he did not reach for other philosophical traditions (such as pragmatism, hermeneutics and phenomenology) that would have been more helpful. Instead he created his own philosophical framework of paradigms, normal science and revolutions, which was rejected by most philosophers and, ironically, not used by himself in any of his major historical works. In his later years Kuhn tried in vain to get his ideas accepted by analytic philosophers by adopting their methods, for example by trying to reduce incommensurability to cross-cutting boundaries of the extensions of terms. I propose that we can develop a more authentic Kuhnian historiography by giving a pragmatist rendition of Kuhn's key philosophical ideas, and employing them as explicit framing devices for historical narrative and analysis. This approach can also help us overcome the problems of "historical philosophy of science" that Kuhn pointed out. Instead of treating the philosophy–history relation as that of a defective inductive proof of theory from a small base of contaminated evidence, we can see it as a dynamic relation of iterative mutual stimulation and improvement.

The Structure of Scientific Developments in the 21st Century

Hanne Andersen (University of Copenhagen, Denmark)

Kuhn's The Structure of Scientific Revolutions has remained one of the most influential accounts of the structure and development of science for more than half a century. However, vested as it was in studies of individual scientists performing basic re-search within their discipline, the account that Kuhn developed was not prepared to accommodate the way in which science developed during the latter half of the 20th and into the 21st century.

In this talk, I shall sketch some of the overarching systemic forces that drive or halt the progress of contemporary science. I shall argue that, similar to the 'essential ten-sion' between tradition and innovation that Kuhn described as the fundamental driver of progress for small-scale research within disciplinary boundaries, similar tensions between contrasting epistemic values are central for understanding pro-gress as well as stagnation in collaborative, interdisciplinary, strategic, and large-scale research. I shall argue that an analysis of how these tensions drive or halt sci-entific progress today can provide new perspectives on classical issues such as crea-tivity and academic freedom, and give important guidance for ongoing debates in science policy on research organization, research funding, and strategic research planning.

A Pragmatic Approach to Scientific Change: Transfer, Alignment, Influence

Stefano Canali (Politecnico di Milano, Italy)

In this paper, I present a pragmatic approach to scientific change, building on the seminal work of Thomas Kuhn and integrating recent work in philosophy of science and technology, to expand our understanding of scientific change in contemporary science. On the basis of a case study of scientific change in data-intensive biomedical research, I identify and discuss three central processes of change emerging in contemporary science: transfer, alignment, influence. While the analysis of these processes builds on Kuhn's work, I argue that we need to expand this and other philosophical models of scientific change with a pragmatic approach – opening our focus on the role of scientific practice and the flexibility of the processes that can elicit change.

I start by introducing the case study of the paper: the epidemiology of the exposome. The exposome has emerged as a new way of conceptualising and studying the totality of environmental exposures in the last two decades and has received significant philosophical attention in relation to discussions on causal inference, post-genomics, data. In epidemiology, the exposome approach is considered highly innovative, to the point that it is usually presented as a new paradigm, as a result of the integration of new, diverse, and large volumes of data. Building on Kuhn's work, I distinguish three different and interrelated senses in which scientific change is discussed in the context of the exposome: change as a new concept to complement the genome, a new approach using diverse health and environmental data, and a new social and institutional context for research, funding, education. We can see novelty along these conceptual, methodological, and social axes: however, this change is difficult to understand using the Kuhnian model, as I argue with reference to processes of transfer, alignment, and influence.

Crucial conceptual and methodological aspects of the exposome approach have been transferred from other areas of the health sciences. While the transfer of new concepts and data has led to methodological and conceptual change, this has also created the need to align changes with other and exiting approaches in epidemiology. Processes of transfer and alignment have thus led to scientific change in continuity with the areas of transfer and in alignment with the original area, thus creating pluralisation rather than disruption and incommensurability. This type of change can be further established through influence, a process that Kuhn discussed in relation to the ability of paradigms of taking over specific areas of research, with the methodological success of solving some new problems and the epistemic appeal of new frameworks and world views. The influence we see in cases such as in exposome research is less dominant and more local, often centring on data as assets for alliances and funding, but also obstacles for long-term projects and interdisciplinary work.

