Third Young Researchers Days in Logic, Philosophy and History of Science

Thomas Kuhn's The Structure of Scientific Revolutions at fifty

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Conference Aims and Scope

In 1962, Thomas Kuhn published *The Structure of Scientific Revolutions*, in which he developed an epoch making theory on scientific methodology and organization. Kuhn's concept of paradigm became a hallmark of modern philosophy of science, identifying the philosophical architecture of the paradigm and analyzing scientists' internal strategies to strengthen and defend the core research program of the paradigm. At the same time, the social embeddedness of the paradigm opened new vistas in historical research, leading to an externalist view of science, to the 'strong program' of the Edinburgh School, and to social constructivism. Whereas philosophers of science studied the foundations of the paradigm and its stability over time, historians tended to regard the paradigm as permeable to social influences, and as a product of negotiating the boundaries of the scientific endeavor. These divergent developments caused philosophy and history of science to lose sight of each other, each of them even considering the other field as irrelevant or misguided. Fifty years after Kuhn's work, a new interest in overlapping themes between history and philosophy of science has emerged. The development of the Science and Technology Studies has incited historians of science to formulate abstract and 'ahistorical' theories about e.g. scientific innovation and circulation of knowledge, which have invited comments from philosophers. New debates have taken shape, focusing less on methodology but raising questions on scientific authority, expertise and public space. This meeting will seek to explore the many ways in which history, philosophy and logic can find fruitful ways to collaborate towards a better understanding of modern science. Before all else, however, this workshop should be an ideal opportunity for young researchers (PhD students, young postdocs) working in the cited fields to present their work to a larger community, and for all researchers in these fields to meet and reinforce scientific and personal links.

Invited speakers

Karine Chemla (CNRS, Equipe REHSEIS, Paris)
Jouni-Matti Kuukkanen (University of Leiden)
Thomas Nickles (University of Nevada)
Josep Simon (Université Paris Ouest)

Scientific and Organizing Committee

P. Allo (Brussel), K. Bertrams (Bruxelles), B. Leclercq (Liège), E. Myin (Antwerpen), G. Vanpaemel (Leuven), B. Van Kerkhove (Brussel)

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Abstracts

The Incommensurability Thesis and the Rationality of Science
Pablo Acuña (Utrecht University)

The goal of the following paper is twofold. In the first part I analyze the meaning and rationale of the incommensurability thesis as originally presented by Kuhn and Feyerabend in 1962, and I argue that it is essentially connected with the theory ladenness of observation: acceptance of the latter entails acceptance of the former. In the second part I tackle the issue of the effects of incommensurability on a rational conception of science. I argue that it does not lead to any difficulty with respect to the rationality and objectivity both of theory choice and of the diachronic course of science. A rational, objective and empirically grounded choice can be made between contending-incommensurable theories; and a continuous and progressive conception of the development of science is compatible with the incommensurability thesis.

Brussels Faculty of Medicine: In between Creation, Diffusion and Education of Knowledge
Renaud Bardez (Universit/'e Libre de Bruxelles)

A travers une étude de cas, en l'occurrence la Faculté de Médecine de l'Université Libre de Bruxelles, mon objectif est de mettre en perspective le rôle des étudiants dans la création et la diffusion des savoirs médicaux nouveaux. Cela m'amène inévitablement à comprendre l'influence des tudiants sur l'enseignement et le rôle joué par ceux-ci dans le processus d'institutionnalisation des savoirs. En partant de l'hypothèse que les étudiants en médecine, particulièrement dans le cadre de l'internat/externat et lors de la préparation de la thèse d'agrégation, sont le moteur d'un changement tant au niveau de la conception de l'enseignement que des connaissances de la matière médicale, je souhaite mettre en exergue la manière dont des spécialités et savoirs nouveaux se sont imposés jusqu'à ètre officiellement enseignés et portés au programme de l'Université. Dès lors, l'analyse du programme d'enseignement en médecine constitue le socle de départ de mes recherches. L'introduction d'intitulés

spécifiques et de spécialités sont le fruit de conflits et de choix d'enseignement importants qui déterminent et traduisent le positionnement scientifique et philosophique d'une Faculté et d'une Université. Le but est de déterminer, en corrélation avec l'enseignement dispensé, le type de recherche expérimentale réalisée au 19ème siècle et la manière dont celle-ci s'est réalisée jusqu'au processus d'institutionnalisation. Ce type d'histoire, à mi-chemin entre les Science Studies et une histoire institutionnelle, pose de nombreux problèmes méthodologiques. La première difficulté réside dans le mode d'enseignement dispensé au 19ème siècle et pour lequel peu de traces subsistent. Outre les procès-verbaux de la Faculté (série incomplète) et du conseil d'administration, il n'existe aucun fonds archivistique construit autour de l'enseignement médical l'Université Libre de Bruxelles. Cela m'amène à procéder à une déconstruction profonde des sources disponibles et à interroger d'autres types de sources tels que les thèses d'agrégation, considérées comme centrales dans ma thèse et dans l'étude de la production scientifique.

