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BOOK OF ABSTRACTS

[2] Don’t be a Demarc-hater: Correcting Popular Misconceptions of Popper’s Demarcation Criterion and Demarcation Problem

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Philosophers of science often fail to properly characterise a historical problem-situation in history of philosophy of science. I address two clear cases: popular misreadings of Sir Karl Popper’s (1) formulation of the problem of demarcation and (2) proposed demarcation criteria in his *Logik der Forschung* (1934/5). By examining this historical case-study, we see how a relatively recent philosopher of science has been misread by later generations of philosophers of science to the detriment of history of philosophy of science.

In the 1950s Carl Hempel claimed demarcation criteria were bound to be both *too restrictive* and *too permissive* to be suitable solutions to the demarcation problem: some pseudo-scientific theories qualify as ‘scientific’; some paradigmatic scientific theories do not. We can call this the *objection from ill-fit*. This objection is widespread in the philosophic literature, and widely assumed today to be a strong objection to proposed demarcation criteria set out by Popper. Consequently, a vast majority of philosophers of science have set Popper’s demarcation criteria aside as an intellectual dead-end, often limiting any coverage of Popper to introductory classes in philosophy of science.

I argue the objection from ill-fit is spurious when directed at Popper’s demarcation criteria. In fact, clear textual evidence shows Popper’s problem of demarcation bears no resemblance to the demarcation problem addressed by Hempel. I then set out numerous other examples of philosophers of science that routinely mischaracterise Popper’s first formulation of the demarcation problem set out in *Logik der Forschung* (1934/5). In actuality, Popper set out in *Logik* two criteria for demarcating *empirically significant* from *empirically non-significant domains of discourse*, not between science and non-science.

I argue the prevalence of the objection from ill-fit (as well as other objections) s likely due to a mistake in textual exegesis: in *The Logic of Scientific Discovery* (1959), Popper translated a key German technical term, ‘*empirischen wissenschaft*‘ as ‘empirical science’, rather than ‘knowledge gained by experience’. Consequently, many of his critics mistake the aim of his stated demarcation criterion to delineate the boundaries between the natural sciences and non-science. I then locate the key passage that likely lead to this mistake in *The Logic of Scientific Discovery*.

I then tackle the problem that philosophers of science routinely misstate Popper’s stated demarcation criteria: Popper explicitly presents *two* demarcation criteria in *Logik*, *not* one. The first criterion, *falsifiability*, applies only to systems of sentences; the second criterion, *predictability of basic statements*, applies only to individual sentences. I elaborate on how these criteria bear no resemblance to how they are presented in the literature.

I then address another conjecture explaining the popularity of the misstatement of Popper’s demarcation criteria: Popper chooses to refer to these two criteria by similar names, and does not frame his criterion of predictability as one of predictability, rather, as one of falsifiability.

I conclude that while the objection from ill-fit may be effective against *some* territorial criteria, no philosopher should continue to present the objection from ill-fit as targeting *this particular* problem of demarcation or Popper’s proposed demarcation criteria.

[5] Hegel's Proto-Modernist Conception of Philosophy as a Science: A Critique of Alan Richardson's Account of the Rise of Scientific Philosophy

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In his paper "Towards a History of Scientific Philosophy" (1997), Alan Richardson attempts to characterize what is distinctive about the late nineteenth and early twentieth century movement of "scientific philosophy". Taking Husserl and Russell (among others) as paradigmatic "scientific philosophers", Richardson describes their philosophizing as animated by what he calls the "modernist sensibility". According to Richardson, philosophers animated by the "modernist sensibility" conceived of philosophy (as a science) as "an intrinsically collaborative project, built by workers relying on the methods and results of their fellows, striving to produce clear, intersubjectivity understood and accepted results" (Richardson 1997, 434). In order to draw a contrast between this modernist view of philosophy and previous views of philosophy as a science, Richardson points to Hegel as the paradigmatic pre-modernist philosopher. According to Richardson, while Hegel conceived of philosophy as a science (on this point he would have had no quarrel with Husserl and Russell), he thought of it as a science that relied on individual genius and as a science whose propositions could not and should not be made accessible to non-philosophers (to the "common people"), and that it was not a collaborative project, but rather a thoroughly individualistic intellectual enterprise. I argue that contrary to what Richardson thinks, Hegel held a proto-modernist (in Richardson's sense) conception of philosophy as a science. In particular, I argue that the views that Richardson attributes to Hegel stem from a conflation of Schelling's views with Hegel's own views. I argue that not only did Hegel personally reject individualistic and esotericist conceptions of philosophy as a science, but that he also attempted to narrate the history of philosophy from an anti-individualistic standpoint in his *Vorlesungen über die Geschichte der Philosophie*. If Richardson's portrayal of Hegel as the paradigmatic pre-modernist philosopher is inaccurate, then his account of the emergence of "scientific philosophy" will require some revision, in so far as we would have to account for the existence of a proto-modernist conception of philosophy as a science in the early nineteenth century. Furthermore, I argue that because Richardson mischaracterizes Hegel's conception of philosophy as a science, he ends up omitting an important strand of nineteenth century and twentieth century philosophy whose proponents thought of themselves as engaging in "scientific philosophy", and who thought of themselves as further developing the modernist elements in Hegel's conception of philosophy as a science. This strand of philosophy is Marxist philosophy (which should not be reduced to what is sometimes called "Western Marxism"). I show that the concept of a "modernist scientific philosophy" can help us categorize some works in Marxist philosophy that cannot be categorized using the "analytic"/"continental" dichotomy, e.g., Engels' *Ludwig Feuerbach und der Ausgang der klassischen deutschen Philosophie*. I also emphasize the historical connections between important figures in Marxist philosophy (especially in the Soviet Union) and key figures in the movement of "scientific philosophy" (as Alan Richardson understands it), e.g., the fact that A. V. Lunacharsky attended Richard Avenarius' lectures and was deeply influenced by his empirico-criticism.

[7] British Idealism and Science: May Sinclair on Spacetime

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At the turn of the twentieth century, idealism dominated Anglo-American philosophy. A few years later, idealism was overpowered by new realisms and pragmatisms (the roots of today's analytic philosophy). Unlike new realists, idealists are often said to be uninterested in science. For example, W. J. Mander's *British Idealism* states:

Realist and pragmatist philosophy not only modelled itself on science, but engaged with it at all levels, developing a worldview that could sit happily with its latest results. The Idealists, by contrast… had little interest in science. (Mander, 2011, 547)

Whilst it is true that *most* idealists were uninterested in science, there are exceptions. This paper will explore one of them: a late British idealist named May Sinclair.

May Sinclair (1863-1946) is well known to literary scholars as a best-selling novelist. She is currently unknown to history of philosophy, yet she published several philosophy articles and two books: *A Defence of Idealism* (1917) and *The New Idealism* (1922). These books were positively received by the likes of Bertrand Russell and John Laird, and she spoke at the Aristotelian Society. I will focus on Sinclair's second book, where she argues that idealism can only survive by taking on elements of realism. One of these elements is the seriousness with which realists approach new developments in science around spacetime.

In 1900, Minkowski stated, 'Henceforth, space by itself, and time by itself, are doomed to fade away into mere shadows, and only a kind of union of the two will preserve an independent reality'. In the early twentieth century, philosophers were just beginning to understand the idea that space and time should be unified as a four-dimensional spacetime manifold. The new realist Samuel Alexander was among the first to grapple with it, and he produced a new kind of metaphysics, on which a unified spacetime sits on the fundamental level of reality. Alexander's *Space, Time, and Deity* claims:

Our purely metaphysical analysis of Space-Time on the basis of ordinary experience is in essence and spirit identical with Minkowski's conception of an absolute world of four dimensions, of which the three- dimensional world of geometry omits the element of time (Alexander, 1920i, 87).

Sinclair took this new work to heart:

by now it has become pretty evident that they [Space and Time] must be taken together. Professor Alexander has shown most convincingly that Time enters into the very structure of Space… It underlies the equations of modern physics in which Space and Time appear as interchangeable terms. It is the principle of the Principle of Relativity (Sinclair, 1922, 220).

And yet Sinclair maintains that we should be idealists rather than realists. As this paper will explain, she does so by discussing various features of spacetime, and arguing that they can *only* be explained by regarding spacetime as a simple kind of consciousness.

Sinclair's account of spacetime is radical, but it is hardly uninterested in science.

[8] Metaphysics and Method in Émilie Du Châtelet

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In her *Institutions physiques* of 1740, Madame Du Châtelet exhibits an appreciation for metaphysical topics hailing from the Leibniz-Wolff tradition and for topics from physics, especially gravity, hailing from the Newtonian one. The common view that she specifically provides a "Leibnizian" metaphysical foundation for "Newtonian" physics, however, is too simplistic. I argue that she actually developed a more subtle view, with two main points. First, Newton and Cotes left behind a confusion: did Newton's contention that all material bodies gravitate toward one another mean that gravity is essential to matter? In the *Regulae Philosophandi*, Newton famously denied that he had made that claim, but he never clarified what it meant to speak of matter's essence in the first place. Cotes muddied the waters with his famous preface to the second edition of *Principia mathematica*. In response, Du Châtelet discusses essences in detail in an early chapter of the *Institutions* in order to clarify why Newton has not shown that gravity is essential to matter, as some (such as Voltaire) had claimed. Hence she deftly uses a seemingly abstract metaphysical discussion in order to clarify the meaning of the most important conclusion of physical theory at that time. Second, and similarly, Newton had noted that he did not know the "reason" that gravity is proportional to mass and inversely proportional to the square of the distance between bodies. He then famously added: "hypotheses non fingo." In lieu of any detailed methodology in *Principia mathematica*, many Newtonians took this rejection of hypotheses as the key to Newton's method. But Newton's mechanist interlocutors and critics, especially figures like Leibniz, could not accept his theory of universal gravity *per se*, preferring instead a vortex theory of planetary motion. Indeed, in his *Tentamen*, published in the *Acta Eruditorum* in 1689, Leibniz introduces the motion of a vortex in the planetary system *ex hypothesi*. That is, he explicitly proceeds through the introduction of an hypothesis. However, like Newton, Leibniz also failed to provide any general discussion of methodology and of the appropriate use of hypotheses. In response, Du Châtelet recognizes the importance of a general discussion of hypotheses, arguing that we can neither ban all hypothetical reasoning from philosophy, nor introduce hypotheses à la Leibniz without any empirical evidence to support them. These two points intersect: in each case, Du Châtelet uses metaphysical and methodological topics to enrich her approach to understanding a principal problem in physics, namely the nature of gravity. She shows that both the Newtonian tradition and the Leibnizian one overstep their bounds: the former wrongly claimed that gravity's "seat" is matter itself, since gravity is essential to matter, and the former wrongly claimed that its seat is the vortices in the heavens. Her middle way approach between these two extremes is subtle and creative.

[10] Kant and the Science of Empirical Schematism

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Recent Kant scholarship has highlighted the important role that Kant's interest in Newtonian physics and the foundations of Euclidian geometry plays in the critical philosophy (Friedman (1992, 2014); Shabel (2003, 2006); Sutherland (2004a; 2004b)). Underappreciated, however, is the fact that other features of Kant's contemporary scientific milieu played a similar role. Here, I examine the influence of Kant's interest in the life sciences of his time. In particular, contemporary work on the classification of biological kinds, specifically the work of Buffon and Linneaus, had an important influence on Kant's account of empirical concepts in the *Critique of Pure Reason*.

Central to Kant's account of concepts is the often poorly understood notion of a schema—a representation that is supposed to mediate between concepts and objects. In the "Schematism" chapter of the first *Critique*, Kant gives a single example of an empirical schema, that which belongs to the concept <dog>. The schema of this concept is "a rule in accordance with which my imagination can specify the shape of a four-footed animal in general" (A 141/ B 180).

Kant's brief account of empirical schemata in the "Schematism" chapter of the *Critique of Pure Reason* raises two questions. First, it is unclear why empirical concepts need schemata; after all, such concepts are formed on the basis of sensible representations of objects. Second, it is not clear how schemata differ from empirical concepts; Kant describes both as "rule[s] for the determination of our intuition" (A 141/ B 180; A 106). The prevailing answer to these questions in the scholarly literature is that concepts are discursive rules for combining predicates while (empirical) schemata are rules for the kind of perceptual processing that underwrites the recognition of objects and first makes concept formation possible (Chipman (1982); Longuenesse (1998); Allison (2004); Ginsborg (2006); Matherne (2015)).

Scholars have overlooked the fact that Kant's single example of an empirical schema is biological and that he emphasizes a mark—<four-footed>— that plays a crucial role in the Aristotelian and early modern taxonomy of animals. By looking at Kant's discussion of four-footedness in the Moscati review and the lecture notes for his course on Physical Geography, we can better appreciate that Kant's interest in the classification of biological objects underlies his use of the example of the concept <dog>. Appreciation of this fact suggests an alternative account of the way in which schemata function as perceptual rules. I suggest that empirical schemata are methods for the classification of objects on the basis of their spatio-temporal form, in particular, *shape* and *number of parts*. We can take Linneaus' method for the classification of plants—in which the number of pistils and stamens is used to determine order and class— as a prime example of what Kant has in mind. An important result of my interpretation is that it brings Kant's account of empirical schemata much closer to his account of pure sensible schemata than has hitherto been recognized.

[13] Science in the making: Hélène Metzger and disciplinary history

Chimisso, C. (*The Open University*)

Hélène Metzger (1886–1944), the leading historian of chemistry of her generation, extensively wrote on the historiography of the sciences and the philosophical lessons that history of science affords us. Notably, she is one of the scholars whom Thomas Kuhn mentioned as inspiration for *The Structure of Scientific Revolutions*. In this paper, I shall focus in particular on a tension in her approach. On the one hand, she painstakingly endeavoured to avoid anachronism (not her term), to the point that she believed that the historian should make herself the 'contemporary' of the scholar whose work she studies. She thought that the historian should not only understand the material world, concepts, taxonomy, worldview and metaphysics of past scholars, but also their emotions, habits and ambitions. Indeed, for her the historian should even employ empathy in order to connect with past readers of chemical texts. Moreover, she launched scathing attacks on the historians who used the concept of 'precursor', and looked in the past for 'anticipations' of modern discoveries. On the other hand, she deliberately focused on the formation of disciplines, namely crystallography and chemistry. In other words, she studied practices, ideas and theories of scholars who had no understanding of being 'crystallographers' or 'chemists'. These scholars had no definition or classification of crystals, did not distinguish them from organic substances, and sometimes only wanted to fill their cabinets with marvellous objects. Her pharmacists, medics, alchemists and amateurs who liked to experiment did not aim to create the discipline of chemistry. Did Metzger strive to be accurate in the detail, while constructing a completely anachronistic history? Does any historian of science who constructs long narratives and who regards natural philosophy as the past of modern science commit anachronism? This second question has attracted a vast literature, which I shall not discuss here. Rather, I shall answer this question by evaluating Metzger's project. I shall argue that there is no fundamental contradiction in her aims; rather, there is a tension that is in fact inevitable in the historian's work.

[14] Moral necessity as 's Gravesande's argument for the knowability of the natural order

Van Besouw, J. (*Vrije Universiteit Brussel*)

This paper challenges the scholarly consensus on Willem Jacob 's Gravesande's philosophy, namely the view that it was predominantly influenced by Locke, Descartes, and Newton. In philosophy, Willem Jacob 's Gravesande (1688-1742) is mostly remembered for his epistemological and methodological defence of the new experimental physics of the eighteenth century. 's Gravesande asserted that what he himself called "Newtonian physics" could yield certain knowledge and defended this statement by reference to God's goodness. While this reference is the supposedly 'Cartesian' part of 's Gravesande's philosophy, its 'Lockean' part would be found in his claim that we cannot reach knowledge of the underlying causes of natural phenomena. This claim is often read as evidence for a general anti-metaphysical stance. Here, I will show that this is an incorrect reading and that ‘s Gravesande in fact provided a detailed metaphysical foundation for his epistemology. Moreover, I will point out how this neglected foundation calls for a reinterpretation of his philosophy in general.

Perhaps surprisingly given his dismissal of causal explanations in physics, the most important topic in ‘s Gravesande’s metaphysics is causality in general. This becomes most clear from his posthumous *Essais de métaphysique*. I will argue in this paper that ‘s Gravesande’s physics and metaphysics meet in the concept of laws of nature. It is well known that ‘s Gravesande argued that the aim of physics was to find such laws, which he conceived of as descriptions of natural regularities. What is not known, however, is that his metaphysics provides an answer to the question of why there are such laws at all and what the nature of the natural order is. As I will discuss, the *Essais de métaphysique* contain first of all an axiomatic-deductive argument for a global causal determinism, but also defend the idea that such a determinism is compatible with human and divine freedom. In a line of argumentation that has much in common with that of Leibniz’s *Theodicée* (1710), ‘s Gravesande argues that, although God’s acts might be morally necessary, this does not make them any less contingent.