Building on Kuhnian lessons, thus, I present a pragmatic approach that complements philosophical models of scientific change with a focus on transfer, alignment, and influence as different and flexible ways to elicit change in contemporary science.

Co-Constructing Worlds: Paradigms, Values, and Weights in Theory-Choice

Matteo De Benedetto (Ruhr-Universität Bochum, Germany) and Michele Luchetti (Max Planck Institute for the History of Science, Germany)

In this talk, we will reconstruct Kuhn's thesis of the plurality of phenomenal worlds by arguing that these worlds result from different subjective weightings of epistemic values. Specifically, we will argue that scientists working within a given paradigm may attribute a subjective weighting to the epistemic values involved in their choices among competing theories. We will frame our proposal within a model of theory choice developed with tools from social choice theory. We will show how our model adequately reconstructs a classic scientific controversy as a co-construction of two different phenomenal worlds: the clash between Mendelians and Anti-Mendelians in the first decades of the twentieth century.

Reconstruing the Kuhnian "Different Worlds"

Qiu Lin (Duke University, USA)

Thomas Kuhn is famous for his thesis of incommensurability, which holds that there is no common measure for comparing successive scientific theories. First put forward in *The Structure of Scientific Revolutions* , Kuhn points out that the conceptual readjustment that typically accompanies scientific revolutions can be so radical that after revolution, scientists effectively "work in a different world."

Kuhn's thinking evolved over the years, but the task of articulating the conditions under which two successive theories are incommensurable remained a life-long undertaking (Buchwald & Smith, 1997). In his unfinished book draft titled *The Plurality of Worlds: An Evolutionary Theory of Scientific Development*, Kuhn raises a further insight on major conceptual readjustment in science: What each of my examples specifically illustrates is not meaning change in general but a locally holistic transformation of meanings: the simultaneous alteration, that is, of the meanings of a set of interrelated terms no one of which could – if textual coherence were to be preserved – have changed in the way it did without changing the others as well.

A trained physicist, Kuhn is quick to see how change in the meanings of parameters during periods of major transition (as in the transition from Newtonian Physics to Einstein's General Relativity) has to be holistic. But construing changes in holistic meaning in terms of textual coherence is too weak, for the primary concern of physicists is not the meanings of the parameters they employ.

Rather, it is the causal structure of the physical world. Taking Kuhn's insight in a different direction, the aim of this paper is to reconstrue the Kuhnian "different world" post-scientific revolution as a different world of counterfactuals. I will proceed as follows.

Section 1 deals with the problem of testing counterfactuals in science. Nelson Goodman (1947) once argues that a counterfactual conditional "cannot be subjected to empirical test by realizing its antecedent." This, I believe, is only partially true. Unlike Goodman, I argue that *some* counterfactual conditionals can indeed be tested – just not by realizing their antecedent.

When existing physical laws are placed within the framework of a theory, they almost always end up being restated counterfactually. Within the framework of Newtonian physics, for instance, Kepler's three laws of planetary motion become counterfactual statements under specified circumstances in which they hold exactly. Testing Kepler's laws thus amounts to testing the counterfactuals in which they are stated.

How do scientists test counterfactuals? Section 2 turns to Simon Newcomb's attempt to solve the mystery of the missing 43 arc-seconds per century residual in the precession of Mercury's perihelion. Newcomb proposed five hypotheses, then eliminated each by means of counterfactual reasoning: if the hypothesis *had been* true, it *would have* produced consequences that were incompatible with various known phenomena (as initially shown by Newcomb himself and later affirmed by Ernest Brown).