Rethinking Computation. Reflections based on ancient Chinese sources
Karine Chemla (CNRS, Equipe REHSEIS, Paris)

The recent development of history and philosophy of science has led to a more and more conspicuous divide between those who study ancient science and those who study modern science. It may be interesting to identify the forces at play in bringing about this divide and to ponder about its recent history. In fact, in this context, modern science attracts an increasing attention, the only ancient name that has not disappeared from discussions on modern science apparently being that of Euclid. This talk is a plea for maintaining the coherence of the discipline and thinking about ancient and modern science conjointly. It is also a plea for thinking about science worldwide. I shall illustrate these suggestions, by outlining a research program on computation for which ancient and modern documents taken everywhere on the planet are a source of new questions and reflection. This research program, in my view, offers opportunities of cooperation between history and philosophy of science.

Logical Omniscience and Structured Propositions
Lorenz Demey (Katholieke Universiteit Leuven)

In this talk I will address a cluster of problems at the interface between epistemic logic and philosophy of language. The classical relational (Kripkean) semantics for epistemic logic leads to the prediction that an agent's knowledge includes all logical theorems and is closed under logical entailment, i.e. that the agent is *logically omniscient*. Of course, in real life agents are not logically omniscient, and thus various logicians have developed and defended a wide range of solutions to this problem. In the talk I will classify these solutions as either *semantic* or *syntactic* (this distinction is not meant to be exhaustive). On a technical level, the syntactic approaches are most successful, but they have an important philosophical drawback. Semantic solutions maintain the widespread idea that knowledge is a semantic notion (i.e. a propositional attitude), whereas syntactic solutions view knowledge as a purely syntactic operator, which directly takes 'raw' formulas as arguments. The main aim of my talk is to argue that this philosophical problem for the family of syntactic solutions can be solved. I will point out that the distinction between semantic and syntactic solutions to the logical omniscience problem presupposes a philosophically laden notion of 'proposition', viz. propositions as *sets of possible worlds*. I will then look at other, more sophisticated theories of propositions, which claim that the proposition π expressed by a natural language sentence s is a *structured entity*, which is isomorphic to (i.e. has the same structure as) the sentence's underlying syntactic structure ℓ (LF/logical form). Like most of mathematics, epistemic logic only works 'up to isomorphism', and thus should not distinguish between the isomorphic proposition π and logical form ℓ. Furthermore, the distinction between the sentence s and its logical form ℓ is only introduced in linguistics and philosophy of language to accommodate possible ambiguities in s (e.g. as is well-known, the sentence 'every man loves a woman' is ambiguous, and is therefore claimed to have two distinct underlying logical forms). In logic, such factors can safely be assumed to have already been eliminated (e.g. an atomic formula like $p \wedge q$ is itself already a perfectly non-ambiguous expression), and thus the sentence s can be identified with its logical form ℓ. In sum, this means that in logic we can identify any formula s with the (structured) proposition π expressed by s. Hence, we can enjoy all the technical benefits of the syntactic approaches to the logical omniscience problem, without having to abandon the idea that knowledge is a propositional attitude: knowledge can still be seen as an attitude towards 'raw' formulas, but equally well as an attitude towards the structured propositions that are expressed by (and isomorphic to) those formulas.

The Problem of Kuhnian Rationality

Rogier De Langhe (Tilburg University)

Thomas Kuhn's "Structure of Scientific Revolutions" is believed to be one of the most important books in 20th century philosophy of science. Yet the book has more enemies than friends and even its friends, fellow historicists such as Imre Lakatos and Larry Laudan have almost invariantly tried to change or reformulate Kuhn's view in search of a historically warranted notion of scientific rationality. Lakatos' notion of rationality is based on a research programme's ability to generate novel predictions, while according to Laudan rationality turns around a research tradition's problem solving ability. For Kuhn there is no paradigm-independent set of rules to decide between rival paradigms. Many commentators took the absence of paradigm-independent rules to imply that paradigm choice must necessarily be irrational on Kuhn's account. Still Kuhn himself maintained that paradigm-choice is rational, although he never developed a specific account of rationality. I will call the problem of finding a conception of rationality that is consistent with Kuhn's account the problem of Kuhnian rationality.

The three most important views on rationality are confirmationism, falsificationism and historicism. Confirmationism is most commonly associated with Carnap's work on inductive logic and stands for the view that scientists should accept those theories that are most likely to be true given the available evidence. Falsification is usually associated with Karl Popper and maintains that scientists should try to falsify theories and reject those falsified such that only those that conform with the evidence remain. Both confirmationism and falsificationism were developed as ahistorical theories of scientific rationality. Should they fail to fit actual scientific practice, then so much the worse for the rationality of scientific practice. An opposed view is taken by historicism, which holds that a good theory of rationality must in some ways conform to the history of science. And should history suggest there is no universal criterion for scientific rationality, than so much the worse for philosophy. And such is the conclusion of one of the most prominent and excentric exponents of historicism, Thomas Kuhn.