This paper elaborates ‘s Gravesande’s argument in some detail, tracking it from the reconciliation of contingency and predestination to its final conclusions that deal with theodicy in an optimistic way. These conclusions, obviously building on discussions surrounding Leibniz’s work as well, underlie ‘s Gravesande’s argument for God’s goodness. As I will show, these arguments led ‘s Gravesande to claim elsewhere that, because of the moral necessity of God’s goodness, we can be certain that God has ordered the world in such a way that its regularities are knowable to us. That is, God has ordered the world by means of knowable laws of nature. In this way, ‘s Gravesande’s metaphysical discussion of God and causality, strikingly similar to that of Leibniz, in the end enabled him to explain why we could expect to find the laws of nature via “Newtonian physics”.

[15] Debunking Knowledge: Nietzsche’s Role in the History of Relativism

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Few historical discussions of relativism fail to count Nietzsche among its representatives and predecessors. “The saying, ‘There are no facts, only interpretations’ could serve as a motto for the relativist movement. It comes from late notes of Friedrich Nietzsche, probably the greatest figurehead of that tendency since Protagoras” (Blackburn 2005, 90). Regarding this reception it is noteworthy that he left traces in the domain of philosophy of science among members of the Vienna Circle and – more obviously – among opponents of the received view, such as Feyerabend, Hacking or Giere. In the light of this widespread and multifaceted reception the inclusion of Nietzsche into the history of debunking absolutist claims on knowledge seems well justified. Nietzsche’s relation to relativism, however, remains significantly unclear. Does he belong to the history of relativism or does he merely serve as a figurehead? He used the word ‘relativism’ only once in his published writings and never adopted it as a position. Essential features of relativism, however, and a number of arguments in favour of dependency, contingency, and limited validity of propositions are present within his work. Moreover, research in his readings and contexts uncovers his deep involvement with 19th century debates about the scope and validity of knowledge-claims and science. By means of a reconstruction of Nietzsche’s ‘relativisms’ and their contexts, this paper aims to shed light on the their 20th century receptions.

Nietzsche knew a positive usage of ‘relativism’ from the work of Friedrich Albert Lange, who understood relativism as the natural consequence of the contemporary developments in the natural and the historical sciences. Lange mainly grants it to the philosophical rigour or Kant and the intellectual talent of French thinkers "that today the exact science in all domains of experience no longer set up absolute truths, but only *relative* ones; that the conditions of the acquired knowledge are always recalled, and that the accuracy of all doctrine is justified on the *reservation of the progress of knowledge*" (Lange 1866, 244). The combination of empiricism and fallibism with a hypothetico-deductive understanding of science leads Lange to a certain concept of relativism. Auguste Comte defended "relativisme" already in 1855 (23; 102). In addition to Comte and Lange, Nietzsche drew many of his methodological and self-reflective conclusions from sources like Schopenhauer, Mill, and Spir, but also from Helmholtz, Du Bois-Reymond, Mach and others. He perceived a certain relativism as the advanced position of self-reflexive scientists and philosophers. On the basis of the human sensual organism, language, culture and values, a number of different successful interpretations of the world are possible and have been historically real. Reconstructing Nietzsche's role in the histories of relativism refines our understanding of this branch in the history of philosophy of science.

[16] On von Neumann's Use of Hankel's Principle of Permanence of Forms

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This paper investigates John von Neumann's requirement that the infinite dimensional algebra of quantum mechanics be a "proper" extension of its finite dimensional algebra, and focuses in particular on his justification of this requirement that is based on the so-called principle of permanence of forms.

Explicitly formulated by the Cambridge algebraist George Peacock in the first half of the 19th Century, this principle was further propagated by Hermann Hankel, whose conception of it was very influential in the German speaking academic world. But the principle of permanence of forms does not appear to be only one thing. For it has been regarded as a principle of theoretical rationality, i.e., one that is indispensable for the development of a genuinely scientific theory, but also as a principle of practical rationality, i.e., one that is merely thought to save brain energy in this development. Some conceived of it as a metaphysical principle, others as a merely semantic principle. Yet others considered that permanence of forms can play a probative role, and thus used it as an axiom in the derivation of mathematical results.

After exploring to some extent the historical roots of the principle of permanence of forms, carefully distinguishing its various interpretations and briefly discussing its influence, I turn to von Neumann's views on quantum mechanics around 1935, when he famously changed his mind on the Hilbert space formalism, which he had himself introduced a few years back, and proposed instead a formalism based on what came to be called von Neumann algebras. Sidestepping the good old discussion about the subjectivist interpretation of his projection postulate, my aim is to determine the role that the permanence of forms may have had in the development of his view of quantum mechanics.

That it did have an important role has certainly not been missed by commentators. Both Miklos Redei and Giovanni Valente, for example, note that von Neumann's insistence on preserving Dedekind's law of modularity is justified by Hankel's principle. What drove von Neumann to change his mind about the quantum mechanics formalism was not the desire to have a mathematically unobjectionable theory. For there was nothing mathematically objectionable in the Hilbert space formalism, or at least nothing as objectionable as Dirac's delta function, for example. What drove von Neumann was the desire to bring about (better) understanding of quantum mechanics. The present paper explains how the principle of permanence of forms can do that.

[24] Kantian Roots of Karl Popper's Scientific Methodology
Paitlova, J. (*University of West Bohemia*)

In the paper, the authors focus on an often overlooked Kantian influence over Karl Popper's early thinking aboutscientific methodology as they analyze the roots of Popper's philosophy within the Kantian tradition. The centre of their attention is Popper's first manuscript *Die beiden Grundprobleme der Erkenntnistheorie* (1932). In this text Popper interprets and criticizes Kant's transcendental approach. The given critique of synthetic apriorism has an impact on a way how Popper solves problems of induction and demarcation, which became crucial for his famous treatise *The Logic of Scientific Discovery*. It is the first aim of this paper to bring out critical reflection of Popper's critical rationalism in the light of Kantian philosophy. Furthermore, the authors are also presenting key factors want that shaped Popper's "radical" views, following his life in the Interwar Vienna together with his interaction with the Vienna Circle. They emphasize the fact that Popper's critique of Kantian apriorism was considered as a direct attack on logical positivism by many prominent members of the Circle. Authors reconstruct the complexity of Popper's relationship with its members as it is often a frequent point of confusion when we discuss Popper's early intellectual life and its influence. It is the second aim of this paper to recognize and identify nuances of Popper's early thinking (in particular, his rejection of synthetic apriorism) and to put them in the perspective of his critique of logical positivism. In effect, the critique of Kantian apriorism connects Popper with logical positivism and, simultaneously, several points of Kant's work are in specific way inspirational for both schools of thought. In short, this paper offers a structured survey of Kantian inspirations in Popper's philosophy in context of his relation to Vienna Circle.

[25] A constructivist interpretation of Euclid's principle of superposition
Blåsjö, V. N. E. (*Utrecht University*)

In the Elements, Euclid appeals to superposition to establish two triangle congruence theorems (I.4, I.8). His proofs are based on placing one triangle on top of the other, seemingly treating them as moveable physical objects. This is generally regarded as one of the major flaws of the Elements. Euclid's use of superposition is seen as a naive appeal to empirical or intuitive considerations that should have no place in a formal treatment of geometry. It is furthermore generally agreed that Euclid himself realised as much, which is allegedly why he avoided the use of superposition whenever he could, and only used it with regret in a few instances because he could think of no alternative. Yet there are obvious problems with this reading. How could such a sophisticated geometer make such a fundamental blunder right at the heart of his geometry?

I argue that Euclid's use of so-called superposition does not in fact involve moving one figure and placing it on another, as the traditional interpretation has it. Instead, Euclid means that the figure is being reconstructed, using ruler and compass, in its new position. When this reading is adopted, many of the standard critiques of Euclid's use of superposition become invalid. Superposition is no longer simplistic and naive, but a natural concomitant of Euclid's emphasis on constructions, which is well-attested for independent reasons, and in keeping with methodological commitments that permeate the entire Greek geometrical tradition.

My interpretation revives the largely forgotten mature view of Zeuthen. In a widely cited paper, Zeuthen argued that Euclidean constructions should be considered existence proofs — a quite restricted and modernistic view of the role of constructions in Greek geometry. However, decades later, in one of his last works, available only in Danish, Zeuthen developed a more sensitive appreciation of the role of constructions as it relates to superposition. Here he explicitly rejects his earlier view that Euclid's principle of superposition is based on motion, and instead argues, as I do, that when Euclid speaks of placing one figure on top of another he really means reconstructing it in that position by means of ruler and compass.

There is, however, an apparent problem with this view, which lead Zeuthen to regard Euclid's reasoning as ultimately circular. Namely, that important constructions occur after, and are logically dependent upon, theorems that, on my reading, appear to assume those very constructions. However, I propose a new interpretation according to which these later construction propositions are not in fact assumed in the earlier proofs. The appearance that they are is due to an overly modernistic way of thinking. My solution consists in extending the construction-based point of view even further, so that the very ontology and meaning of geometrical objects and statements in Euclid's geometry are construed in such terms. In this way I resolve the alleged flaw in the Elements in a way that construes Euclid's reasoning as coherent and sophisticated.

[27] Cause and Effect in Leibniz's Brevis demonstratio

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Leibniz's famous argument against Descartes' conservation principle (CP) proposed in *Brevis demonstratio erroris memorabili Cartesii* (1686) has attracted a lot of heat in the 17th Century and beyond. The debate is still going on as to how we should understand Leibniz's argument and what exactly it was supposed to show. The standard reading would suggest that Leibniz's argument went something like this: motive force is not identical with quantity of motion; rather, it is to be identified with *vis viva*; motive force is what is conserved; therefore, conservation should be accounted in terms of *vis viva*, and not in terms of quantity of motion.

In a lesser known draft of the same year (*Considérations sur la conservation du mouvement ou de la force*, 1686 Sept.), Christiaan Huygens responded to Leibniz's argument and found it essentially lacking. Huygens endorsed the standard reading that Leibniz argued against Descartes by ascribing equivalence of motive force and quantity of motion to him. He goes on to point out, however, that this equivalence was not part of Descartes' view. Descartes never thought that quantity of motion is conserved because it is equivalent to motive force. Rather, he "derives the law immediately from the immutability of God" (*Oeuvres complètes*. Tome XIX, p. 163). This is in fact evident in both Descartes' *Principles of Philosophy* and *The World*. So it seems that Leibniz's argument misses its target.

In this paper I propose a new reading of *Brevis demonstratio* according to which Leibniz is not referring to Descartes' formulations of CP in the *Principles* or *The World*. Rather, he is constructing it vis-à-vis Descartes' letters to Constantijn Huygens (AT I 432-48 et al.). In these letters Descartes presents an argument that the same force that can lift a weight of 100 units to a height of 2 ft. can also lift a weight of 200 units to 1 ft. He further argues that "this principle must be accepted if we consider that the effect must always be proportional to the action that is necessary to produce it" (AT I 436).

I see Leibniz's *Brevis demonstratio* as including this line of Descartes' argument. The new reading grounds Leibniz's argument in the equivalence of cause and effect, and not in the supposed equivalence of motive force and quantity of motion. Granted that Descartes doesn't accept the latter, he is evidently endorsing the former. This provides Leibniz with a further reason to deny CP – quantity of motion is not conserved because it is not identical with motive force and thus violates the equivalence of cause and effect. In addition, this reading is closer to Leibniz's own view of emerging dynamics and to the form of argument that he presents in the *Brevis demonstratio*.

[28] Cournot and Renouvier on Scientific Revolutions
Schmaus, W. (*Illinois Institute of Technology*)

A century before Kuhn's *Structure of Scientific Revolutions,* French philosophers of science had already begun thinking about scientific revolutions. Although the notion can be found as early as the eighteenth century, for many earlier writers, the notion of a revolution carried the sense of a return to or restoration of an initial starting point, as in the revolution of celestial bodies. Bertrand Saint-Sernin credits Antoine Augustin Cournot with the earliest account of scientific revolutions involving a complete and fundamental change, much as in political revolutions. However, Cournot did not develop the analogy between scientific and political revolutions any further. For that we must turn to Charles Renouvier.

In the *Considérations sur les marches des idées et des événements dans les temps modernes*, Cournot described revolutions in astronomy, mathematics, chemistry, and economics. One might argue that the idea of an astronomical revolution originated with Kant's Copernican revolution. But Saint-Sernin objects that Kant did not anticipate the possibility of further revolutions in this science, having thought that Newton had achieved the final truth in celestial mechanics. Furthermore, Cournot did not regard Copernicus's achievement as fully revolutionary. As he saw it, Copernicus and Tycho Brahe were not truly innovative, as they merely perfected the tradition of geometrical theorizing about celestial motions, without providing a mechanics of forces that could produce these motions, which was left to Kepler, Galileo, and Newton. A scientific revolution requires more than a new concept of explanatory goals, however. Copernicus was able to initiate a heliocentric revolution in astronomy while Archimedes and Nicholas of Cusa were not because Copernicus belonged to a tradition or school, continuing the work of Peurbach and Regiomantus and preparing the way for Tycho and Kepler. Finally, in a scientific revolution, a simpler hypothesis eventually wins out over a competing hypothesis that becomes increasingly complicated over time as it accommodates new empirical results.

While Cournot saw scientific revolutions as the work of remarkable individuals, Renouvier regarded them more as communal products. For Renouvier, the sciences, like society as a whole, each rest on a set of conventions or a social contract. Political history and the history of science alike reveal a pattern of alternation between periods emphasizing authority and periods allowing greater liberty. In the latter, what scientists formerly accepted on authority is questioned and challenged with observations, experiments, and reasoning. Revolutions occur when the limitations of the old social contract are realized and it is either corrected or replaced by a new one.

Of course, neither Cournot nor Renouvier had precisely Kuhn's notion of a scientific revolution. Yet they offered accounts that complement each other in ways that result in a fairly sophisticated notion of a scientific revolution. Renouvier provided the idea of a community of scientists coming to realize that a set of conventions previously regarded as authoritative is no longer solving their problems, while Cournot offered the idea of a change in explanatory goals and concepts and the search for simpler explanations.

[29] Chance, Statistics, and Experiment in Early Evolutionary Biology
Pence, C. (*Louisiana State University*)

Evolutionary biology is now taken to be a paradigmatic example of a statistical theory, which offers philosophers of science a variety of interpretive challenges concerning causation, theory structure, and inter-level relationships. But this was not always a feature of evolutionary theory. Darwin's own writings are non-statistical, and the tools and methods of statistics itself were developed along with the introduction of statistics into evolution by the "biometrical school" in the 1890s and 1900s, most prominently by W. F. R. Weldon and Karl Pearson, a story compellingly told by authors such as Hacking and Porter. Excitingly for philosophers and historians of biology, this development of statistical methods in evolution did not pass unremarked in its day. Pearson's extensive, broadly positivist writings in the philosophy of science, culminating in his *Grammar of Science,* are by now well known. The biometricians, in turn, were extensively criticized by a number of "traditional" naturalists, particularly the school which developed around William Bateson and, later, the early Mendelians, a chapter in the history of genetics also fairly well understood.

Less well appreciated has been the defense of statistical methodology of W. F. R. Weldon. Weldon's position as an accomplished experimentalist, theorist, and statistician makes him uniquely suited to offer a comprehensive picture of the status of and reasoning behind the introduction of statistics into biology at the turn of the twentieth century. In this talk, I will use a variety of archival materials to attempt to offer a picture of the philosophy of science behind Weldon's shift toward statistics. A combination of rarely discussed published materials and archival work – particularly a book manuscript and a variety of notes for that manuscript left unfinished at Weldon's untimely death in 1906 – offer a comprehensive picture of the reasons Weldon had for moving toward statistical methodology in the life sciences.

Weldon's approach, I will argue, offers a third way between the traditional dichotomy (offered by histories such as that of Provine) between a positivist, mathematized, statistical biometry and a holistic, biologically oriented Mendelian genetics. Weldon hoped to deploy statistics, I will show, as part of an integrated philosophy of science bringing together fruitful contributions from statistics, experiment, and early cellular biology. Studying this approach is valuable for (at least) two reasons. First, it offers us a novel way in which the life sciences might have developed had Weldon survived (a point recently argued eloquently by Radick). Second, and my main point here, is that Weldon's surprisingly sophisticated approach to the justification and use of statistics in the life sciences offers philosophers of science lessons that are useful today, both in the contemporary analysis of evolution and for statistical theories more generally.