Section 3 argues that counterfactual reasoning is theory-specific. The counterfactual reasoning licensed by a given theory is determined by the conceptual scheme (understood as the relationship between fundamental parameters such as mass, space, and time) that the theory provides. In Newtonian physics, mass, time, and space all vary independently of one another. But in General Relativity, an increase in mass causes space to curve and the time spent traversing that space to lengthen. While Newtonian physics can still be thought of as a special case of general relativity, the counterfactual domain licensed by the former departs radically from that licensed by the latter. In this sense, physicists working within the conceptual schemes of these two theories can indeed be said to work in two different worlds.

Kuhn's 'Newtonian Paradigm': The Cause of the Impenetrability of 18th Century Physics?

Michael Veldman (Duke University, USA)

Kuhn argued that scientific inquiry becomes a fruitful program of "normal science" when collective acceptance causes a "paradigm" to coalesce out of a field of controversy. In Kuhn's work, Newtonian physics is something like the paradigmatic paradigm, used to map out a canonical structure for the evolution of science. Contra Kuhn, in 18th century physics, there was no such thing as the 'Newtonian paradigm'. One reason is that the concept of force – central to Newtonian physics – remained controversial throughout the 18th century. Nonetheless, in the Introduction to Structure, Kuhn claimed that:

"Effective research scarcely begins before a scientific community thinks it has acquired firm answers to questions like the following: What are the fundamental entities of which the universe is composed? How do these interact with each other and with the senses? What questions may legitimately be asked about such entities…?"

The idea of a Newtonian paradigm has proven to be a compelling lens for philosophers interested in the history of physics. But if we take off those glasses for a moment, we will see that none of these questions had consensus answers in the 18th century, and that Euler, Maupertuis, d'Alembert and others were working out their own foundations for what would only later become 'classical mechanics'.

In my paper, I will focus first on Kuhn's second question as it is informed by the dispute over force and impenetrability. The question of the nature of forces was up for grabs for most of

the 18th century. One aspect of the surrounding controversy traces back to Newton's own deployment of the concept of 'force of inertia', which puzzlingly categorizes the cause of the persistence of a body in its state of motion as a 'force,' alongside pushes and pulls. Euler criticized this and other perceived issues in Newton's conception of matter, and attempted to resolve them by means of a fundamental theory of the nature of forces and bodies that relied crucially on corporeal impenetrability. This and related work by the likes of d'Alembert helps us appreciate that Newtonian theory was not anything like a comprehensive paradigm for 18th century physics. We are then put in a position to see that Euler, Maupertuis, d'Alembert and others were engaged in an active search for foundational principles for mechanics, implying they were not, as Kuhn thought, simply addressing residual "problems of application" of Newton's theory, still less producing "reformulations of the Principia" (Structure, 32-3). Rather, they represent an alternative strand of mechanical theory – importantly, one motivated by deep conceptual disagreements with the Principia. Though critical, the paper aims to bring to light lessons that deepen the historical understanding of science Kuhn's work so well exemplified. As a final payoff, we make room for a different, hopefully more nuanced interpretation of the emergence of a "Newtonian paradigm": 'Continental' mechanics first offered well-defined, comprehensive alternatives near the end of the 18th century; it was this that allowed it to delimit the concepts and physical applications subsequently taken to define the "Newtonian paradigm".

Paradigms and Communities: On Reading Kuhn's History of the Quantum

Jan Potters (University of Antwerp, Belgium)

More than a decade after the appearance of 'The Structure of Scientific Revolutions', Kuhn published his 'Black-Body Theory and the Quantum Discontinuity,' in which he discussed the emergence of the quantum within physics. While the book is known for its claim that Planck did not introduce the quantum in 1901, the most discussed question since its publication has been whether its narrative is in line with how Kuhn had earlier characterized scientific change. While Kuhn himself saw Black-Body Theory as quite in line with Structure, most of its readers have claimed that the book in fact abandoned many of the central points, in particular the claim that paradigm switches are to be understood as gestalt switches. Jochen Büttner, Jürgen Renn and Matthias Schemmel quite adequately summarize this criticism as follows:

"According to Kuhn's theory, a sudden gestalt switch that can usually be ascribed to an individual ends a period of crisis resulting from anomalies and brings about a new paradigm. […] [I]n the early history of the quantum discontinuity, breaks with classical physics were rather the result of the gradual and tedious exploration, not just by an individual scientist but also by the scientific community" (2003, p. 56)

In this talk, I will distinguish, following Joseph Rouse's (1987) work, two interpretations of paradigms: a theory-focused one, which conceptualizes a paradigm as a set of explicit theoretical commitments about what the world is like, and a practice-driven one, according to which it is to be understood more as an implicit way of knowing how to go about, comparable to a skill. I will then argue that the criticism of Black-Body Theory presupposes a theory-focused reading, and that, if one rather starts from a practice-driven interpretation, one obtains a reading of the book that is very much in line with how Kuhn characterizes scientific

change in Structure. This will then allow me, in particular, to nuance the close connection that is often drawn between paradigm switches and gestalt switches. Rather, on the practice-driven view, a paradigm can only come to function as a sort of gestalt – i.e. as something structuring a scientist's perception – given a scientific community in which scientists enter through education and training.

In the last part , I will then reflect on why Kuhn, after the publication of Structure, came to put more emphasis on the practice-driven aspects of paradigms. I will highlight two aspects of his work: his work on the Sources for the History of Quantum Physics project, and in particular the interviews he conducted, and his attempts to simulate aspects of paradigm change by means of computer experiments. These, I will then suggest as a sort of tentative conclusion, indicated to Kuhn the primordiality of communities over paradigms.

Thomas Kuhn and Modern Mathematics

David Corfield (University of Kent, UK)

It is now 40 years since a group of philosophers and historians published a collection of articles (Gillies (ed.) 1992) which took as their central question the usefulness for the philosophy of mathematics of Kuhn's constructions in the philosophy of science. Important work was done in constructing case studies from the history of mathematics to see whether the concepts of 'revolution' and 'paradigm' made sense there. Now, a feature of the collection which should raise doubts about the universality of its findings is how the vast majority of the studies were addressed to periods prior to 1900. In this talk I shall be discussing the relevance of Kuhn's ideas, and of the objections made to them by Michael Friedman in 'Dynamics of Reason' (2001) and other works, to aspects of the mathematics of the past 90 years.

The Role of Mathematics in the Copernican Revolution

Donald Gillies (University College London, UK)

In an earlier publication I argued that the Copernican Revolution can conveniently be divided into two, partly overlapping, phases. The first phase comprises the work of Copernicus and Kepler. The main focus was on improving the accuracy of planetary astronomy. This goal was successfully achieved, and enabled much better astronomical tables to be produced. The Prutenic tables of 1551, compiled using Copernicus' work, were much superior to any previous ones, and the Rudolphine tables, compiled by Kepler himself and published in 1627, were better still. The second phase began with Galileo's telescopic discoveries, which were published in 1610. It resulted though the work of Galileo, Descartes, Huyghens and Newton in the creation of a new mathematical mechanics. The main thesis of this paper is that the role of mathematics was significantly different in the two phases. The mathematical apparatus used by Copernicus and Kepler did not differ very much from that used by the ancient Greeks. Kepler made a striking innovation in using ellipses rather than combinations of circles to describe planetary orbits, but this did not require mathematical innovation. The mathematics of ellipses had been worked out by Apollonius of Perga, and Kepler could take it 'off the shelf'. This pattern is also to be found in the revolution in physics in the first three decades of the twentieth century. Though relativity and quantum mechanics transformed

physics, they did not require any new mathematics. When Einstein was developing General Relativity, he was able, like Kepler, to take the necessary mathematics 'off the shelf'. A quite different pattern is to be found in the second phase, for the development of a new mechanics brought into existence analytic geometry and calculus, which together produced a complete revolution in mathematics. In the paper I will try to analyse both the heuristics that produced this revolution and the philosophical problems that the revolution created.