Even a simple notion of means-ends rationality reveals that rationality in Kuhn's account is problematic. Let's call this notion 'optimizing rationality'. This conception of rationality states that an agent is rational if and only if s/he chooses that option which most efficiently provides the means to reach a given goal. Applied to Kuhn's account this implies that rationality can be situated at two different levels: the level within a paradigm and the level between paradigms. At the level within a paradigm, rationality is not problematic. The paradigm provides the goals for which scientists try to find the means to reach them. However, for Kuhn there is no single, paradigm-independent set of goals to decide between rival paradigms. This lack of a rational basis for paradigm-choice was responsible for the bulk of the charges of irrationality against Kuhn. If not brought about by the sheer construction of the problem, which appears to leave no possibility for rationality, then surely by the solution suggested by Kuhn himself. According to Kuhn, choice of a paradigm depends on "persuasion" (1970, 198) and, if successful, results in "conversion" (ibid. 150). This leads Kuhn to assert that "the superiority of one theory to another is something that cannot be proved in the debate. Instead, I have insisted, each party must try, by persuasion, to convert each other." (ibid. 198) In the absence of paradigm-neutral criteria for the acceptance of paradigms, social factors seem to be guiding paradigm choice. Choice based on persuasion' and conversion' seems to stand very far from a rational choice and indeed, according to Kuhn himself, misconstructions of this view is the main reason for the charges of irrationality against him. (ibid. 199) These charges came from scholars such as Dudley Shapere (1964), Israel Sheffler (1967), Karl Popper (1970) and Stephen Toulmin (1970). The interpretation of Kuhn responsible for this criticism is what Gerald Doppelt calls "the neo-positivist interpretation" (Doppelt 1978, 35). This interpretation focuses on paradigms as linguistic entities and thus understands incommensurability as an incommensurability of concepts. Scientists in different paradigms speak a different language and because there are no common concepts at all, translation, communication and rational argument between paradigms is radically impossible. Kuhn calls this interpretation "seriously misconstrued":

"the proponents of incommensurable theories cannot communicate with each other at all; as a result, in a debate over theory-choice there can be no recourse to good reasons; instead theory must be chosen for reasons that are ultimately personal and subjective; some sort of mystical apperception is responsible for the decision actually reached. More than any other parts of the book, the passages on which these misconstructions rest have been responsible for charges of irrationality." (Kuhn 1970, 198-9)

From the very beginning Kuhn has resisted such an interpretation. To contrast his view with the neo-positivist formulation of the problem of Kuhnian rationality, I will call his view the Kuhnian view. His reaction in the postscript to the second edition of the Structure of Scientific Revolutions to the neo-positivist formulation of the problem is to reformulate incommensurability not so much as an incommensurability of concepts (taxonomic incommensurability) but an incommensurability of values (methodological incommensurability; see Sankey & Hoyningen-Huene 2001, Carrier 2008). Scientists from different paradigms disagree about what the problems are and what counts as a solution. They disagree about the definition and weighting of the values used to evaluate theories. This renders cvommunication "partial" (Kuhn 1970, 198), but not impossible. As a consequence the neo-positivist formulation of the problem of Kuhnian rationality, which requires this impossibility, does not hold water.

Kuhn maintained that scientists were rational in coping with this circularity, but he never developed a specific account of rationality. He did formulate intuitions about how a solution might look like, but because these contained words such as 'conversion' and 'persuasion' the neo-positivists allegations of irrationality were only reinforced. On the view of rationality as conversion there are no explicit rules laid out for why scientists accept a paradigm and there is no benchmark for progress, two key aspects of most accounts of rationality. In what sense can this rationality be more than at best a sociological phenomenon? What guarantees does this form of rationality offer that scientists actually choose a good solution from a set of alternatives? Nearly half a century after the publication of the Structure of Scientific Revolutions, no satisfactory answers have been reached to these questions. Moreover, the last decade has seen very little work on historicist theories of rationality in general (Matheson 2008). This paper renews interest in the topic of historicist rationality and submits that Kuhnian scientists are indeed rational. However, their rationality is one of satisficing, not optimizing. I will demonstrate that this new view provides the unique combination of compliance with Kuhn's account whilst remaining a genuine form of rationality. The paper is structured as follows. First I describe the two main views on why the notion of rationality is problematic in the work of Kuhn, the neo-positivist one and the one Kuhn preferred himself. Secondly I introduce satisficing, contrast it with optimizing and show that it can solve the problems associated with Kuhn's version of the problem. In the third section I argue that the view that Kuhnian scientists are satisficers is consistent with Kuhn's work. Agents with a satisficing decision function are able to reproduce the typically Kuhnian features of aggregate scientific change: normal science, crisis and revolution. This is demonstrated through the constructing of an agent-based model in which agents satisfice between alternative standards.