[31] Gödel, Skolem, and Husserl's Crisis

Hartimo, M. (*University of Jyväskylä*)

Drawing from Husserl's own description of his method in the introduction to Formal and Transcendental Logic (1929), the paper will first argue that Husserl's view of mathematics is a species of so-called "mathematics first" approach (Shapiro, Maddy). This makes Husserl's approach context-sensitive to the extent that the development of mathematics has an impact on his philosophical views. In this paper as an example I will focus on Husserl's knowledge of the foundations of mathematics in the 1930s and the respective changes in his philosophical views. I will show that Husserl learned about Gödel's incompleteness theorems as well as the general idea of the so called Skolem–Löwenheim theorem before his death. Gödel's first incompleteness theorem states that there are statements in the language of the system so that neither they nor their negation can be derived from the axioms. Generalized Skolem–Löwenheim theorem holds that, assuming first order predicate calculus, if a set of axioms has an infinite model it has a model of any infinite cardinality. Since the models of different cardinality cannot be isomorphic with each other, the latter shows that there are no pure structures (if the domain is infinite, and first order predicate calculus is assumed) of natural or real numbers. I will then examine the consequences of this knowledge to Husserl's views. I will argue that the drastic change in Husserl's view of definiteness between Formal and Transcendental Logic (1929) and the texts written in the 1930s and now included in the Crisis, can be explained by Husserl's newly acquired knowledge of these theorems. I will argue that while Gödel's results are not particularly damaging to Husserl's views, due to Skolem Husserl eventually becomes critical of definiteness as the guiding ideal of modern mathematics. As a consequence he gives up on his "Dedekind abstraction" view of formalization and views eidetic variation as the only legitimate access to abstract concepts in his writings of the 1930s.

[32] Law and Structure in Dilthey's Philosophy of History
Hamid, N. (*UPenn*)

While Dilthey publicly distanced himself from the growing neo-Kantian movement of the late-nineteenth century, he nevertheless drew inspiration not just from Kant but also from some early proponents of the "back-to-Kant" movement. In particular, Dilthey shares a conception of philosophy's relation to empirical science present in the work of philosopher-scientists such as Helmholtz. Briefly, this paper argues, Dilthey recognizes with Helmholtz several degrees of possible philosophical (we might call metaphysical) involvement in the empirical sciences, ranging from a near-exclusion of hypotheses that reach beyond phenomena, to derivations of phenomena from a priori first principles. Like Helmholtz, Dilthey opts for an intermediate position on which philosophy begins from an experiential standpoint within empirical science in order to uncover general causal laws and structures. This interpretive approach frames my positive account of Dilthey's philosophy of historical knowledge.

As is well-known, in Chapters 14–15 of *Einleitung in die Geisteswissenschaften* (1883) Dilthey attacks the claims of German philosophy of history and Anglo-French sociology to being properly scientific. Philosophy of history, as practiced by Hegel and his followers, ought to be rejected, Dilthey argues, in part because history does not have a fixed object, or a permanent meaning that could be aimed at (GS I 92). But his rejection in *Einleitung* (and elsewhere) of philosophy of history sits uncomfortably with later pronouncements in *Aufbau* (1910) concerning the object of historical science. There, he characterizes the "first object" of historical science as "what is immutable" in the historical process and which gives history its true "sense and meaning". He identifies these immutable elements as "structural relationships" and "patterns" (GS VII 172-185). To many readers, Dilthey's position in *Aufbau* expresses a deep tension in his thought and indicates his failure to address the challenge of relativism he had recognized as a threat to his philosophy (e.g. Beiser 2011, 359-64). By contrast, this paper argues for continuity between the earlier and later Dilthey on the viability of philosophy of history. I argue that Dilthey's later, explicit endorsement of philosophical history is in fact present in his earlier views (from the 1850s to the 1880s). Even in *Einleitung*, Dilthey's restrictions on philosophy of history are qualified to target only a specific kind of project, namely, one that begins from unanalyzable, apodictic first principles as the basis for truth-apt claims about socio-historical reality. In contrast to the speculative approach, Dilthey offers a more modest conception of philosophical history as aiming at the discovery of laws and structural relations in the historical manifold, which constitute its invariant object. I show how Dilthey conceives the coordinate roles of lawful explanation alongside narration and description in the task of historical understanding. The resulting enterprise diverges not only from speculative philosophy of history, but also the positivist history Dilthey associates with authors such as H.T. Buckle, as well as traditional narrative historiography. In Dilthey's efforts to articulate a middle path, we find parallels with a broadly neo-Kantian conception of the relation between philosophy and the special sciences.

[33] Heinrich Rickert, the historical sciences, and the autonomy of philosophy
Kinzel, K. (*University of Vienna*)

Wilhelm Windelband and Heinrich Rickert engaged in a life-long controversy with Wilhelm Dilthey about the distinction between the human and the natural sciences. In this paper, I argue that this debate was driven by a deeper concern with "psychologism" and "historicism". Ultimately, at stake was the transcendental character of values. For Windelband and Rickert, the idea that values are immanent to psychological or historical processes seemed to threaten the autonomy of philosophy. This paper reads Rickert's account of the historical sciences as developed in his *Grenzen der naturwissenschaftlichen Begriffsbildung* (1902/1921) and *Probleme der Geschichtsphilosophie* (1904/1924) as a defence of the autonomy of philosophy.

In order to demarcate the historical from the natural sciences, Rickert takes up and revises Windelband's distinction between nomothetic and idiographic sciences: the natural sciences form concepts by "generalization", while the historical sciences proceed by "individualization": their concepts express an individual, unique and unrepeatable – and in this sense historical – content. Rickert characterizes the procedure of individualization as "value-relating", arguing that "historical individuality" can only be grasped as a meaningful unity if it is related to a value.

Rickert thinks of his analysis as purely formal: the individuating method is not to be justified by reference to historical reality. In particular, Rickert objects to "psychologism" – the idea that psychology is relevant to historical method. In his view, "psychologism" commits a dual mistake. It bases the epistemological account of the historical sciences on the nature of the historical object, and it misconstrues that object as psychological. On Rickert's account, the formal method shows the ultimate object of history to consist in "irreal meaning configurations" which attach to the empirical realities studied by the historian. And he insists that such irreal meaning cannot be thought of as immanent to psychological processes. By classifying psychology as a "generalizing" science, he removes psychology from the domain of meaning which the historical sciences focus on.

But although both history and philosophy deal with meanings and values, Rickert is keen to keep them separate too. He introduces a distinction between the theoretical and the practical "value-relation" and claims that only the former is relevant to historical method. The task of practical evaluation is then left to philosophy, which has to judge the course of history on the basis of an absolute system of transcendental values. A "universal history" that reveals the meaning of the historical process is only possible if grounded in the system of absolute values discovered by philosophy. Hence, Rickert makes philosophy relevant to history, but not the other way around.

Ultimately, the demarcation between "generalizing" sciences and "individuating" sciences, and the classification of psychology with the former, is meant to preserve another demarcation: that between the transcendental philosophy of values on the one hand, and psychological or historical accounts that would treat meanings and values as strictly immanent to empirical processes.

[37] The Hypergalois Programme of Felix Klein

Heller, H. (*University of Vienna*)

In the 1870s Felix Klein developed a theory that links permutation groups with symmetry groups of geometrical objects. Most prominently, he identified the Galois group *A5* of the general quintic equation with the symmetry group of the icosahedron and thus geometrically demonstrated the unsolvability of the general quintic. An important intermediate step was to interpret the projective complex line *P1(**C**)* with the 2-sphere, called the Riemann number sphere. The deep connection between algebra, geometry and complex analysis via group theory is considered an important improvement in the abstraction process of group theory (Wußing 1969), maybe the most important of the time, next to Klein's Erlangen Programme. The development of what started as the Icosahedron Problem and was soon generalised to what Gordan jokingly called the "Hypergalois Programme" was of mathematical success, insofar as the central questions – the Form Problem and the Normal Problem – could be solved for the Galois groups of general equations of degree 6, 7 and 8. It was shown in 1899 by Wiman that the Hypergalois approach does not simplfy the original problem of solving algebraic equations with degrees ≥8.

This paper will discuss the rise and decline of the Hypergalois Programme from both historical and philosophical perspectives. Historically, the programme can be seen as a fore-runner of emerging representation theory (Hawkins 1972), although it is not considered part of it (Curtis 1999), lacking both an abstract group conception and generality. I will challenge this view by comparing Klein's and his disciples' works with Issai Schur's representation theoretic generalisation from 1911 and by analysing Klein's influence in later algebraic textbooks such as Weber's *Lehrbuch der Algebra* (1896), Fricke's book of the same title (1924) and van der Waerden's *Moderne Algebra* (1930/1).

Philosophically, the role of the Hypergalois Programme within the "structuralist turn" (Corry 2012) in mathematical practice is highly ambivalent. On the one side, the simultaneous treatment of *A5* (and other groups) as groups of transformations, permutations and automorphisms facilitated a crystallisation of the abstract group structure, while on the other side Klein refrained from an axiomatic definition of groups. The advantage of the programme, I want to argue, was to mutually translate between different mathematical objects via their common property of forming a group (group as a property), rather than the modern approach of reducing these objects to simply being a group (group by definition). The sparse reception of the programme in algebra text books since the 1930s can explained on the same grounds – the tendency of contemporary mathematicians to avoid techniques outside the axiomatically given structures. Wußing's analysis of the Hypergalois programme beyond the icosahedron problem in (1969) as little useful to extend the new group concept can be read in the same line. Therefore, Klein's programme serves as an interesting historical case study that can be interpreted in favour of an inter-structuralist approach of mathematical practice in Carter's (2008) sense.

[40] Aristotle on Environmental Causation

Popa, T. M. (*Butler University*)

Aristotle's biological works, which don't have anything to say about evolution, do nonetheless have quite a bit to say about adaptation (e.g. in terms of *bios* or way of life) and about the dependence of morphological or functional features on the environment. In this paper I discuss the nature of the causal explanations Aristotle offers in this respect, and the historical background against which they are articulated. I argue that such causal connections play a central role in a variety of contexts (from the pathology of birds to the composition of hair etc.) and that they situate Aristotle in a rather impressive tradition.

A careful reading of Hippocratic texts like *Regimen I* and *II*, *Epidemics I* and *III,* and *Airs, Waters, Places* is likely to give the measure of Aristotle's indebtedness to early medical schools. Some of the Hippocratics famously thought that they could explain physical constitutions and the onset and evolution of diseases by relying, among other things, on their observation of climatic and meteorological conditions, and geographical features. They claimed that our understanding of how the elementary materials making up the world interact and are altered can also help us to gain insight into our temperament and mental capacities, a link sometimes invested with moral significance.

Given the textual evidence available to us, it is arguably impossible to establish with certainty a filiation between Aristotle's views and method and *any particular* early medical work. Still, in my view, certain types of causal inferences (regarding, e.g., health, size of the adult organism or characteristic temperament) which can be found in the Hippocratic texts very likely became part of a reservoir of ideas and methods that left their imprint on Aristotle's method of inquiry. The explanatory apparatus deployed by some of the more philosophically minded Hippocratics share important features with accounts offered by Aristotle in the biological corpus, notably in his handling of material causation. Thus, systematic connections between environmental factors, physical constitution, material dispositions and morphological or behavioral variety are dealt with e.g. in *History of Animals* VIII (IX) and in *Generation of Animals* V in ways that appear remarkably reminiscent of early Hippocratic works.

[41] Universal spirit and particles: John Evelyn's matter theory in *Elysium Britannicum*

Matei, O. L. (*Vasile Goldis University*)

The purpose of this paper is to discuss one of John Evelyn's projects for natural history, namely the *Elysium Britannicum*. Although a significant amount of attention has been given so far to John Evelyn (Harris and Hunter, 2003; Hunter, 1995; Smith, 2001; Goodchild, 1991; O'Malley and Wolschke Bulmahm, 1998; Campbell-Culver, 2006) his interest for compiling natural histories has not yet fully investigated. During the second half of the 1650s, Evelyn started to devote increasing attention to the improvement of gardening and concentrated on what would prove to be another unfinished project, *Elysium Britannicum*; which begun as a natural history of the trade of gardening, described plans for situating a garden, ways for cultivating several plants and, most interesting, methods for conducting experiments in a "Hortulan elaboratory". In order to explain how nature works and to provide the background for the manipulation of its entities in the process of experimentation, in *Elysium*, Evelyn introduces some elements of matter theory.

In the same decade of the 1650s, apart from working on the *Elysium*, Evelyn undertook the task of translating Lucretius' *De Rerum Natura*; and, in 1656, he published the First Book, accompanied by an extensive study on Lucretius. Although Evelyn's involvement with Epicureanism and atomism influences his matter theory, Book I of the *Elysium Britannicum* displays Evelyn as an eclectic philosopher, rather mixing alchemy (the *spiritus mundi* tradition perhaps via Nicaise Lefebvre whose classes of chemistry Evelyn attended in Paris) and atomism (coming from his translation of Lucretius). The purpose of this paper is twofold: on one hand I will present Evelyn's matter theory from *Elysium Britannicum* and, on the other hand, I will try to see how it served the purposes of compiling an experimental natural history. I will look into John Evelyn's *Elysium Britannicum* not as a work of a virtuoso compiling a natural history of gardening (Hunter, 1995), but as a text disclosing Evelyn's view of the universe, with the goal to manipulate entities of matter through the use of experiments in the garden.

[44] From Definitions to Axioms: The Meaning of Geometrical Principles from Euclid to Hilbert

de Risi, V. (*SPHERE/ CNRS*)

The talk addresses the epistemological question of the transformation of the meaning of axioms and postulates from the times of Euclid to the modern age.

It offers a short historical survey on the development of the system of axioms in elementary mathematics. As a matter of fact, Euclid's list of principles for geometry (5 postulates and 5 common notions) was extended and transformed already in ancient times, and during the middle ages and the renaissance several new principles were added to the corpus of elementary mathematics in order to better ground Euclid's geometry. In the early modern age, the editions of Euclid's *Elements* radically innovated on the system of principles employed in the foundations of mathematics, and some 350 new axioms were conceived and applied to Euclid's proofs in geometry. Several of these new principles were later considered and accepted in famous 19th-

Century axiomatic constructions, such as those by Pasch, Pieri and Hilbert. The first part of the talk, thus, offers some general views and examples of the important historical transformation of the Euclidean axiomatics in the modern age.

More importantly, however, with the passing of the centuries the meaning of principles changed as well. While it may be difficult to understand what Euclid himself had though on the epistemology of definitions, postulates and common notions, it is fairly evident that already in late antiquity there was a general consensus that the true principles of demonstration were the definitions. Axioms and postulates, following these broadly Aristotelian views, were rather proven starting from the definitions themselves. This epistemology of principles was generally accepted also in the early modern age, and it was in fact refined by several mathematicians and epistemologists. As a matter of fact, several editions of Euclid's *Elements* in the early modern age offered explicit proofs of many ancient and new axioms in geometry. Such an attitude may (partly) explain the enormous growth of geometrical principles in the 17th and 18th centuries, since they were not conceived as principles in the proper sense but rather as theorems (or, in any case, as provable statements), and it may also account for the rich epistemology of definitions that was articulated in those years (with the important distinctions between real and nominal definitions, for instance). However, the resistance of a few principles to be proven, despite the best efforts of several mathematicians (and especially the famous Parallel Postulate) brought some people in the 18th Century to believe that certain axioms could not be reduced to the definitions of the terms involved. This new perspective on axiomatics ended up by engendering a new epistemology of mathematical principles, which found its first fully-fledged realization in the work of Johann Lambert and was later expanded in the 19th Century (by Dedekind, Pasch and others), eventually arriving at Hilbert's famous conception of axioms in the *Grundlagen der Geometrie*. The talk ends by exploring some aspects of the latter story.

[46] G.B. Riccioli's Use of "Epicepicycles" and Spirals

Marcacci, F. (*Pontifical Lateran University*)

According to Giovanni Battista Riccioli (1598–1671), planets describe orbits in a fluid sky. In his *Almagestum Novum* (1651) he stresses the need for a single geometrical explanation for the motion of heavenly bodies and he considers a separate *Primum Mobile* as an unnecessary hypothesis. Beyond observational data, three assumptions must be taken into account:

1) The relation of planets to the Sun has to be quantified, exactly as was done in the past when astronomers calculated;

2) The solar parallax, which is important to establish the distance between the Sun and the Earth and the distance the so-called "second inequality" to understand retrogradation and other irregularities; between the Earth and the planets must be determined as exactly as possible;

3) The size of the apparent planetary magnitude and its real variation cannot be explained by appealing to the nature and substance of the skies since the variation of the planets' apparent magnitude depends on changes in their distances from the Earth.