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Incommensurability at work: identity and conflicting points of view in historical debate

Anton Froeyman (Universiteit Gent)

In this talk, I will present an analysis of a historical debate, the so-called Historikerstreit, which serves as an instance of Kuhnian incommensurability. This analysis confirms the Kuhnian point of view on incommensurability on two specific points. First, it shows a great deal of misunderstanding rather than disagreement. Different sides in the debate do not quarrel over factual claims, but rather over points of view. Second, it shows that the debate is not about what the answer to a given question is, but about which questions are legitimate ones in the first place. However, this analysis also adds a dimension to the Kuhnian framework: it turns out that the different points of view articulated in the Historikerstreit are connected with social and political identities, suggesting a strong connection between scientific points of view and non-scientific social identities. This point is further elaborated by a short look at a second debate, the so-called History Wars in Australia, which pushes the Kuhnian framework to its limits, suggesting a form of relevant incommensurability between scientific and non-scientific points of view rather than an incommensurability within the scientific community.

Which pattern of discovery?

Tjerk Gauderis (Universiteit Gent)

Formal analysis of hypothesis formation in scientific discovery generally results in a myriad of formal patterns. Some well-known examples are single fact abduction, abduction of generalizations, analogies, common cause reasoning, etc. However, the question which pattern a scientist confronted with an anomaly should follow, remains largely unanswered and is often considered to be implicitly clear, an idea based on the assumption that the formal structure of the anomaly and the background knowledge is unambiguous. The history of science teaches us that this assumption is untenable. Often competing hypotheses were of a very different formal kind, even when the proposers of these hypotheses worked in the same tradition or paradigm.

In this talk, I will focus on two cases in the history of physics, in which a persevering scientific puzzle both let some researchers propose a new entity and others retract a well-accepted principle. The first case is the continuous spectrum in beta-decay in the 1930s, an anomaly that led both to the neutrino hypothesis and to the proposal to restrict the universal principle of energy conservation. The second case I will discuss is the anomalous orbit of Uranus in the 19th century, which led to the hypothesis of a hitherto unobserved planet (Neptune) and the proposal to retract Newton's laws.

Contemporary philosophy of science incorporates a strong historical component in the analysis of scientific discoveries and acknowledges to some degree the following Kuhnian perspective: anomalies are perceived differently by scientists working in different paradigms. In this paper, I will argue for a strong version of this idea and claim that even scientists working in the same paradigm, often perceive the formal structure differently – this is often a result of their personal history in the field - and, hence, perceive the concepts and prioritize the elements of the theory slightly different. More in particular, I will, by analysis of the two historical cases, defend the claim that in physics hypothesizing new entities results from taking methodologically conservation laws as foundational, while retracting existing principles results from a preoccupation with the application of concepts. It is important to notice that this thesis concerns hypothesis formation processes, not the scientists themselves. It is possible that a scientist looks at the problem from a different perspective and comes up with another hypothesis via another formal pattern (although this requires a conceptual shift) or that he acknowledges that two formally different hypotheses are equally valuable and that a future experiment should decide.

Scientific disagreement, pluralism and Kuhn: Accounting for Harvey's circular motion of the blood
Laszlo Kosolosky (Universiteit Gent)

Aristides Baltas (2000) once claimed that the study of scientific controversies was philosophically underdeveloped because of the work of Thomas Kuhn. Though Kuhn did bring attention to scientific disagreements, Baltas alleged that he did so only under the "all-embracing" notion of a paradigm change, which led to the philosophical community overlooking the "finer details" of such disagreements. Goodwin considers Baltas to be correct in the sense that much of the philosophical interest in, and attention to, Kuhn's Structure of Scientific Revolutions has focused on the alleged incommensurability of settled modes of inquiry and the transitions between them. However, in his recent publication, Structure and scientific controversies', Goodwin emphasizes that, against traditional views on the matter, several important sorts of non-revolutionary scientific disagreements were both identified and analyzed in Structure. He starts with the observation that there are many scientific controversies that cannot be comfortably regarded as revolutionary controversies' in Kuhn's sense. In a typical case, several scientists who are unequivocally members of the same normal science tradition (and thus share a paradigm) have a persistent disagreement over a scientific issue that resists, at least for a significant amount of time, resolution by standard means.

In detail, Goodwin pinpoints two kinds of non-revolutionary disagreement underlying Kuhn's work: controversies of articulation and controversies of interpretation. In the former, the dispute centers on the ambiguous assimilation of a new phenomenon to core results. In the latter, the dispute centers on what elements of the core must be surrendered to keep the paradigm afloat.

On the one hand, the dispute between Darwin and Wallace seems like a fitting example of what Goodwin referred to as a controversy of articulation. The dispute arises out of the difficulties encountered by both in assimilating a class of phenomena - dramatic sexual dimorphism - that they both suppose their approach should explain. They pursue different strategies in relating these phenomena to the core of consensus results acknowledged by both. Whereas Darwin sees a novel mechanism and a new analogy to artificial selection, Wallace sees more elaborate consequences of a species adapting to its environment. On the other hand, the Copernican Revolution, overly discussed by Kuhn himself (1957), could be seen as an example of a controversy of interpretation. While it is correct that Aristotelians at the time "made changes that went well beyond what could be described as the articulation of the Aristotelian paradigm or exemplar" (Ariew 2009, 297), it seems plausible to regard these changes as reinterpretations of the paradigm in the face of persistent and significant anomalies. Recent work on the Copernican Revolution has brought out details about these conservative adherents to Aristotelian cosmology, who thought they could accommodate Galileo's observations by modifying their current system as opposed to rejecting it in favor of heliocentrism. Ariew here describes the work of Jacques du Chevreul who (Ariew 2009, 296):

"managed to accept the observations made by Galileo with the assistance of the telescope, but did not regard these phenomena as evidence for either the Copernican or the Tychonic system. He accepted Galileo's observations from more or less within the framework of Aristotelian cosmology."

This quote is particularly interesting as the comparison to another historical episode of controversy and disagreement in science, not discussed by Kuhn nor by Goodwin, is striking: i.e. William Harvey's discovery of the circular motion of the blood. In particular, Jacques du Chevreul's reasoning reemerges in the work by Harvey's mentors Realdo Colombo (1516-1559) and Girolamo Fabricius (1537-1619). The former is regarded as the discoverer of the pulmonary transit of the blood, whereas the latter introduced the notion of valves in the veins. The analogy becomes evident as both of them successfully attempted to incorporate their results in Galen's general physiological framework, the predominant paradigm at that time. So to speak, both managed to avoid any major disagreement as their theses got taken on fairly quickly amongst their peers. Both examples thus serve as rather clear-cut examples of controversy of interpretation, in Goodwin's terms.

Harvey (1578-1657), however, when proposing his idea that the blood flow could only be understood in terms of a circular motion, does not seem to fit in any of the categories of disagreement defined above. In this paper, I will show how Harvey's thesis and the quarrel he had with its peers could neither be captured in terms of revolutionary science (as he opted to use similar techniques and vocabulary as his predecessors did,), controversy of articulation (as there is no new phenomenon at play,) nor controversy of interpretation (Harvey's well-known quantitative argument' but makes sense when abandoning Galen's physiological framework,). Harvey, although lacking an overall framework, thus takes a leap of faith when introducing his views, which he tries to back up among others by performing vivisection, using analogies, executing ligature experiments and devising distinctive thought experiments. Particularly his quarrel with Hofmann on the purpose of the motion of the blood illustrates the distinctiveness of this historical example of controversy in science (Kosolosky & Provijn, under review; Provijn, 2013).

Apart from backing up the claim on disagreement in the Harvey example, the second part of the paper addresses to what extent Kuhn's notion of scientific disagreement is able to tackle historical examples as such, arguing at the least for a supplementary notion of controversy apart from the ones described by Goodwin. These insights could moreover shed light on the notion of pluralism in Kuhn's work and in philosophy of science literature in general. In particular, if time permits, I tackle the question whether disagreement/dissent among scientists is at all required in order to have a diversity of theories, by drawing on previous results from research on the pursuit worthiness of theories (in Kuhn), which claim that scientists may agree that different theories are worthy of pursuit in a given domain and yet each of them may engage in a pursuit of only one of them (uSešelja, Kosolosky & Strasser, 2012; uSešelja & Strasser, under review).

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Revolution as evolution. The concept of evolution in Kuhn's philosophy and contemporary historiography of science
Jouni-Matti Kuukkanen (University of Leiden)

American homeopathic psychiatry and the promise of patient-centred asylum care: the Middletown State Homoeopathic Hospital (1870-1910)
Valérie Leclercq (Université Libre de Bruxelles)

After the Civil War, American homeopaths attempted to invest the field of institutional psychiatry. At that time, American asylums were going through their first severe crisis. While the population of State hospitals kept increasing dangerously, the medical profession and the public were slowly losing faith in the efficiency of mental treatment. Furthermore, as therapeutic ideals gave way to custodial measures, American asylums suffered the negative publicity of several mistreatment scandals and as a consequence found themselves under the fire of passionate outside criticism.

In that context, homeopaths put forward their project as a benevolent and promising alternative. To gather support from the public, the State and the homeopathic community, they insisted on what had always been one of the main selling arguments of the "New School" theory and practice: humane and highly individualized treatments. Indeed, in addition to offering therapies much less aggressive than those of their regular' counterparts, homeopathic physicians also believed that each disease was unique to each sufferer and thus called for a unique form of treatment. Relying almost exclusively on patients' words and symptom accounts, homeopathic practice positioned the sick person at the centre of its application and organization. By promising the importation of a similar patient-centred therapeutic dynamic into the context of state-supported mental medicine, homeopaths hoped to try their hand at a medical discipline they thus far had been prevented from entering, to offer a refuge to their mentally-afflicted followers and perhaps even to succeed in resolving the complex social and medical issue of mental alienation.

Based on a short research project carried out around the *Middletown State Homoeopathic Hospital (N.Y.)* – one of the few asylums eventually established in the United States by homeopaths at the end of the 19th century – this short presentation will expose the difficulties with which New School asylum specialists were confronted in their ambition to provide their inmates with a comfortable environment and a set of highly individualized therapeutic options, detailing homeopaths' attempt – and ultimate failure – to re-value the participation of mental patients in their own recovery process.