In the light of these assumptions, Riccioli does not find the standard eccentric-epicycle theory satisfactory, and he employs a variable oscillation of the eccentric center and of the epicycle's diameter without a deferent. As a result, Riccioli chooses to use what he calls "Epicepicycles". He takes the Keplerian elliptical theory into account and proceeds to planets move along spiral orbits, which have variable sizes. The two inequalities, which astronomers had always tried to explain, are now justified: the spiral trajectory, obtained by means of the oscillation of the eccentric center, warrants the first inequality, which is the observation of the planets' velocity; the variable amplitude of the spirals, obtained by the variation of the epicycle's diameter, ensures the second inequality, which is the apparent retrograde or progressive planetary motion. Riccioli's innovations are of great interest and help to understand the nature of the astronomical debates between Copernicus and Newton.

[50] Positivist approach towards history of the a priori

Sokolova, T. (*RAS Institute of Philosophy*)

Recent critical discussions on the concept of *a priori* usually find the term old-fashioned or redundant for the XXIth century epistemology and philosophy of science and for these reasons the a priori is to be rejected. The same tendency is peculiar even for the theories, which keep the rationalistic element in epistemology and do not support completely naturalistic approach. Our claim is to show via historical development of the usage of *a priori* that this tendency does not necessarily lead to the purification of epistemology from its excess component. In our opinion, before rejecting a concept, it would be proficient for an inquisitive mind to look at how it was accepted in the first place. Our approach in this investigation is based on positivist, not essentialist grounds. Except asking *what* was meant by a priori, we will ask *how* and *in what context* the term was introduced into philosophy and became a significant part of its professional vocabulary.

Though the a priori has a long history, the pre-Kantian period of its usage is poorly investigated. Dictionaries propose different variations of its first appearance in the history of philosophy. The dates vary from XIII to XVII century for different European languages. E.g., the notorious American *The Century Dictionary and Cyclopedia* ascribes the first usage of the term to a logician Albertus de Saxonia (1320-1390), who presumably changed the old concept *demonstration propter quid* into *demonstration a priori*, but it does not provide any prove. Several articles ascribe *a priori* to Johannes Duns Scotus (1266-1308), though the term he used in his works was *per priora*. Occam (1287-1347) used the term in his *Summa Logicae* as a synonym to *demonstration propter quid*. This is the earliest usage of the term we were able to locate in logic (but not particularly in a scientific investigation). Furthermore, Descartes called a priori "a School terminology", so the term was used by Scholastics. However, Descartes himself hardly ever used the term and only in the cases to make his arguments clear to his opponents. In 1626, a minim monk and chemist Jean François in a letter to Mersenne used a priori in a context of his medical and chemical investigation. This is the earliest date we located *a priori* not in a logical investigation, but in a "scientific" one. Further, the term was adopted by Leibniz and became a necessary part of almost every philosophical investigation.

On this evidence, we claim that the usage and transformations of *a priori* through its early history is highly important to reveal the process by which a term from theology via logic and mathematics eventually becomes a universal concept for both sciences and humanities, and stays a stumbling stone for philosophy. As a result, we will show that the a priori still holds the benefits of a good philosophical and epistemological concept in respect of such features as applicability and flexibility, and on the contrary to its critics, helps to avoid redundant language constructions meant to take its place.

[54] Implicit definitions and the development of modern axiomatics

Schiemer, G. (*University of Vienna*) and Giovannini, E. (*CONICET, Argentina*)

Among the many epistemological and methodological issues triggered by the radical transformation that mathematics underwent in the nineteenth century, the problem of understanding and explaining what is exactly the *subject–matter* of a pure mathematical theory was perhaps one of the most urgent and pressing ones. More specifically, in the context of the emergence of abstract or modern axiomatics, this problem was translated into the question of what exactly an axiom system characterizes or *defines*, that is, into the inquire about the exact nature of the so-called method of *implicit definitions*. Accordingly, the notion – or better, notions – of ‘implicit definition’ is nowadays identified as one of the most fundamental methodological innovations of early modern axiomatics. However, even though this fundamental role of implicit definitions in the development of modern axiomatics is often stressed, it is fair to say that we still lack a clear historical and conceptual understanding of this notion.

The main goal of this presentation is to offer a historically sensitive account of the development of the notion of implicit definition in nineteenth and early twentieth century axiomatics. We will survey different contributions to the understanding of this notion both in some key representative cases in the history of modern axiomatics – especially in the works of Dedekind, Pasch and Hilbert – as well as in early philosophical reflections, in particular in the works of Frege, Schlick and Carnap. Firstly, we will claim that in this period it is possible to distinguish *two main approaches* or positions regarding the question of what an axiom system defines. On the one hand, the view that axiomatic systems define *higher-order entities*, such as concepts or relations – or in modern model-theoretic terms, a *class of models* or *structures*. On the other hand, the view that axioms can be regarded as definitions of the *meaning* of the primitive terms of a mathematical theory. Secondly, we will argue that while the first approach had a clear *mathematical motivation*, the second position was mainly suggested by *philosophical reasons*. More precisely, while the first approach was intimately bounded with the emergence of the structural understanding of mathematical theories, the second approach was rather prompted by the philosophical problem of explaining the nature of the primitive terms of a mathematical theory. Finally, we will suggest that it is instructive to relate these two conceptions of implicit definitions to two important traditions in nineteenth and early twentieth century axiomatics, i.e., one which takes axiom systems primarily in a semantic way, as means to define the subject-matter of a theory, and the other which gives more importance to the proof–theoretical role of axioms, that is, axiom systems as tools or theoretical devices to prove theorems.

[55] Federigo Enriques and the philosophical background to the discussion of implicit definitions

Biagioli, F. (*University of Vienna*)

Implicit definitions have been much debated in recent philosophy of science in relation to logical positivism. Not only have the logical positivists been particularly influential in establishing this notion, but they have also addressed the main philosophical problems connected with the use of such definitions: The problems of clarifying what are implicit definitions and whether there can be such definitions at all, as well as the problem of delimiting their scope in mathematics and science. However, less attention has been paid to the philosophical roots of this notion, which was actually introduced for the first time in German language by Federigo Enriques in the *Principles of Geometry* (1904) and discussed in detailed also in his *Problems of Science* (1906). This is partly because of the difficulty of situating a somewhat eclectic figure such as Enriques (who, besides being a brilliant mathematician, was one of the protagonists of the Italian development of scientific philosophy and also worked on a variety of psychological and pedagogical issues about the learning and practice of mathematics), and partly because his view has sometimes been taken to falsely imply that axiom schemata provide definitions of particular objects.

This paper will argue that: (1) Enriques clearly distinguished between the two different meanings of implicit definitions as definitions of higher-order entities or structures, on the one hand, and as definitions of the primitive terms of an axiomatic system, on the other. (2) Enriques acknowledged that implicit definitions in mathematics are actually possible only for the first kind of entities. However, (3) he also addressed the philosophical problem of finding the implicit definitions of the primitive concepts at the intersection of mathematics, physics and psychology. Contrary to what the received interpretation assumed, Enriques did recognize that axiomatic systems cannot determine the primitive concepts but only establish symbols for such concepts, which admit infinitely many interpretations. Therefore, he pointed out that the use of implicit definitions in science requires, in addition, a concrete interpretation of the theoretical terms, which have to be correlated to concrete objects. (4) Finally, Enriques addressed the problem of bridging the gap between the mathematical and the philosophical meanings of implicit definitions, by identifying structuralist patterns within mathematics that provide a clarification of the formal conceptual relations, and so also serve (indirectly) the purposes of applied mathematics. These patterns include mathematical abstraction, which in Enriques's view clarifies what is and is not necessary to assume for the development of a theory, and the reduction of postulates (and consequently of primitive concepts). Examples from Enriques's work on the history of non-Euclidean and non-Archimedean geometries will illustrate these patterns.

My suggestion is that, while 1–3 subsequently found a clear expression in logical positivism, 4 is a specific implication of Enriques's approach that deserves deeper consideration also from a contemporary perspective.

[56] Ptolemy on Nature

Feke, J. (*University of Waterloo*)

Although Claudius Ptolemy is known first and foremost for the *Almagest*, it is in his *Harmonics* that he presents the most detailed account of his metaphysics. In *Harmonics* 3.3, after completing the exposition of his music theory, he turns to the nature of harmony. To specify what harmony is, Ptolemy delineates a metaphysical framework. Although Aristotle famously proposed four causes, according to Ptolemy there are three principles: matter, movement, and form. Ptolemy defines the three principles and then identifies three causes: nature, reason, and god. In this talk, I will examine the relationship between Ptolemy's three principles and three causes, as well as their relationship to the three theoretical sciences: physics, mathematics, and theology. Furthermore, I will elucidate what role nature, in particular, plays in Ptolemy's decisions between competing astrological and cosmological theories in the *Tetrabiblos* and *Planetary Hypotheses*, respectively.

[58] The role of implicit definitions in the Peano School

Cantù, P. (*Aix-Marseille University*)

The search for mathematical primitives, and more generally the discussion on the role of definitions in mathematics, was certainly the most important objective of the Peano School. At the 1900 Paris International Conferences in Philosophy, Mathematics, and Psychology, the Italian group impressed Russell with their clarity of language and reasoning. As a matter of fact, they presented no less than six papers on the topic of definitions. Peano introduced definitions as conventionally chosen equalities that determine the primitive concepts of a theory and simplify its language. Burali-Forti discussed the difference between definitions by abstraction, definitions by postulates, and nominal definitions. Padoa developed a definability criterion to verify whether a system of primitive symbols is irreducible, and presented two further papers on the principles of geometry, and on the definition of the field of natural numbers. Vailati interpreted Brentano's tripartition of mental facts in representations, expectations and volitions as having a logical meaning, corresponding to the distinction between definitions, factual propositions and judgments of value.

The topic of implicit definitions, or rather "definitions by axioms" as they were called in the school, is central both in the discussion among the members of the Peano school itself and in their interactions with exponents of logicism and pragmatism. On the one hand it marks a distinction between Peano, who accepted nominal definitions as well as definitions by abstraction and definition by postulates, and Burali-Forti or Padoa, who did not. On the other hand it explains the reactions to Frege's definition of number and to Russell's alternative. But it is also relevant to understand the emergence of metatheoretical issues in the school (Pieri, Padoa) and the importance of piecemeal definitions.

The analysis of implicit definitions cannot be separated from the investigation of contextual considerations. The dependence of definitions on different contexts emerges in several topics that were highly debated in the Peano School: conditional definitions (relative to a class), definitions by abstraction (relative to a given relation), definability (relative to a given set of primitives), choice of definitions and postulates (relative to the application field).

The interest for implicit definitions is certainly related to the mathematical effort to axiomatize mathematics, but it also has an epistemological counterpart. Though never declaring any specific interest in philosophy, Peano shared with Vailati a pluralist, antidogmatic, and anti-foundationalist conception of definitions. Defending the plurality of views that emerged in his own school, Peano argued that definitions by abstraction, by a nominal definition, by means of a relation, and by means of an operator are "equally logic and equally rigorous": the best definition is nothing else but the definition that each teacher prefers. Vailati's and Peano's tolerance for implicit definitions does not coincide with the positions of Padoa, who remained always critical of definitions by abstraction, and Burali-Forti, who had declared that definitions by abstraction and by postulates are based on intuitions, rather than on concepts.

[61] Quine, Dewey, and the Pragmatist Tradition in American Philosophy of Science

Howard, D. (*University of Notre Dame*)

This paper argues that Quine should be seen as the principal, later-twentieth-century representative of pragmatist science, especially in the form that was developed by Dewey in the earlier twentieth century. Quine was explicit about his debt to Dewey and his understanding of his project as continuous with the Deweyan pragmatist tradition. The paper explores various themes in Quine's philosophy of science, from theory holism, translational indeterminacy, and ontological relativity to truth and realism, with special emphasis on the manner in which much of Quine's program is driven by the commitment to behavioral semantics, which is what he identified as his main point of agreement with Dewey. The paper concludes with reflections on the examples represented by Dewey and Quine of how to be an antifoundationalist without slipping into radical relativism.

[62] The axiom of choice and the road paved by Sierpiński

Therrien, V. L. (*Western University*)

Ernst Zermelo used the disastrous reception to his 1904 Well-Ordering Proof as a catalyst for serious inquiry into the requirements of a proper formal axiomatic system for set theory. Presented in 1908, Zermelo's attempt was without doubt inspired by Hilbert's 1899 *Grundlagen der Geometrie*. Of Hilbert's deductive system, Zermelo would retain: i- the use of a domain of objects with a primitive relation; ii- the explicitation of implicit assumption and transfiguration into axioms; and, iii- the emphasis on the independence and consistency of these axioms. But, given the overwhelmingly negative immediate reception of both his 1904 and 1908, how did this abstract view of sets come to be canonical by the mid-1930s? Particularly, how did the contentious “general postulate of choice” come to be the widely accepted “axiom of choice” of modern set theory and classical mathematics?

The acceptance of AC can be seen as “a turning point for mathematics (…) symptomatic of a conceptual shift in mathematics” (Kanamori 2012, 14). Whilst Western Europe remained quite hostile to this new vision of logic and mathematics, it was in Eastern Europe, at the Warsaw and Lwów Schools of Mathematics (1918–39) that the seeds of this conceptual shift briefly landed and yielded a cultivar that was to supplant and overtake the Western world. From 1908 until 1916, articles supporting AC or exploring some of its consequences were scant and scarcely concerted. The situation changed dramatically in 1916 when Wacław Sierpiński, a young professor at the Lwów University published a series of articles on AC and revived the dormant debate surrounding AC – albeit on completely different grounds. Eschewing theoretical concerns about the nature and methodology of mathematical practice, he paid little attention to the dominant question as to whether Zermelo’s existence postulate could be accepted as a mathematical construction. Instead, he recentred the discussion towards practical matters (*viz.*, its consequences, its interrelations and degree of necessity within various proofs, as well as its role in obtaining various basically trivial mathematical theorems). Starting in 1918, Sierpiński also rallied the newly formed Polish schools of mathematics around a common programme of research which was to include an in-depth exploration of AC’s role in a few select branches of mathematics. Originally adopting an objective stance *vis-à-vis* AC, his programme was to eventually completely supplant the previous philosophical and methodological debates – and Sierpiński was to become AC’s biggest champion since Zermelo. The posterity of AC as we know it would be unimaginable without Sierpiński’s efforts: “Since the labours of Mr. Sierpiński and of the Polish School, a revolution has been produced. A certain number of mathematicians have fruitfully used the axiom of choice; things are no longer in the same place” (Lebesgue 1941, 109).

[64] Lessons from Sherrington: what a theory of consciousness should tell us about pain

Brown, D. J. (*University of Queensland*) and Key B. (*University of Queensland*)

Recent debates about consciousness have raised the question whether the phenomenal quality or ‘what-its-likeness’ of a mental state is the same thing as its being conscious. Citing pain as an example, David Rosenthal (2011:435) boldly asserts “Pains do sometimes occur without being conscious, that is, without one’s being aware of them”. He claims that “being in a state with qualitative character is independent of one’s being in a conscious state, and we need different theories to explain the two.” He is not alone in considering as evidence for the distinction possible cases of dissociation, usually involving distraction, where what is supposedly a pain is either nonconscious or moves in and out of conscious focus (see, e.g., Burge (1997; 2007), Lormand (1996), Kim (1996); cf; Block (2011); Bayne & Montague (2011), Kriegel (2011); Levine (2011); Jorba (2016)). A nonconscious pain in this sense is not supposed to be a merely dispositional state; it is an occurrent pain with the intrinsic phenomenal properties of a pain that just happens not to be conscious. Block (2011) has criticised Rosenthal’s view of the independence of consciousness and sensory quality, suggesting that his higher order thought theory of consciousness lacks the explanatory power to account for what-it-is-like to be in pain. Burge (2007) notes that psychological studies have suggested that working memory, a prerequisite for higher-order cognition, is not needed for phenomenal consciousness, but in general his account of the dissociation between phenomenal character and consciousness has developed on the basis of conceptual arguments, which he admits leave open questions of empirical adequacy.

This paper examines whether there are as yet empirical foundations for the distinction between phenomenal character and consciousness in the case of pain and the relevance of empirical evidence to conceptual advances in our understanding of pain. We suspect that empirically the distinction does not hold and that maintaining it obfuscates the nature of pain and its biological functions. In arguing for this, we seek to rehabilitate the clearer distinction between nociception and pain that has been a feature of experimental work at least since Descartes. The concept of nociception is curiously absent from philosophical debates about the phenomenality of pain. This, we argue, is a mistake and one that fails to mark the differences between nociceptive mechanisms and pain but also, importantly, their intersection. Of particular historical interest in the evolution of thinking about pain in the biomedical sciences are the decerebration experiments performed by C.S Sherrington (1857-1952). These landmark studies revealed differences in the neuroanatomical origins of nonconscious and conscious behaviours associated with noxious sensory stimuli. We draw upon these lessons in recommending directions for future theorising about conscious phenomena such as pain.

[65] Duhem on Good Sense and Theory Pursuit

Shaw, J. (*The Rotman Institute of Philosophy, Western University*)

There has been an emerging consensus that Duhem's concept of 'good sense' is an extra-logical notion by which scientists adjudicate between distinct, but empirically equivalent theories. In other words, the function of good sense is limited to theory choice (Stump 2007; Ivanova 2010; Kidd 2011; Fairweather 2012; Bhakthavatsalam 2017). The purpose of my paper is twofold. The first is to show that Duhem never allocates this role to good sense. In fact, there is no problem of theory choice at all in Duhem's philosophy of science. Rather, good sense is given the role of choosing when to *abandon* a theory. In other words, Duhem's notion of good sense is best understood as applying to the question of what theories we should cease to pursue. The second goal of this paper is to elaborate on this notion of good sense and what problems it presents.

In the first part of my paper, I argue that theory choice was never an issue for Duhem because the 'problem' of theory choice is poorly formulated. The secondary literature on Duhem often equivocates between two distinct notions of theory choice: 1) choosing theories to *pursue* and 2) choosing theories to *accept*. For 1), Duhem explicitly argues that scientist can pursue *any theory they want* insofar as it satisfies certain constraints. Therefore, according to Duhem, we do not have to 'choose' between competing theories but pursue them both. For 2), I argue that underdetermination, for Duhem, is merely a tentative stage in the deduction of mathematical hypotheses from experimental laws that is dealt with through further mathematical elaboration of the theory. As such, there is no problem of theory choice for Duhem.

In the second part of my paper, I reconstruct a positive interpretation of good sense. Here, I show that good sense is something that only emerges from a *historical education* and, as such, good sense has its source in particular pedagogical practices. If these practices are not employed, then no scientist has good sense. As such, pace Stump and Kidd, good sense cannot be understood as a virtue *of scientists* but a by-product of *the virtues of good scientific education*. Additionally, good sense at early stages of theory pursuit is *pluralistic* such that many different scientists will employ good sense in opposite directions. However, according to Duhem, good sense ultimately converges on the unanimous rejection of a particular theory and thus demands our cessation of pursuing that theory. This makes good sense a feature of a *community* and not *individual scientists* against the consensus in the secondary literature.

I conclude by gesturing at a potential problem of Duhem's understanding of good sense. I argue that Duhem has accidentally deflated good sense of any methodological significance. Since the educational system that creates good sense is ultimately grounded on explicit methodological rules, good sense is merely the result of a procedure that is justified methodologically, making good sense reducible to explicit methodological criteria.

[67] Davy on Analogical Reasoning

Hricko, J. D. (*National Yang-Ming University*) and Shan, Y. (*Durham University*)

The purpose of this paper is to examine the role and nature of analogical reasoning in Humphry Davy's work on electrochemistry in order to shed some light on the nature of analogical reasoning in scientific practice. We begin with Davy's work on chemical decomposition reported in his 1806 and 1807 Bakerian Lectures. Then, we analyse and examine the nature and role of Davy's analogical reasoning. Finally, we draw some general implications for analogical reasoning in scientific practice."

In his *Elements of Chemical Philosophy* (1812), Davy regards analogy as one of the three fundamental methods that chemical philosophers use to acquire scientific knowledge and truth. In his words, "in the progression of knowledge, observation, guided by analogy, leads to experiment, and analogy, confirmed by experiment, becomes scientific truth" (1812, p. 1). Although he never explicitly defines 'analogy', Davy illustrates analogical reasoning with his work on chemical decomposition in his Bakerian Lectures from 1806 and 1807. Analogical reasoning, for Davy, is a way of reasoning by looking for similarities to guide the enquiry in order to acquire generalised facts and knowledge in science. It should be noted that, on Davy's view, analogy is not a simple process of theorising or modelling by accumulating similar observed facts to generalise a universal statement. Analogy is involved in the process of both generating and justifying the hypotheses. In addition, analogy, observation, and experiment are not three independent activities. Rather they are mutually intertwined in practice. Observation is not only guided by analogy, as Davy suggests; it also provides the foundation for analogical reasoning. On the other hand, experiment is not merely undertaken to test a hypothesis proposed by analogy; it is also designed with the help of analogical reasoning.

The picture of analogical reasoning that we get from Davy's work stands in contrast to much of the work on analogy in the philosophy of science. Philosophers have often regarded analogy as a part of inductive reasoning (Keynes 1921; Hacking 1983; Copi, Cohen, and McMahon 2014), and have sometimes construed 'analogy' as a synonym for 'model' (Achinstein 1964). But an examination of Davy's work exposes the difficulty of characterizing analogy's role in observation and experimentation as a purely inductive one, or one focused primarily on modelling. To be sure, philosophers have shed much light on the role of analogy in modelling and theory construction (e.g., Hesse 1963; Darden 1982; Shelley 2003). However, there is arguably no comprehensive philosophical analysis of analogical reasoning in scientific practice more generally, especially when it comes to the practices of observation and experimentation that Davy highlights. By examining the role that analogy plays in Davy's work, we can thereby obtain a more complete picture of the roles that analogy can play in scientific practice.

[71] Before the analytical turn: Rudolf Carnap's way into politics, 1908–1920

Damböck, C. (*University of Vienna*) and Werner, M. (*Vanderbilt University*)

In November 1920, Rudolf Carnap bid farewell to his friends in the German Youth Movement: "After entertaining thoughts to teach at free schools, in institutions for adult education and that sort of thing, after trying it out in Jena (Adult Education Center) I have been turning to pure science and have come to think of it as my real area of work." A year later, he submitted his dissertation, a contribution to modern epistemology, at the University of Jena. In 1923 Carnap was introduced to the Vienna Circle of Logical Empiricism by Hans Reichenbach (like himself a philosopher, phycisist, and member of the youth movement), that became his intellectual home for decades to come. Based on more recently available archival materials such as Carnap's diaries, circular letters, and correspondences with various friends such as Wilhelm Flitner (1889-1990) and Franz Roh (1890-1965) before, during and after World War One, I will show the ways in which Carnap's turn to pure science as well as his principled commitment to dialogue (outlined in the introduction to the *Logische Aufbau der Welt*), his support for reform of all aspects of life (gender relations, education in schools and universities, politics, and academic thinking and writing) was deeply shaped by his involvement with the German Youth Movement. Raised by a pietist mother supportive of pedagogically informed reform movements (such as the holistic Elmau-Sanatorium, Trüper's curative pedagogy) Carnap early on sought intellectual and personal inspiration outside traditional institutions. A student in Freiburg he became a co-founder of the *Freischar Freiburg*, a group of students protesting against the traditional all male student corporations. Instead, they established the sociability of the so-called *Wandervogel* (ramblers, founded by High School students in 1896) in a university environment, open to female and male students with an interest in nature, discussion of modernist culture and politics, as well as creative expression in dance, theater performances and singing. In Jena, Carnap joined the like-minded Sera Circle and the Free Student Movement, both groups trying to transform student life in more democratic, socially responsible and self-determined ways. His allegiance to the principles of the so-called Meißner Youth – Carnap was a co-organizer of the first open-air meeting of various youth movement groups on the Hoher Meißner in October 1913 – found its most radical expression in his *Politische Rundbriefe* of 1918, discussing among like-minded friends Germany's future in terms of the goals of the German Youth Movement: socialist, pacifist, democratic, community-oriented, free and just. These rather leftist ideals led Carnap after a few years of rather unsatisfying experimentation to turn to the fundamentals of thinking: logic – in order to strip thinking of its obscuring metaphysics and create a new order from scratch which would allow for true communication, and thus true transformation of the world.

[72] The development of Carnap's Aufbau as being illustrated by the correspondence and the diaries

Damböck, C. (University of Vienna)

There are various ways to reconstruct the development of Carnap's first major book, The Logical Structure of the world (Aufbau). One option would be to take the bibliography of this book and to reconstruct influences by means of careful readings of the various texts being mentioned by Carnap. Another approach would be to consider the diaries, the correspondence, and certain manuscripts to be found in the Nachlass. This paper mainly develops research in the sense of the second option, partly building on research on the Carnap papers by Carnap scholars such as André Carus, Hans-Joachim Dahms, Thomas Uebel, and Gottfried Gabriel, as well as work that was done in the past in the sense of option one. If we consider all these insights we already got from research on various influences on the Aufbau, with the inclusion of Neo-

Kantianism, the Dilthey school, Russell, Frege, Poincaré, Husserl, Mach, etc. What can we learn in addition to these insights, if we now consider the correspondence and the diaries again? We now for the first time have a complete transcription of the diaries and we have a collection of the entire correspondence – with the inclusion of the private correspondence that was closed for research until recently – as well as transcriptions of various relevant documents from the Nachlass. These additional sources allow us to carry out three different tasks on which the proposed talk will elaborate:

First, we can figure out in detail when Carnap wrote which portion of his book. It turns out, in particular, (a) that most parts of the book where written in 1925, (b) whereas the overall plan was developed already in 1920, the concrete plan for the book to be written in 1925 was only conceived in 1924, as a direct reaction to Schlick's invitation to submit the book as a habilitation thesis in Vienna, (c) after 1925 Carnap mainly rigorously shortened the book but hardly added much – with the exception of the preface that was written in 1928.

Second, we can reconstruct small portions of the original text from 1925, because there are a couple of fragments of this text that were recently found in the Nachlass.

Third, we can figure out better in which way certain intellectuals to whom Carnap was in close touch at the time when he wrote the Aufbau might have influenced him. The diaries and the correspondence show that Franz Roh, Wilhelm Flitner, Hans Freyer, Broder Christiansen, and the Bauhäusler Sigfried Giedion and László Moholy-Nagy influenced Carnap rather strongly, although only two of them – Christiansen and Freyer – are mentioned in the bibliography of the Aufbau.

[73] Apex or appendix? The roots of Carnap's "Testability and Meaning" in the Vienna Circle's Protocol-Sentence Debate as illustrated by the Carnap-Neurath correspondence

Damböck, C. (*University of Vienna*) and Friedl, J. (*University of Graz*)

As already highlighted by scholars such as Thomas Uebel and Juha Manninen, Otto Neurath played a crucial role in Rudolf Carnap's conversion from the standpoint of The Logical Structure of the World to Physicalism. Neurath tells his friend in unmistakable terms: "Carnap, Carnap, harden up." (Letter of 20 Dec. 1930, my translation). Although Carnap publicly acknowledged a first version of his position already in 1931, the concepts that Carnap and Neurath associated with the term "physicalism" remained quite different over the whole period of intellectual interchange that followed. Even after Carnap's acceptance of fallibilism, the two never reached a consensus on whether the form of the protocol sentences is freely defined – Carnap's conventionalism – or determined by history and society – Neurath. As it is revealed by the correspondence, Carnap soon felt uneasy with the debate, always considering ways to further clarify and refine his position. Did Carnap ever gain a conception of physicalism and protocols that satisfied him?

In this talk I will take Carnap's 1936/37 paper "Testability and Meaning" as my starting point. Despite the unquestioned importance of this huge article, the connection to the protocol-sentence debate is usually overlooked, probably because it is viewed to be a first stage in Carnap's intellectual development after the protocol sentence debate, rather than his final word on the topic. In fact, however, important parts of Carnap's paper have their roots in the protocol-sentence debate. Carnap's final word on Neurath's version of protocol-sentences is to be found here and it is only here that Carnap – at least in his own view – finally managed to develop a satisfactory version of physicalism doing justice to the sophisticated balance of the logical and empirical aspects of the problem.

Although I will also try to scrutinize Carnap's 1937 standpoint at a systematic level, the focus of my talk will be the reconstruction of the road to "Testability and Meaning", by means of a closer look at the correspondence between Carnap, Neurath, and Schlick. It is in the correspondence rather than in the published papers that the targets of criticism, the connections between ideas and, generally speaking, the background to Carnap's philosophical development are unequivocally available. This reconstruction allows us to shed new light on "Testability and Meaning", considering it not just an appendix or a postscript but the real apex of the protocol-sentence debate.

[74] The 1940–45 Neurath-Carnap correspondence and its philosophical significance

Damböck, C. (*University of Vienna*), Tuboly, A. T. (*Hungarian Academy of Science*) and Cat, J. (*Indiana University*)

Publications and personal/scientific correspondences have different customs and rules. One could argue that letters provide an informal and thus a more flexible platform for unpolished and knotty thoughts: philosophical thinking in the making, as it is. Otto Neurath's works and philosophical ideas were often criticized for being ambiguous, shallow, and dissolute, so what could one expect from his correspondence?

The 1940–45 letters of Neurath and Rudolf Carnap is marked by Neurath's arrival and immediate internalization in England and by Carnap's recent settlement at Harvard with Quine, Tarski, and Russell. It was also the time when Carnap started his famous *Studies in Semantics*. Those five years still might be seen as the period of acclimatization for both and one could also say that this was a period when they had to keep up their friendship in a new language (they just started to correspond in English) despite various personal and philosophical difficulties.

The exchange of letters testifies how Neurath and Carnap get acquainted with new colleagues, themes, and *philosophical* problems: one of them that might be especially relevant for the history of philosophy of science in particular, and for the history of analytic philosophy in general, is the study and context of semantics. Since Neurath did not publish much on the issue, his exchange of letters is even more promising for our understanding of what type of strategies he used against semantical investigations, and how Carnap tried to convince him about the neutrality and metaphysical innocence of semantics.

Neurath's arguments against Carnap (based on a line of heritage regarding semantics, absolutism, Plato's *Republic* and Nazism) had varied influence and fate: many of them were carried over by some of his friends (e.g. A. F. Bentley, Charles Morris), while many became notorious in the works of others (e.g. Popper and Russell). It is also true that these arguments were considered to be simple-minded and abused by some of his logical empiricist friends (Herbert Feigl, in a letter to Carnap, called A. F. Bentley an idiot who follows "Neurath's elephant behaviorism in the China shop of Semantics").

Thus Neurath's letter provide many interesting remarks and various notes that might be considered as potential arguments from a strict behaviorist point of view: and as Thomas Uebel has shown earlier that Neurath's naturalism in the protocol sentence debate had a social scientific twist (contrasting it with Quine's program), it could be shown as well that Neurath's naturalism regarding semantics, logic, induction, and probability (that is, regarding all the topics that Carnap was working on during the 1940s) also had a social scientific and political twist. And this 'twist' is connected to Neurath's activities in England, where his political and social ideas became even more practical and international.

[75] Condillac's Changing Mind
Dunham, J. W. (*Durham University*)

In this paper, I argue that Condillac developed, during his philosophical career, an increasingly sophisticated account of the development of the mind's embodied perceptual and cognitive capacities that paid close attention to developments in the life sciences.

According to the received view of Condillac, he is a disciple of Locke who developed his master's philosophy into an *austere sensationalism*. It is called *sensationalism* because it maintains that there is nothing in the mind that it did not acquire from sensations. It is *austere* because unlike Locke and Hume, who postulated the existence of innate faculties that do the work of combining and organising these sensations, it suggests that the mind is *completely empty* at birth, and that even these faculties must be acquired from sensations. Moreover, commentators on Condillac's philosophy have claimed that a further unsavoury philosophical position must follow: a mechanistic mental determinism. If the mind is wholly without structure before receiving patterns from the external world, then it is wholly determined by those patterns.

In this article I argue that, at least from the publication of the first edition of his *Traité des sensations* in 1754, this widespread interpretation of Condillac's philosophy of mind is false. It is based on the idea that Condillac understands the mind to be entirely passive. In contrast, in the first part of this article, I show that due to the inspiration of the 'vital materialist' views prominent in the circles within which he socialised during the early 1750s, Condillac argued that the mental structure of the mind is developed in response to the activity of a pragmatic needs-driven force. This force, I argue, forms for Condillac *a real a priori element in perception and cognition*.

In the second section, I show that between the first edition of the *Traité des sensations* and his later posthumously published second edition and his *Logique*, Condillac developed a theory of mental faculties that *does* deny that we are born with fully formed and perfectly functioning faculties, but, nonetheless, *does not* assert that our mind is 'empty at birth'. Rather, Condillac claims that these mental faculties have their basis in the faculties of the body and, therefore, that these mental organs must develop through growth and exercise just as our bodily organs do. In these later writings, Condillac's drops his well-known criticisms of the postulation of 'instincts', and starts to stress their importance and equivalence with what he had previously called '*impulsions*', thus echoing the work (and taking seriously the criticisms) of Hermann Samuel Reimarus, which had become popular in Paris during the 1770s thanks to the translation of his famous work on animal instinct.

I conclude this paper by suggesting that when we pay close attention to the developments in Condillac's thought, rather than treating his philosophy as an unchanging whole, a much more promising theory of mind emerges, one that is far from the 'simple sensationalism' with which his name is commonly associated.