This can also be the occasion to reflect on the place of patients in the era of scientific medicine.

Paradigm lost. How scientists change their minds and survive scientific revolutions

Geerdt Magiels (Vrije Universiteit Brussel)

When Jan IngenHousz in 1779 discovered the process which we now call photosynthesis, he did so in the conceptual framework of the phlogiston theory. At about the same time Antoine Lavoisier was rethinking the chemical elements and their reactions and was about ready to put the puzzle of chemistry back together in a radically new way. Fifteen years later, IngenHousz reformulated his theory on the ecological relationship between plants, animals and the atmosphere in the terminology of the new chemistry. He successfully bridged two supposedly incommensurable paradigms. He is an interesting and hardly known example of a scientist who changes his mind on the basis of data and rational arguments. It probably is very much in line with what Kuhn intended with his 'paradigm shift' and helps to shine new light on the way science works.

This eighteenth century (r)evolution from the old the the new chemistry was one of Thomas Kuhn's paradigmatic examples of a paradigm shift in his The structure of scientific revolutions. Since then, paradigms have been shifting, both within science and all over society. It has become a household word to describe anything that changes (theories, styles, viewpoints etc.) and changes abruptly and irreparably. It has become so clich that the web magazine Wired elected it in summer 2009 to be thrown down a black hole together with the 'silver bullets' and the 'holy grails' of this world.

Kuhn's concept of a paradigm and its shift has been under heavy flack from many sides and has been a point of controversy in the philosophy of science for all those years. On the basis of some hitherto unknown archival material, this paper will attempt to approach the discussion from a fresh angle, based on a close look at the works and thoughts of some natural philosophers who contributed to the discovery of photosynthesis in the second half of the eighteenth century. This discovery of photosynthesis, arguably the most important biochemical process on earth, has been greatly neglected in the history and philosophy of science. This is a curious phenomenon as such, but what makes it even more intriguing is that this discovery took place at the time of and was closely linked with the overthrow of phlogiston by the new chemistry of Lavoisier.

In 1779, IngenHousz was a true phlogistonian in the wake of Priestley, one of his friends and colleagues and the one of the main defenders of the phlogiston theory. IngenHousz, a physician from Breda in the Netherlands but at that time working in a country estate in Southall Green near London, described the results of his experiments in a phlogiston-world: plants were producing dephlogisticated air (now known as oxygen). However, by 1794 he endorsed the new terminology from Paris fully, proving himself to have become a 'new chemist', applying this new theory to reframe his explanation of photosynthesis: plants used carbondioxide and water to produce their vegetable matter and release oxygen, necessary for all other organisms on this world to survive.

It is illuminating to reconstruct the way he and some of his contemporaries have been thinking, how they came to conclusions based on the facts they observed, and which were the assumptions they were making to knit everything together. It illustrates how science does not follow a clear-cut straight logical path from neutrally observed facts to unavoidable conclusions, a causal explanation of those facts. In reality, the building of a scientific explanatory theory follows a tortuous road, led and often misled by assumptions, intuitions or theoretical constructs. Investigators are often puzzled by contradictory or unexpected results, which would not end up appearing that contradictory or unexpected if they had known more about the real relationships hidden in the phenomena they studied.

In the course of less than fifteen years, IngenHousz changed his chemical perspective on the world. This is a far cry from the idea that old paradigms only disappear with the death of the last pensioned professors believing in them. It is also different from the idea that the switch from one paradigm to the other is based on irrational, argumentative elements. IngenHousz shows in his step from phlogiston to oxygen that it can be perfectly rational for a man to change his point of view. The new system offered him a better toolbox to handle the problem in front of him. One of his follow natural philosophers and fierce competitor was Jean Senebier who made the same switch, albeit a little quicker. In the texts of both men, one can see how they use both terminologies together, side by side or separately over a period of more than ten years. Senebier wrote in his 1788 book: "I propose nothing that is unique or unheard of in chemistry when I say that sunlight decomposes the fixed air contained in the leaf. I am no less consistent with the principles of sound chemistry when I say that the carbonaceous substance or phlogiston is combined in the plant with the resins." [my italics] Senebier, just as IngenHousz, used the two terminologies together. They did this as if they were translating the concepts in their minds, or on behalf of their readers, to make it easier to understand what they were talking about. Others would make the switch later still, or not at all, partly depending on their personalities, education, professional or social context. Priestley stuck to phlogiston until his death; Jean Baptiste de Lamarck would end his opposition to the Lavoisierian system in 1802. Another great biologist, Lazzaro Spallanzani, was very quick in recognizing the value of the new system to understand processes of decomposition and recomposition in living organisms.