[78] Auguste Comte and J. S. Mill on the question of physical causes: the case of Joseph Fourier's theory of heat
Esanu, A. (*University of Bucharest*)

As Larry Laudan pointed out in the 1970s, in a convincing attempt to revive Auguste Comte's positive philosophy, one of the most interesting and largely overlooked aspects of Comte's 19th century philosophy of science was his categorical rejection of causal theories in natural science, such as Laplace's interpretation of Newtonian mechanics or the expansion of Laplace's mechanical model of particles and forces to electricity, magnetism or heat. But Laudan himself is not very clear on how Comte's overall non-causal approach to physical science was modern rather than, say, naive. For example, in a famous critique from *Auguste Comte and Positivism*

(1865), J. S. Mill made the case that Comte was in a grave confusion about the very notion of "cause" and its explanatory power in physical science. Moreover, said Mill, the confusion stemmed from a too narrow and uncritical attachment to his anti-metaphysical program, which made Comte unable to distinguish between "ultimate" causes (in the Aristotelian sense, that is metaphysical natures) and "physical" causes (*i.e.* proximate phenomena that figure as invariable antecedents to other physical phenomena). Thus, according to Mill, the thing that eventually made Comte reject causal theories in natural science was his incapacity to understand physical or, in other words, phenomenal causes.

In this presentation I aim to show that Mill's critique of Comte's misunderstanding about physical causes is partially oblique, in the sense that Comte had envisioned a model of explanation in physical science that did not need to rely on physical forces at all. So, it may be true that Comte was not attentive to the conceptual distinction between metaphysical and proximate causes in science, but nevertheless he was clear on the dispensability of explanations based on forces/causes acting on particles, at least in some branches of physical science (electricity, magnetism, heat). By stressing on this point of collision between Comte and Mill, I hope to make more precise the sense in which Laudan could have meant that Auguste Comte's rejection of causal theories in physics had a "modern twist about it". Comte seems to have alluded to a mathematical analytical construction of physical theories, which treated physical phenomena in terms of their variations rather than their proximate causes. The main argument I will offer for this claim resides in a reconstruction of Comte's position from the monumental *Cours de philosophie positive* (1830-1842) on a particular topic: Joseph Fourier's theory of heat from *Theorie analytique de la chaleur* (1822), which provided Comte with a rival approach in physical science to that of Laplace. In order to accomplish this reconstruction, I will also rely on Gaston Bachelard's comprehensive but also overlooked analysis of Comte's understanding of Fourier's theory of heat, from *Étude sur l'évolution d'un problème de physique. La propagation thermique dans les solides* (1928).

[79] Kantianism and "organisation of the mind". A neglected aspect of Kant's legacy in 19th century physiology of mind

Pecere, P. (*University of Cassino and Southern Lazio*)

While 19th century neo-Kantianism in general has been the object of intensive scholarship in the last years, Marburg neo-Kantianism and its connection with the exact sciences have received by far the most attention (e.g. Nordmann, Friedman, eds., 2006; Makkreel, Luft, eds. 2010; De Warren, Staiti 2015). This historiographical tendency originally started with the wide philosophical acceptance, in 20th century philosophy, of Hermann Cohen's critique of what has been called the "naturalistic" or "physiological" account of Kant's philosophy, as it had been developed by Friedrich Lange following a perspective first sketched by Hermann von Helmholtz. Friedrich Beiser, who devotes more space than other contemporary scholars to this side of neo-Kantianism (in Beiser 2015), still basically accepts this kind of criticism concerning both Helmholtz and Lange. Resulting from this kind of premise, the physiological side of neo-Kantianism has been neglected. Moreover, the original interests of Kant himself for the physiology of mind have been considered marginal with respect to his major biological and anthropological ideas (Sturm 2009, chapter 5).

In this paper I want to focus on this side of the Kantian legacy starting from Kant's short writing on Samuel Soemmering's "Über das Organ der Seele" (1796). In this essay Kant adopts a twofold strategy concerning the prospect of a physiology of mind: first, he defends the *possibility* of a physiological study of mind, grounded on the empirical analysis of fluids in the brain as "being *continuously* organized without ever being organized": this process can be subject to chemical analysis and thus provide a physiological ground to the law of associations of ideas (AA 12: 33). Second, Kant insists on the *limits* of this physiological account with respect to "pure consciousness" as the condition of the intellectual synthesis of representations, since the latter involves "a priori grounds" and hence cannot be reduced to physiological processes (AA 12: 31-32; see Pecere 2017).

The influence of Kant's essay on Soemmering on 19th century German physiology has not been the object of sufficient investigation yet, but there is evidence that German physiologists and philosophers were aware of it and adopted a similar perspective on the background of the materialism controversy. Helmholtz's idea of the "organization of the mind [*Organisation des Geistes*]", in his speech "Über das Sehen des Menschen" (1855) and in the "Handbuch der physiologischen Optik" (1867), notably involves a reappraisal of this aspect of Kantianism for the establishment of an anti-reductionist physiology of mind: while projecting the detection of physiological correlates of mental processes, Helmholtz claims that some intellectual laws and processes cannot be reduced to natural processes. Similar ideas were defended by Mathias Schleiden ("organisation of reason", 1863) and by Friedrich Lange in his "Geschichte des Materialismus und Kritik seiner Bedeutung in der Gegenwart" (1866, 1875[2]: "organisation of thought"). As it has been reminded in recent scholarship, this tradition – via Helmholtz's school – eventually led to Freud's conception of the I as an "organisation of mental processes in a person" in "Das Ich und das Es" (Arminjon 2010; Longuenesse 2017).

[90] Beyond the Metaphysical Foundations of Natural Science: Kant's Empirical Physics and the General Remark to the Dynamics

McNulty, B. (*University of Minnesota*)

The General Remark to the Dynamics (hereafter, "Remark"), appended to the second chapter of Kant's *Metaphysische Anfangsgründe der Naturwissenschaft* (hereafter, "MAN"), is a perplexing tract. Therein Kant includes a four-part consideration of a disparate collection of physical topics—including density, cohesion, elasticity, and chemical dissolution, among others—that is bookended by discussions of the comparative advantage of his force-based, dynamical mode of explanation versus the corpuscularian, mechanical approach. These topics—especially the discussion of sundry empirical phenomena—appear to be largely disconnected from the aprioristic account of rational physics at the heart of MAN, and hence the motivation behind and the upshot of the Remark are both somewhat obscure.

Michael Friedman (1992, 2013) and Oliver Thorndike (2018), in different ways, highlight the importance of the Remark. Friedman emphasizes that the Remark concerns the specific variety of matter—those properties that vary among different types of matter—and provides a (non-explanatory) model for experimental physics, while Thorndike contends that the physical phenomena considered in the Remark constitute the foundation of *empirical* physics, opposed to the rational physics presented in the body of MAN. However, both interpretations raise questions regarding the details of the assumed relation between the *a priori* metaphysical foundations of natural science and the topics discussed in the Remark.

In this paper, I argue that Kant, indeed, aimed to ground empirical physics in the Remark, and I flesh out the details of the manner in which the rational physics of MAN provides the framework for its empirical counterpart. In order to substantiate this interpretation and to support Kant's claim that the moments discussed in the Remark are truly those to which the specific variety of matter (and empirical physics, generally) "are collectively reducible a priori" (AA 4:525), I detail his account of empirical physics by examining pre-Critical works on natural philosophy, notes from his lectures on physics, and his *Reflexionen*. My considerations reveal that the empirical physical phenomena of Remark are *not* reducible to the fundamental attractive and repulsive forces presented in the body of the Dynamics of MAN, even in theory (contra Thorndike). Rather, the specific variety of matter requires the assumption of additional, original forces of matter. Empirical physics is hence grounded on the metaphysical foundations of natural science, for Kant, not in the sense of being reducible to the latter's fundamental forces. Nevertheless, I maintain that the moments appearing in the Remark are meant as an explanatory foundation for empirical physics (contra Friedman). My interpretation not only elucidates an arcane passage of MAN, but moreover, by detailing the boundaries between the metaphysical foundations of natural science and empirical physics, it clarifies the aims of the work as a whole.

[91] Locke, Newton, and Demonstration in Natural Philosophy

Connolly, P. J. (*Lehigh University*)

Locke was an early and vocal supporter of Newtonian natural philosophy. This is often seen as problematic insofar as Locke's epistemic commitments are alleged to be at odds with Newtonian methodology. Specifically, Locke asserts that natural philosophy could not be made a science. Within the realm of natural philosophy we are confined to experiment and conjecture. But Newton's *Principia* contains demonstrations that purport to offer certainty. These considerations have led a number of authors (e.g. Anstey, Domski, Winkler, Schuurman) to argue that Locke was either inconsistent on this front, radically changed his views, or made some form of special exception for Newton.

This paper provides an alternative approach to Locke's understanding of Newtonian natural philosophy. On this alternative approach, Locke's embrace of the *Principia* is fully consistent with the epistemic framework outlined in the *Essay*. And Locke did believe that demonstration could play an important, albeit circumscribed, role in natural philosophy.

The paper first argues that there is room in Locke's thought for a distinction between "demonstrations" and "science." Thus, while Locke does indicate that he believes natural philosophy cannot be made a science, this does not entail that he rejects demonstration in natural philosophy. Early modern conceptions of *scientia* involved more than demonstration, they involved demonstrations *from first principles*. Recognizing this allows us to see that Locke's rejection of science in natural philosophy was motivated not by concerns about demonstration *per se*, but rather by his commitment to our ignorance of the real essences of bodies. While we might achieve demonstrations in natural philosophy, they would not count as scientific insofar as they could never take the real essences of bodies as their starting point.

The paper then argues that Locke could have understood the *Principia* as an example of mixed mathematics. Episodes in Locke's biography show that he had a connection to and explicitly approved of certain mixed mathematical programs in optics and astronomy. In this type of endeavor *certain empirically tractable features* of bodies could be modelled mathematically. Importantly, these models would be incomplete in the sense that they would offer only partial characterizations of the features and powers that bodies possess. Demonstrations could be provided within these models. These demonstrations would not provide certain truths about the nature of bodies or their essences. But they would show that *insofar* as bodies had certain features they would behave in certain ways. Thus, this approach would have been compatible with both Locke's broadly empiricist methodology and his general epistemic humility regarding the true nature of material substances.

Before concluding the paper suggests that this approach to understanding bodies was shared by Locke and Newton. Newton's characterization of bodies in the *Principia* and *De Gravitatione* as well as his skepticism about the possibility of knowing the essences of material substances are briefly reviewed.

[93] Henri Bergson's Biological Theory of Knowledge

Herring, E. (*University of Leeds*)

During the first decades of the 20th century, French philosopher Henri Bergson was an international celebrity and his theories of 'duration' and creative evolution were discussed in most intellectual circles. He believed that science and philosophy were two different but complementary forms of knowledge. According to Bergson, scientific knowledge was somewhat removed from reality especially when applied to questions about life and mind. Science allowed for human control over nature rather than a profound understanding of reality. On the other hand, Bergsonian metaphysics, if conducted correctly could provide knowledge of reality in itself without requiring practical results.

In his 1907 bestseller *Creative Evolution*, Bergson proposed an evolutionary explanation for the limitations of science i.e. its tendency to replace and confuse fluid and mobile realities with rigid concepts, models and symbols. Paradoxically, intelligence had evolved in such a way that it was bad at understanding evolution or, in Bergson's words, "Intelligence is characterised by a natural incomprehension of life". Bergson also used his vision of evolution, in particular the common descent of intelligence and instinct, to argue that unmediated (i.e. non-symbolic and non-conceptual) knowledge of reality was possible.

After briefly explaining Bergson's theory of duration and his idea that, when it came to the notions of time, change and evolution, mathematics and physics were far removed from reality, I will introduce the epistemological solution that Bergson eventually proposed. He distinguished between two modes of access to reality, analysis and intuition, and claimed that the latter would be better suited for gaining access to temporal realities like consciousness and life. Bergson grounded this theory of knowledge in the theory of life he developed in *Creative Evolution*. I will conclude with reflections on why Bergson matters for the history and philosophy of science, in particular the history and philosophy of biology.

By uncovering Bergson's original epistemological position which uses the evolutionary history of human consciousness to understand our different modes of access to reality, this paper aims to shed new light on contemporary debates about scientific realism.

[94] Demonology Naturalized: The Baconian Roots of Joseph Glanvill's Inquiry into Witchcraft

Schwartz, D. (*American University in Bulgaria*)

This paper builds on Stuart Clark's Baconian analysis of Joseph Glanvill's work on witchcraft. Like Clark, I am not primarily interested in tracing the influence of particular bits of text. What I aim to explore is the degree to which Glanvill's inquiry into witchcraft is compatible with Francis Bacon's methodology as properly understood. This project will also leave us in a good position to address other figures in the early Royal Society, including the more skeptical Robert Hooke.

I will show that Glanvill's two principal writings on witchcraft—*A blow at modern Sadducism* and *Saducismus triumphatus*—follow the Baconian methodology in some important ways: (1) As Clark has well observed, Glanvill agrees with Bacon's insight that we can learn about nature's laws by studying the departures from those laws (assuming we set the facts down cautiously and without superstition in a history of pretergenerations); (2) The argumentative strategy of *A blow at modern Sadducism* is to clear away the distortions and prejudices introduced by a wrongly materialistic worldview; this reflects agreement with Bacon's view that if the idols are demolished then the facts of natural history will almost speak for themselves; (3) The particular mechanisms of demonological causation proposed by Glanvill are reminiscent of Bacon's own hypotheses in the *Sylva sylvarum* (one of the texts where Bacon acknowledges the possibility of witches communing with evil spirits); (4) Neither Glanvill nor Bacon hesitate to use scripture as a source of natural historical data; and (5) Glanvill, like Bacon, attempts a *via media* with regard to superstition. Bacon's distaste for superstition has sometimes been exaggerated. Bacon advises natural philosophers to avoid superstition, but he says precious little about how to identify superstition *as* superstition. He apparently accepts the effectiveness of some trials by ordeal, though he leaves it open whether the mechanism is divine or natural. He also counsels against being superstitious *about* avoiding superstition, which suggests that he could easily have praised Glanvill for erring on the side of inclusiveness.

Despite all of the above, I will explain why it is unlikely that Bacon himself would have approved of Glanvill's demonological pursuits. In a passage in the *Parasceve,* Bacon states that only natural wonders should initially be admitted into the history of pretergenerations, whereas superstitious miracles (which I take to be wonders of supernatural origin) should be investigated only after we have made significant progress in natural philosophy and then relegated to a treatise of their own. After showing that Bacon's own natural histories bear this approach out, I suggest that it is in keeping with his more general practice of giving priority, in the order of inquiry, to the most certain and most useful knowledge, thereby laying the groundwork for science to correct and enrich itself.

[95] Kant's Pre-Critical Monadology and Leibniz: Ubeity, Monadic Activity, and Idealist Unity

Slowik, E. (*Winona State University*)

An interesting upshot of the prevailing "realist" interpretation of Leibniz' system (which repudiates a purely idealist or immaterialist interpretation of his late metaphysics and accepts the existence of an external world of some sort, e.g., Garber 2009) is the close proximity it places his monadological metaphysics vis-à-vis Kant's own pre-critical period monadology, a point that has been raised by various commentators with respect to the non-spatiality of monads in particular (e.g., Rutherford 2004, 231-233). Yet, while Kant's pre-critical monadology does indeed have many features in common with Leibniz' approach, especially concerning the monadic spatiality issue, several key components in Leibniz' and Kant's respective monadic systems have been neglected in prior investigations of this similarity: namely, (i) the concept of ubeity, a scholastic distinction that Leibniz discusses in the *New Essays* to characterize the different ways that a being can be related to space; (ii) the crucial role that monadic activity or operation assumes in their respective systems; and (iii), God's function as the grounds or foundation of the monads and the material world. As will be revealed in this presentation, a focus on issues (i), (ii), and (iii) discloses new evidence, and supports new arguments, concerning the close connection between Leibniz and Kant's respective monadologies.

In the *Physical Monadology* (1756), Kant forthrightly adopts Leibniz' preferred form of ubeity, which is repletive (i.e., where the being is not situated in space although its activity or operations are in space)—and, quite importantly, Kant supports his conclusion by offering an analogy between a monad's sphere of activity and God's presence "to all created things by the act of preservation" (*Physical Monadology* 1:481), a description that exactly matches Leibniz' account of repletive ubeity, whereby God "operates immediately on all created things, continually producing them" (*New Essays* II.xxiii.21). That is, just as Leibniz characterizes God's repletive ubeity through the act of preserving the world, an hypothesis that rejects the presence of God's substance or being in space, so Kant posits a corresponding monadic repletive ubeity doctrine that similarly denies the presence in space of a monad's substance (or internal determinations). As will be demonstrated, Leibniz makes a similar analogy between a monad's operation/activity and the “extension of power” doctrine, the latter comprising a further Scholastic conception that includes God’s continual preservation of the world (and hence includes repletive ubeity), but he hesitates since he apparently associates the extension of power hypothesis for lesser beings with a physical influx, which he denies (preferring his pre-established harmony thesis instead). Kant, as is well-known, openly supports a type of physical influx among his monads, and thus his *Physical Monadology* may represent one the most elaborate attempts to defend the non-spatially situated component of Leibniz’ original monadological hypothesis alongside a full-fledged notion of inter-monadic activity.

[96] Husserlian Phenomenology and Scientific Realism

Kattumana, T. J. (*KU Leuven*)

Edmund Husserl’s treatment of modern science in his *Crisis of the European Sciences and Transcendental Phenomenology* (1954) has influenced both phenomenological and continental reflections on modern scientific practice and its claims. Contemporary scholarship extends this influence to analytic philosophy as well, especially the work of Wilfrid Sellers. In this text Husserl claims that modern science, despite its remarkable achievements, experienced a crisis at the beginning of the 20th century. Husserl traces this state of affairs back to Galileo’s innovations that, while considerably contributing to the success of modern mathematical physics, also lay the grounds for what would become its predicament in the 20th century. In particular, Husserl criticizes Galileo for forgetting certain empirical practices, such as surveying/measuring and their role in the emergence of ideal mathematics, the substitution of a mathematical world of idealities for the empirical world, and the resultant reductionist conception of the world. Husserl proceeds to show how the phenomenological approach is well equipped to deal with these issues by employing a phenomenological analysis of the sciences that grounds its practice in the life-world. In doing so, Husserl aims to delimit the scope of, and locates the life-world as the ground for, the validity of all scientific claims.

However, the implication of Husserl’s positing the life-world as the ground for the validity of all scientific claims is not entirely clear. Although Husserl clearly recognizes the successes of modern science and argues that grounding the natural sciences in the life-world does not call into question its claims to ‘objectivity,’ his analysis is simultaneously explicit in its aim to delimit the scope of these very claims. In this regard, Husserl’s analysis pertaining to the status of scientific theories has been a matter of scholarly dispute. Among the many interpretations available on this issue, scholars like Patrick Heelan (1987) and Harald Witlsche (2012) have claimed that Husserl’s critique of science is anti-realist with respect to scientific theories. The present paper argues against an anti-realist portrayal of Husserl’s philosophy of science and claims that, by grounding the validity of scientific claims in the life-world, Husserl attempts to develop the grounds for a robust form of scientific realism regarding scientific theories.

This paper is divided into four sections. The first section critically examines both Husserl’s analysis of Galilean science and his claims regarding the crisis confronting modern science in the 20th century. Section two analyzes Husserl’s conception of the life-world and the implications of grounding the natural sciences in this domain. The third section considers anti-realist and realist readings of Husserl’s claims regarding scientific theories. Lastly, building on sections one and two, the fourth section criticizes an anti-realist interpretation of Husserl’s claims and argues in favor of a realist interpretation. In the process, the paper aims to arrive at a better understanding of the phenomenological approach to modern scientific practice and, in particular, Husserlian phenomenology’s construal of scientific theories.

[97] Physics is a Kind of Metaphysics: On Émile Meyerson's Influence on Einstein's Rationalistic Realism

Giovanelli, M. (*University of Tübingen*)

Gerald Holton has famously described Einstein's career as a philosophical 'pilgrimage.' Starting on 'the historic ground' of Machian positivism and phenomenalism, following the completion of general relativity in late 1915, Einstein's philosophy endured (a) speculative turn: physical theorizing appears as ultimately a 'pure mathematical construction' guided by the faith in the simplicity of nature (b) a realistic turn: science is 'nothing more than a refinement' of the everyday belief in the existence of mind-independent physical reality. However, Einstein's mathematical constructivism which supports his unified field theory program appears to be at first sight hardly compatible with the common sense realism with which he countered quantum theory. Thus, the literature on Einstein's philosophy of science has often struggled in finding the thread between ostensibly conflicting philosophical pronouncements. This paper, relying on unpublished material, claims that Einstein's dialog with Émile Meyerson from the mid-1920s till the early 1930s might be a neglected source to solve this riddle. Einstein met Meyerson for the first time in Paris in 1922. Their dialogue was resumed a few years later after Einstein read Meyerson's 1925 book La déduction relativiste. After some initial resistance, Einstein came to appreciate Meyerson's emphasis on the 'Hegelian' treats of modern physics. After the failed attempt to have the book translating into German, Einstein agreed to write a glowing review of Meyerson's book on relativity. Meyerson offered to Einstein that combination of constructivist rationalism and realism he was searching for. In popular writings of that period, including articles published in the New York Times and the London Times, Einstein was not afraid to publicly agree even with Meyerson's bold comparison with Hegel. As he wrote jokingly to the "too positivistic" inclined Schlick: "you will be surprised at the 'metaphysician' Einstein" (Einstein to Schlick, 28-11-1930). The 'physics as metaphysics' parlance was meant as a tongue-in-cheek reminder of the paradoxical nature of the relationship between physics and reality (a) the belief in the independent existence external world and (b) the conviction that the latter can be grasped only by speculative means. Einstein could present his search for unified field theory as a metaphysical-realistic program opposed to the positivistic-operationalist spirit of quantum mechanics. Einstein's infatuation for Meyerson's work in his late Berlin years is revealing of the extent of his 'philosophical pilgrimage.' If Schlick had been Einstein's main philosophical interlocutor at the turn of the 1920s, Meyerson seems to have taken his place at the turn of 1930s. The reasons why Einstein was fascinated by Meyerson's thought are still clearly recognizable in Einstein's often-quoted Herbert Spencer lectures, in which speculative-rationalistic approach to physics, is combined with a realistic train of thought. "Physics is a kind of metaphysics", Einstein famously wrote to Schrödinger, "Physics describes reality. But we don't know what reality is unless we describe it with physics!" (Einstein to Schrödinger, 19-06-1935).

[100] Three kinds of narratives in the history of early modern philosophy and the case for digital fictions.

Sangiacomo, A. (*University of Groningen*)

In this paper I present a new approach to the history of early modern natural philosophy, which I call 'digital fiction'. Digital fiction is a method that allows historians to (1) integrate digital techniques and tools to study the socio-semantic networks of early modern natural philosophy; (2) implement models to investigate the evolution of the early modern debate; and (3) develop a fine-grained interpretation of both the historical materials and the patterns uncovered by the digital analysis. The paper is divided into two parts.

In the first part, I offer a general methodological defence of digital fiction. I argue that historians of early modern natural philosophy usually rely on two (non-exclusive) standard methods. On the one hand, they reconstruct historical debates from the point of view of today's philosophical concepts. On the other hand, they reconstruct the historical context of ideas and concepts by investigating the way in which they were rooted in past debates. The first approach risks of misrepresenting the historical meaning of past concepts and ideas by interpreting them with today's categories. The second approach runs into the problem that a careful and really inclusive investigation of the historical context requires to deal with massive corpora that can be hardly handled by individual researchers or small teams. Digital fiction remedies to both these problems. First, digital fiction requires to reconstruct the socio-semantic network associated of the concepts or problems that the historian wants to investigate. Networks analysis allows the historian to take into account the whole historical corpus and study how its social dimensions (i.e. the relationship among the historical actors involved) and its semantic dimension (i.e. the conceptual vocabulary used by these authors) co-evolved across time. Second, digital fiction aims to explain the evolution of early modern networks by implementing different models to represent counterfactual scenarios and study how the network would have evolved if certain conditions were met. Models allow the historian to play with different intuitions and conceptual constructions without assuming that they were actually endorsed by the historical actors. Eventually, the results obtained by modelling the evolution of the network are compared with relevant textual excerpts derived from the corpus. In this way, the historian can use the historical materials to verify the hypothesis generated by the models and use the models as heuristic devices to investigate new aspects of the historical materials.

In the second part of the paper, I offer an illustration of the digital fiction approach based on the in-progress results of a project that I am currently running. The project aims to implement digital tools based on word2vec technology to study the evolution of the conceptual vocabulary associated with natural philosophy in seventeenth and eighteenth-century correspondences. The project illustrates both the potentials and the limitations of digital fictions.

[101] Debunking the Myth: Einstein on Implicit Definitions

Giovanelli, M. (*University of Tübingen*)

In the opening paragraphs of his 1921 lecture *Geometrie und Erfahrung*, Einstein praised Schlick's "book on epistemology" for having "very aptly" defined the axiom of geometry "as 'implicit definitions'". For the emerging Logical Empiricism, the lecture became the manifesto for the distinction between the modern 'axiomatic' conception of geometry and 'practical geometry.' For the mathematicians, the 'distance between two points' is as an attribute of the two points which obeys the axioms of, say, Euclidean geometry. In this sense, the question whether the propositions of Euclidean geometry are 'true or false' is meaningless. The latter, however, is precisely the question in which the physicists are interested. To answer it, it is necessary, to associate the concepts of geometry with real objects, e.g., the concept of 'distance' with the behavior of rods. The propositions of Euclidean geometry can be then said to be 'true' when they correspond to the observed behavior of rods. However, Einstein continued, the rods at our disposal are not rigid. To isolate them from distorting influences, one should possess a detailed knowledge of the dynamical laws that describe their material constitution. Thus, Einstein famously claimed, *sub* specie aeterni, Poincaré was right when he claimed that only geometry together with physics can be said to be true or false. Nevertheless, since we do not have a physical theory powerful enough, *sub* specie *temporis*, we should rely on practical geometry. Scholars have often struggled to make sense of Einstein's 'delicate dance' (Friedman, 2009) between Schlick's language of implicit definitions, Helmholtz's geometrical empiricism, and Poincaré's conventionalism. *Geometrie und Erfahrung* is often mistakenly read as the reflection of an amateur philosopher on the foundation of geometry. On the contrary, this paper will show that it should be regarded as the non-technical response of a professional physicist to technical objections raised by other relativists. To understand Einstein's arguments, it is necessary to unveil the complex dialogical background (Beller, 1999) hidden behind the apparent simplicity of the lecture's prose. The text does not develop a single line of reasoning, but appears as the superposition of several arguments addressed to different interlocutors. Einstein, using Schlick's parlance of implicit definitions, defended, contra Weyl (Ryckman, 2005), the Helmholtzian traits of general relativity, a theory that makes testable predictions about the behavior of rods and clocks. At the same time, contra Pauli (Hendry, 1984), Einstein referred to Poincaré to leave open the possibility of a unified field theory in which rods and clocks seem to become unusable as empirical indicators. In the ensuing years, Einstein, in pursuing his unification program, resorted to progressively more abstract mathematical structures that could not be directly defined in terms of the behavior of physical probes, and whose only justification was the success of the theory as a whole. Far from supporting the logical empiricists' opposition between axiomatic and practical geometry, Einstein, in correspondence with Reichenbach in the mid-1920s, ended up declaring the very notion of geometry as meaningless.

[102] Newton's Rule 3 is Less Complicated than You Think: It's A Rebuke of Huygens and a Defense of Simple Induction

Biener, Z. (*University of Cincinnati*)

Rule 3 is the most discussed of Newton's *Regulae Philosophandi*. It first appeared in the *Principia*'s second edition (1713), where it replaced the first edition's (1687) Hypothesis 3, an alchemically-tinged claim about the transmutation of all bodies into one another. Yet Rule 3 does not mention transmutation. Rather, it focuses on the invariable, universal qualities of matter. The switch from transmutation to invariability has caused historians some consternation.

I. B. Cohen suggested that Newton abandoned Hypothesis 1 because it was too vulnerable to criticism by supporters of alternate matter theories. Ernan Mcmullin suggested that Newton came to realize that Hypothesis 3 conflicted with atomism, since it allowed even mechanical qualities like impenetrability to be transmuted. J. E. McGuire defended the compatibility of Hypothesis 3 with atomism, but held that Newton didn't want to defend his atomism publicly. McGuire also suggested that in Newton abandoned Hypothesis 3 because he adopted Locke's primary/secondary distinction. The latter claim has been especially influential.

But the origins of Rule 3 betray a simpler story. In this talk, I first present a play-by-play reconstruction of the immediate events that lead to Newton's formulation of Rule 3 in the winter and spring of 1690. I argue that the Rule's genesis shows that Newton's direct concern was not tempering transmutation or promoting a Lockean epistemology, but that he was responding to a few key passages in Huygens's *Discours de la cause de la Pensanteur* (1690). Placing Rule 3 in this context explains some of its most curious features, such as the seemingly out-of-context discussion of the *experimentum crucis* and Newton's odd claim (after stressing how well-founded the impenetrability of bodies is) that "the argument from phenomena will be even stronger for universal gravity than for the impenetrability of bodies."

Rule 3's origins in Huygens's *Discours* also shed light on Newton's concept of *universality*. This concept has also been the subject of some debate, since Newton went out of his way (disingenuously, according to some) to assert that gravity's universality did not entail that gravity was a primary or essential property of matter. Examining the Rule's origin, as well as offering a more thorough study of Newton's use of adjectival and adverbial forms of *universus* in the *Principia*, shows that "universality" was a much more deflationary concept. It's proper home was within discussion of simple induction from instances, and it was meant to indicate nothing more than the applicability of the "universal" predicate to all members of a certain class, *even if that class was highly restricted*. I show that Newton's contemporaries (like Pemberton) read the rule this way.

Taken together, these considerations show that Newton didn't approach the *Principia* with a coherent, worked-out philosophical position already in mind. Rather, he articulated that position after the *Principia* was initially published, in response to a series of contingent events.

[107] What Use Can the Relativized *a priori* Be to Feminist Philosophy of Science?

Crewe, B. (*University of British Columbia*)

Feminist philosophy of science has long critiqued the value-neutral ideal of scientific practice—in addition to being practically unattainable and implausible in retrospect, it is argued that this ideal also inhibits the detection of oppressive values in science. These feminist critiques have complicated and enriched traditional empiricist accounts of the workings of science, but have also raised significant conceptual challenges. Here, I suggest that many twentieth-century Neo-Kantians are similarly critical of naïve empiricist accounts of the relation between scientific theory and observation. However, these philosophers proceed by relativizing Kantian *a priori* principles in their theories of scientific knowledge, and by using this framework to account for the interaction of science and values. For Carnap, for example, this interaction occurs with reference to the selection of linguistic frameworks, which he understands to be a voluntary and pragmatic endeavor. For Kuhn, on the other hand, paradigms play the role of Carnapian linguistic frameworks, and their coming to be is a historically contingent matter.

This paper uses Michael Friedman's account of the Kantian relativized *a priori* in the work of Kuhn and Carnap to reframe one of the central problems of contemporary feminist philosophy of science, namely; the possibility of objective knowledge given the value-ladenness of science. In pointing out that science is not and has never been value-free, feminist philosophers of science are often (rightly or wrongly) left with the task of articulating epistemic norms by which both scientific knowledge *and* feminist values might be adjudicated, in order to stave off the threat of moral relativism and merely descriptive science. I argue that a Kantian transcendental approach to this task will help shift debates in feminist philosophy of science from a seemingly intractable problem—how of some values are justified in science over others—to the necessary preconditions of such a debate. These preconditions involve accounting for how it is that epistemic agents are politically embedded, and a consideration of the limits and possibilities of developing critical accounts of ideology from within ideology. I argue that the problem of objectivity in feminist philosophy of science should be addressed in terms of the conditions on the possibility of knowledge given the effects of power and ideology. This focus is a necessary prior step to resolving concerns common to feminist empiricists and standpoint theorists, surrounding theory-choice, normativity, and the justification of values. Specifically, a transcendental methodology carves a middle path through accounts that assume feminist theory to justify feminist values in science (wherein feminist theory is taken to be external to and justified independently of science as a discipline and practice), and accounts that naturalize values and take the goals of feminism and social justice to reduce to the epistemic and methodological goals of science. Ultimately, my aim here is not a definitive resolution of the aforementioned concerns but rather, in the Kantian spirit, the articulation what must occur before an attempted resolution.

[112] Dipping needles and rotating poles: What a mistaken solution tells us about legitimate and illegitimate uses of mathematics in natural philosophy

Georgescu, L. (*University of Groningen*)

The paper tells the story of a proposed solution (a mistaken solution from today's vantage point) to an empirical problem. It is concerned with how prior natural philosophical commitments about mathematics and measurement informed the way in which it was assessed. In *The Sea-Mans Kalendar* (1636 [1638?]), Henry Bond (correctly) predicted that magnetic declination would be 0° in 1657, and would then increase westerly for (at least) thirty years. The predictions were not quite exact, but were sufficiently accurate to place Bond's work on the Royal Society's agenda. Based on these predictions, Bond went on to claim that, by using his model of magnetism, he could offer a technique for determining longitude, presented in *The Longitude Found* (1676). In this almost-forgotten treatise (Howarth (2002) is a notable exception), Bond combines natural philosophical considerations, empirical measurements, trigonometry, and know-how about navigational tables, in order to solve one of the holy grails of seventeenth-century science: the problem of longitude. The solution offered was, for Bond, closely connected to his proposed model for magnetic inclination (one of very few proposed in the seventeenth century, alongside Edward Wright's (1610)). Bond's solution depended on (1) complex trigonometrical constructions and manipulations, applied to (2) the dynamics of dipoles precessing around the Earth's axis over hundreds of years. Despite criticism of Bond's technique for finding longitude, the tilted dipole model was soon borrowed and adapted as a possible solution to both magnetism and the longitude by many natural philosophers including Hooke (1674), Halley (1683), and Harrison (1694).