There is another factor of consistency that spans this period of 'paradigmatic change' and bridges the supposedly unbridgeable interparadigmatical crevasse. All men involved in this story were using the same scientific instrument. The instrument for studying 'airs' of all sorts and how they were transformed, was the eudiometer. In 1805 De Saussure was still using eudiometers to measure the amount of oxygen in his air samples, just as IngenHousz had been doing fifty years earlier. The apparatus was slightly improved compared to the primitive tube Priestley first used, but the principle was the same. So, on an instrumental level, things stayed pretty much the same. At that level all people involved were talking about the same thing. That is probably why Priestley and other phlogistonians were well equipped to understand what those fashionable chemists from France were saying. So much so that they could think about what their opponents next argument would be.

The history of plant physiology and chemistry in the late eighteenth century shows how Kuhn's version of the history of science (or the caricature that has been made of it) leaves room for improvement. The so-called incommensurability between two paradigms dissappears as an artifact when one looks at science as a process of gathering trustworthy knowledge, and does not consider the theories just as end products.

In many ways Leonard Nash's 1952 account of the research on plants and the atmosphere (virtually the only thorough study of this chapter in the history of science for a century) is very inspiring as it was written before Kuhn's conceptual thunderstorm hit the philosophy of science. Nash (who was one of Kuhn's mentors) illustrates how Priestley, IngenHousz and Senebier tried to understand what they saw happening in front of their own eyes. And how difficult it was to do that properly, in order to get reliable knowledge. He shows how time and energy consuming it is when people try to come to intellectual grips with a complex world that is out of reach of the theoretical framework they have at their disposal. So they start again and again, with trial and error, and with all the creativity they can generate. With a critical eye on their methods, knowing that they can't know everything, as the writings of IngenHousz, Senebier and De Saussure amply illustrate. Scientist were no know-it-alls with the hubris to think they could understand everything. Just as they still aren't today.

The conceptual evolution in the minds of IngenHousz and Senebier shows how scientific evolutions have to be seen more as organic changes in complex systems, rather than as cataclysmic breaks in theoretical thought. The concept of a paradigm shift has mutated beyond recognition.

The History of Physiology at the Dawn of the Scientific Revolution – Some Issues and Methods
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Related to the history of medical ideas in the 16th and 17th centuries, the present paper aims at appraising the role of medicine in the development of early modern corpuscular theories, leading to Cartesian mechanism characteristic of the Scientific Revolution. Firstly, it will examine the early modern neo-atomism, i.e. the early modern approach to natural philosophy combining ancient atomism (Democritus, Epicurus, Lucretius), Renaissance Neoplatonism and iatrochemistry (Paracelsus), and mechanism of the classical age (Descartes). Secondly, it will study the confrontation between neo-atomism and the Galenic medical tradition taught in the universities since the 12th-13th centuries. Thirdly, it will survey the recent historiographical trends related to this research in history of matter and chemistry, and history of early modern medicine. Finally, it will discuss the epistemological status of the history of early modern physiology as related to the concept of Scientific Revolution. It seems relevant to reappraise the development of physiology at the dawn of the Scientific Revolution, as it is located at the crossroads of (1) the Galenico-Avicennian tradition transmitted by the universities, (2) the interest in natural philosophy and its medical implications, (3) the development of the new corpuscular science in the beginning of the classical age. This will allow us to question the concept of Scientific Revolution as defined by historians of philosophy (Koyr, Butterfield) or philosophers of science (Kuhn), and cast new light on the importance of medical knowledge, particularly physiology, in the development of the new science of the 17th century.

Thomas Kuhn on Scientific Change: From History to Dynamical Theory
Thomas Nickles (University of Nevada)

The postgenomic era: a Kuhnian revolution in genetics?
Laurence Perbal (Université Libre de Bruxelles)

The today genetics is characterized by a new epistemological era: the postgenomic era. The typical theoretical and practical approaches question the genes eye view and methodologies become all causes embracing (genetic, environmental, epigenetic, etc.) and systemic (proteomics, interactomics). We will enlighten these epistemological developments with research on the genetics of anti-sociality and try to identify the social, historical or scientific reasons of such evolution. However, is the emergence of postgenomics the sign of a paradigmatic revolution in Kuhnian sense of the term? This paper offers to answer this question.

Medical film traces in Brussels and Vienna in the early twentiety century
Katrin Pilz (Université Libre de Bruxelles)

In course of my dissertation project, I examine a representative selection of medical film documents and related media in Brussels and Vienna and their function as a new visual technology for popularizing and communicating knowledge. Historically, Berlin and Paris stood out as leading medical cities - and hence role models for bordering countries, and their urban capitals; Brussels and Vienna are notable examples of such. Drawing on institutional and visual observations, I will examine the function of the cinematograph as a new media apparatus, the introduction of film into the medical scientific community, and its impact on new scientific and academic structures, as well as medical specialist disciplines. The production of medical films emerged in the early 20th century in both medical and popular spheres in the west countries. Early cinematography and scientific medical imaging are strongly connected : the medical and scientific films are directly related to early historical cinematography, and are part of the first experimental phase in film production. The medical film was seen most notably as a chance to record and show vivid movements of the body, whereas until then the dead, dissected, pathological body had been the centre of attention in medical visual culture and practice . In some cases, the initial euphoria of medical cinematography seemed to decline over the following decades as a result of political sanctions and the lack of missing financial support.

In the history of medicine, traditional visual artefacts such as medical lithographs, wax models, microscopic photography, and radiographs have put a particular emphasis on disease, dissection and surgical techniques, and on innovations that have a significant reason. The cultural aspects of looking in science and medicine have a particular position within the visual culture debate. Scientific images are considered to be research evidence that represents objectivity, but scientific imagery cannot be perceived in isolation from its cultural context or authority; rather, it must be seen as an explicit visual actor that is affected by the interplay of power and knowledge of co-existing spheres. Visual media play an essential role both in medical education and training within the scientific community and in the dissemination of knowledge about the healthy and diseased body in the public sphere. These media have had a substantial impact on the history of visual culture as well as the history of science and medicine. In the late 19th and early 20th centuries, new technical innovations, such as the invention of cinematography, X-rays and the increased use of photography in medical journals and literature, established a new media culture in medicine. Since the beginning of the 20th century, photography and the film camera allowed the documentation of new medical techniques and skills. The radiograph was the first technically produced image of the inside of the body and resulted in the construction of many myths, not only in the medical scientific community, but also in public culture. Over the last twenty years, the visual or pictorial turn has been introduced to disciplines within the humanities and history. Consequently, the turn towards visual sources in the history of science, technology and medicine has followed and opened new perspectives on the social culture of medicine. The discovery of the X-ray by Wilhelm Rntgen and the invention of the cinematographer by the brothers Lumire, both in 1895, gave rise to a new gaze and new speculation about the application of these technologies. These innovative imaging technologies of scientific knowledge thus had an impact on industry, culture and politics. I will debate how scientific knowledge has been generated by medical films and how the functional stages of medical cinematography have appeared based on processes of knowledge acquisition. I consider specific academic structures in Brussels and Vienna, as well as the influence of visual culture in medicine, as fundamental for the emergence and application of medical cinematography, and also address the medical films contribution to particular medical specialist disciplines.

Between monism and pluralism : some strategies for emergentism
Olivier Sartenaer (Université Catholique de Louvain)

In 1875 the British philosopher George Henri Lewes introduced the concept of emergence in the philosophical literature. This concept, intended to suggest the idea of "an apparent discontinuity grounded in an actual continuity" – in the way an iceberg is emerging from water – constituted the core of a new philosophy of nature that explicitly claimed to constitute a middle ground between the antithetical views that were, on the one hand, monist materialism inherited from Democrituss atomism and, on the other hand, modern versions of Platos substance dualism. By claiming to be the defenders of such a conciliatory view, Lewes and the subsequent "British Emergentists" developed their philosophy on the basis of a fundamental tension. They had to conceptualize a kind of natural creativity – in continuity with French spiritualists like Bergson – without giving up the materialist ideal of scientificity; they had to provide a view that pays tribute to the monist and determinist commitments of modern sciences without eliminating from nature what seems to be genuinely novel or unpredictable.

The constitutive tension of British Emergentism – holding at the same time some form of natural discontinuity and continuity – crystallized in different scientific controversies in the beginning of the 20th century. For instance, in the field of biology, the "emergence of life" was meant to capture the materialist thesis of the dependence of living organisms on a physico-chemical basis while arguing that living systems are not identical to physico-chemical systems. Emergentists were thus positing themselves in the "no mans land" between, on the one hand, dualist vitalists postulating – la Bergson or Driesch – the existence of an irreducible vital stuff like lan vital or entelechy and, on the other hand, monist (iatro-)mechanism directly inherited from the cartesian concept of animal machine. In the field of mind sciences, British Emergentists held that "the mind emerges from the body", asserting in this way an actual continuity in the mind-body relationship (mental properties depend on neurophysiological properties), but also a kind of discontinuity between such entities (mental properties are not neurophysiological properties). Emergentists were then defending a middle ground doctrine between versions of Cartesian interactionist dualism (committed to the existence of radically heterogenous stuffs like res extensa and res cogitans) and Spinozist monism typical of materialist neuroscientists like – for instance – proponents of Galls phrenology.

An immediate question arises here : is such a middle ground between radical monism and dualism philosophically viable or consistent ? In other words : is it possible to hold together a certain form of natural continuity and discontinuity ? The main objective of the proposed talk will be to give an insight of the ways contemporary emergentists answer these questions. In particular, on the basis of a purely conceptual distinction of different levels of tension between monism and pluralism (the levels of substances, properties and predicates), I will provide a framework allowing to understand the main emergentist strategies that make the ideas of continuity and discontinuity compatible. These strategies will then be associated with different and frequently discussed concepts of emergence, like for instance theoretical, explanatory or ontological emergences. The overall upshot of such a conceptual analysis will be the building of a taxonomy that allows to clear up the nebulous debates pertaining to the reductionism issue.

Textbook Science: Kuhnian Times, Uncertain Futures
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