Bond's solution is indeed erroneous, but that could not have been known at the time. Yet the treatise was received with much skepticism both inside and outside the Royal Society. Within the Royal Society, the criticism came in 1674 from the Commission led by Pell and Hooke, and was centred on dissatisfaction with the dipping instruments Bond used and the insufficient number of observations on which his solution was based. On the other hand, Peter Blackborrow in *The Longitude Not Found* (1678, 1680) and Thomas Hobbes in *Decameron Physiologicum* (1678) offer explicit criticisms of Bond's use of mathematics. Blackborrow's criticism is based on an account of the appropriate way to model the relevant physical phenomena such that spherical trigonometry can be applied, while Hobbes criticises Bond's use of geometry and his uncritical reduction of physical motions to spherical trigonometry. My paper will focus on this latter set of criticisms.

Bond's treatise, and the responses it received, offers a springboard to show precisely how slight mutations related to legitimate ways of using mathematics to model natural phenomena lead to different accounts of the limitations of a proposed solution, which, at the time, had (a) considerable empirical support and (b) showed (some) predictive capacity.

[114] What We Talk About When We Talk About THIS Being Blue: C. I. Lewis and R. W. Sellars on the Object of Perception

Neuber, M. (*University of Tübingen*)

There is currently some reawakened interest in the relationship between American pragmatism, on the one hand, and American critical realism, on the other (see Hatfield 2015 and Klein 2015). Both schools of thought shared many aspects, but there were also significant differences. One of these differences pertains to the object of perception or, more precisely, to the question of what we talk about when we talk about *this*, for example, being blue.

It was C. I. Lewis who developed the most articulated pragmatist account of perception. In his view, when we perceive something as being blue, we perceive a certain constellation of sense impressions. However, according to Lewis, we do not perceive these sense impressions directly. Rather, the process of perceiving involves an element of interpretation (or construction). Thus in his 1929 *Mind and the World Order*, Lewis made it clear that, for him, perceptual experience contains two elements: "something given and the interpretation or construction put upon it." (Lewis 1929, p. 192) The interpretative (or constructive) part of perception was by Lewis considered in a quasi-Kantian manner as what he famously called the pragmatic *a priori*. On the other hand, the given element is according to Lewis exhausted by the realm of sensory presentations. He therefore concluded that "[m]etaphysical issues which supposedly concern what is transcendent of experience altogether, must inevitably turn out to be issues wrongly taken. Why not a world of sensa with *nothing* behind them?" (1929, p. 148).

Both in his seminal 1932 book *The Philosophy of Physical Realism* and in his 1937 paper "Critical Realism and the Independence of the Object," Roy Wood Sellars extensively discussed Lewis's account of perceptual knowledge. His attitude toward the latter's view was twofold. On the one hand, he (quite enthusiastically) welcomed Lewis's 'dynamized' conception of *a priori* elements in perception (while at the same time interpreting these elements in a more naturalistic, evolutionary manner). Yet on the other hand, Sellars vehemently objected to Lewis's identification of the object of perception with the sensorily given. Being partially inspired by the findings of the German Gestalt psychologists, Sellars claimed that "[t]ranscendence […] expresses the realization that the object known is not given as the sense-presentations and concepts are, that knowing is *through* concepts and directed at what is intended but not given." (Sellars 1937, p. 547) Thus, when we talk about *this* being blue, we talk about a particular thing and not about the sensorily given alone. Exactly here, according to Sellars, "is the basic watershed of epistemology which divides the critical realist from positivist and pragmatist." (*ibid*.)

In the talk, I will argue that Sellars's critical realist conception has more to recommend it because it better explains the actual mechanism of perception Furthermore, I will point out that by arguing particularly against Lewis's (neo-)pragmatist account, Sellars anticipated the crucial position of his son Wilfrid's critique of the "Myth of the Given."

[115] Beyond the Clock as a Model of Living Beings: Leibniz's Distinction Between Natural and Artificial Machines

Duran, R. (*Playa Ancha University*)

During the seventeenth century, the clock (automatic machine par excellence) seemed the most appropriate model for thinking about living beings. Descartes, for example, thought living beings and their organic bodies were nothing more than machines, equivalent to human artifices, but more perfect because they were divine creations. Living beings, as divine machines, were superior to human machines because of the subtlety of their construction. For Descartes the difference between artificial and natural machines is only a gradual and not essential one. German philosopher G. W. Leibniz (1646-1746) thought otherwise. For him the distinction between natural and artificial machines is radical, essential. Natural machines are infinitely complex machines, machines within machines *ad infinitum*; while artificial machines do not, reaching a limit of complexity. Now, this distinction is not only structural but also functional: natural machines or living beings function as an organic whole, something that artificial machines cannot do. In some way, in each part of a natural machine there is a reference to the whole as a fundamental unity, and this unity in turn refers to the infinite parts composing the living being. A clock lacks this kind of unity, so for Leibniz it cannot be an adequate model of living beings. The distinction drew for Leibniz may seem arbitrary if we do not take into account two fundamental questions which Leibniz's model of living being try to answer, going beyond the mere mechanistic model based on the clock, questions the mechanistic model cannot answer in an adequate way: the explanation of the origin of the structure or form of the living being, and the activity of the living being as a dynamic and structural unity different from its environment and maintained over time. With his notions of natural machine and corporeal substance, Leibniz can solve these insufficiencies of the purely mechanical model. The living being constitutes in Leibniz a hierarchical and dynamic structure of monads, where one of them constitutes the dominant monad that gives unity to the living being and organizes the subordinate monads *ad infinitum*. Living beings' unity in Leibniz is a structural and dynamic unity, an organization, which the German philosopher calls "organism". In his model, living beings are not constituted mechanically or serially, but at once, by an act of creation and they are endowed with a principle of activity of their own. The scheme of the presentation will be: a) The clock as a model of the living being in the seventeenth century; b) Insufficiencies of this model: the problem of the origin of biological forms and their conservation over time; c) Leibniz's model of living beings as a way to surpass these problems.

[120] The double origin of Poincaré's conventionalism: Methodological structuralism and hypothetical-deductive method

de Paz, M. (*University of Seville*)

The origins of Poincaré's conventionalism have always been situated in problems about the status of certain scientific principles caused by the development of new scientific theories. Thus, it is typically a philosophical position emerged from scientific practice. For example, regarding geometrical conventionalism, it was the existence and consistency of non-Euclidean geometries what prompted the development of a philosophical position that could account for the status of the axioms of geometry without engaging in a discussion about their truth. Similarly, conventionalism in physics and mechanics was provoked by the discussion of the status of its fundamental principles, starting with the Newtonian laws of motion.

Conventions have been the object of several contemporary philosophical debates, e. g. concerning their agreement with modern science (for example, with general relativity), their rigidity, flexibility, or constitutivity in scientific theories, their relation to realistic or anti-realistic positions, and so on. Here I would like to consider conventionalism as a philosophical position originated from two specific methodologies proper to modern mathematics and the modern natural sciences: methodological structuralism and hypothetical-deductive method – thus, as a philosophical position which emerged from a way (or rather, two ways) of doing science. What is more important, I will try to show that these two methods are connected in both disciplines, geometry and physics.

First, Poincaré's claim that geometry is the study of the formal properties of a certain continuous group is typically a structuralist claim. And we will show that geometrical conventionalism could be understood as a consequence of this way of doing and understanding geometry. Second, Poincaré's defense of a 'physics of principles' where the conventional status of the principles is made explicit can be linked to a movement of abstraction in 19th century physics that is similar to the conceptual approach in mathematics (characteristic of the structuralist position). Third, Poincaré's discussion of the status of geometrical axioms and his reading of them as conventions is related to Riemann's understanding of geometrical axioms as hypotheses. The use of the term hypothesis implies the non-certain (hypothetical) character of the axioms, as well as the possibility of choosing different sets of them. Fourth, the treatment of physical principles as conventions also stresses their hypothetical character, since they cannot be established as true or false and alternatives are possible. Also, hypothetical-deductive method emerges by the mid-19th century and is visible in Riemann's manuscripts on mechanics and physics. In fact, the availability of alternatives and the possibility of choosing is what connect conventions and hypotheses.

The words 'convention' and 'hypothesis' have to do with methodological flexibility, that is, with the idea of having different conceptual possibilities, and the conceptual approach is typically represented by the structuralist position.

[121] Structuralism avant Dedekind?

Ferreirós, J. (*University of Seville*)

The methodology of structuralism can be found in a rather advanced form in the writings of Dedekind, who tends to be celebrated as the great pioneer of this 20th century style. One may say that Dedekind's structuralism was rather mature by 1871, although his ideas continued to develop into the 1880s. The historian can of course find precedents to Dedekind and in particular some mathematicians whose work influenced his way into structuralism. But, can one speak of structuralism before Dedekind? This will be our topic.

The talk will discuss ideas and methods found in Gauss, Galois, and Riemann – three of the most important influences for Dedekind. Around 1830, Gauss was publicly defending the idea that mathematics is "the science of relations", a viewpoint that links back with such great thinkers and mathematical philosophers as Descartes and Leibniz. By the same time, Galois had drafted his crucial contribution to modern algebra, advancing the study of equations and their solutions by means of the analysis of associated groups. This idea of establishing interconnections between some mathematical objects (the equation and its solutions) and some other, seemingly disconnected ones (the group of permutations) would become a landmark of structuralist methodology. But not everybody would pick it up in the same way. Contributions such as those of Gauss and Galois would start to open the way towards a more "conceptual" approach to mathematics, a way that was forcefully promoted by Riemann in his theories of functions and of manifolds. The case of Riemann is particularly interesting for HOPOS, since his methodology and insights were strongly connected with philosophical reflections, on the one side, and with new ideas and practices in science, on the other. Thus we shall devote more time to an analysis of his views, in particular (1) the link he established between analytic functions and some geometric objects, the Riemann surfaces, and (2) the way in which he proposed to reconsider the notion of physical space, starting from very general and abstract concepts.

[122] The secret life of notations: What mathematical drafts tell us about choosing and changing notations

Haffner, E. (*Bergische Universität Wuppertal*)

The so-called "conceptual mathematics" that emerged in the second half of the 19th century in the works of Bernhard Riemann and Richard Dedekind is based on the view that the development of a theory ought to be founded on concepts and avoid relying on specific forms of representations (*Darstellungformen*), such as indeterminate variables, infinite series, or specific notations. *Darstellungformen* are somewhat arbitrary, as they depend on the – necessarily subjective, even though deeply reflected upon – choices (of a variable, a notation, etc.) made by the mathematician. Such components should, Dedekind tells us, be avoided. In this talk, I consider two examples, taken from unpublished mathematical drafts, to show how some concealed considerations on notations can challenge our views on the matter.

My first example concerns the genesis of Dedekind's theory of Dualgruppen (equivalent to our modern lattice theory) ("Über Zerlegungen von Zahlen durch ihre größten gemeinsamen Teiler" in 1897, "Über die von drei Moduln erzeugte Dualgruppe" in 1900). The notion of *Dualguppe* finds its roots twenty years earlier, in module theory. In (Dedekind, 1877) the introduction of notations for divisibility (<), LCMs (–), and GCD (+) of modules allows for the formulation of new theorems, which display a specific kind of dualism (in any true formula, the operations can be switched to obtain a new true formula). Interested by this dualism, Dedekind pursued his investigations on the matter, which led to the introduction of *Dualgruppen*. Dedekind's Nachlass contains over 500 pages of drafts related to these twenty years of work. In these drafts, we can follow the slow, cumbersome elaboration of the theory through repeated computations and progressive generalizations. Dedekind appears to hesitate between several notations in this process, as the + and – symbols chosen initially are intrinsically related to modules. The many hesitations on this matter, which can be observed in Dedekind's drafts, show the importance of the choice of notations – even for Dedekind –, as well as possible criteria for this choice, e.g., the generality embedded in the notation.

My second example illustrates a different aspect of the choice of notations. I will consider the edition of Riemann's *Gesammelte Werke* (1876) by Heinrich Weber and Dedekind, and how their editorial work led Weber to alter Riemann's initial notations in some cases. The edition of Riemann's Werke was a tedious work, which took up several years, and required many editorial decisions in order to publish the best possible versions of Riemann's texts. These decisions were discussed in Dedekind and Weber's correspondence (ed. by Scheel, 2014) and are now available to us. The correspondence, as well as the study of Riemann's Nachlass, show that during the proofreading and correction process through which he took all of Riemann's texts, some of Weber's decisions regarding Riemann's notations were mainly due to practical considerations (e.g., ease of printing). These examples, not uncommon in the 19th century, invite us to consider the idea that criteria for the choice of notations can go beyond purely mathematical and conceptual considerations.

[123] Shifts in Hempel's Logic of Science

Dewulf, F. (*Ghent University*)

When, on 31 May 1939, Hempel gave a radio talk on the WIND Chicago Channel, he summarized logical empiricism to his layman audience as "the science of science, i.e. a study of the language of science". The recently developed formal logic was, according to Hempel, a perfect tool for this new study. The application of formal logic to scientific language would, however, prove the most challenging puzzle in Hempel's philosophical career in the United

States. In this paper I reconstruct Hempel's methodological shifts in the application of logic to the study of science. By investigating his work on scientific explanation, I distinguish between three phases. This distinction shows how Hempel, initially, separated the logic of science from the history of science and scientific practice, while later in his career he incorporated these perspectives on science in his logical analysis.

In Hempel's first paper after his migration to the United States, "The Function of General Laws in History", he uses formal logic to analyse the writings of historians. The aim of the paper is to show that historians presuppose hypothetical generalizations in their writings. Hempel reconstructs parts of historical texts in a formal scheme, which allows historiography to be evaluated on an empirical basis. In his personal correspondence from 1942, he received opposing reactions to this logical reconstruction. On the one hand Otto Neurath warns Hempel about the use of formal schemes that impose norms on scientific inquiry that are external to scientific practice. On the other hand Charles Stevenson advises Hempel to improve his logical analysis of scientific laws, and to bring it closer to language intuitions.

Hempel takes on Stevenson's arguments on laws, almost verbatim, in his 1948 paper co-published with Paul Oppenheim, "Studies in the Logic of Explanation". This paper initiates an analysis of scientific explanation from a set of intuitive cases and pays much more attention to language intuitions as norms to evaluate the logical account of scientific explanation. This methodological approach to the logic of science is far removed from the warnings that Neurath had given Hempel ever since they started corresponding extensively in 1935. I discuss several instances of this correspondence from 1935, 1937, 1942 and 1944 in which Neurath argues with Hempel about the importance of a "pragmatic-historicizing view on science". According to Neurath, the abstract, formal analysis of scientific language without the pragmatic historicizing view can only reify older metaphysical prejudices. Hempel was never persuaded, and at several points in his correspondence, he explicitly rejected the relevance of a historical perspective to the logic of science.

When Hempel evaluates the criticisms to the 1948 paper in his *Aspects of Scientific Explanation* (1965), he explicitly argues for his model by using language intuitions, formal test-cases and historical episodes in the sciences as seemingly interchangeable elements that can weigh in on the argument for a logical model. After 1965, Hempel would increasingly use a naturalized approach to his logic of science, paying attention to the pragmatic and historical elements of scientific practice.

[126] Understanding inter-theoretic contradictions and the many lives of historical reconstructions

Martinez-Ordaz, M. (*UNAM*)

Here I aim at providing interesting responses to two important questions from the philosophy of science, namely: Can philosophers of science benefit (in a significant way) from historically inaccurate historical reconstructions? and What has the history of science said about the limits of the philosophical thesis of inconsistency toleration in science? On the one hand, it is commonly argued that history of science has the main role of either supporting or falsifying philosophical theses (Popper 1934; Kuhn 1970; Lakatos 1970; Laudan 1977; Nickles 1986, 1995; Vickers forthcoming). Additionally, it is expected that, in order to fulfil such task, historical information is used highly accurately when facing philosophical claims (Pitt 2001, Schickore 2011, Kinzel 2015). On the other hand, the case of intertheoretic inconsistency is fascinating in itself, as not many case studies have been provided as exemplars of intertheoretic inconsistency. What is more important, some of these paradigmatic exemplars have been claimed to be historically inaccurate (Davey 2014). The combination of these facts leaves us with the impression that the history of science might have shown the limits of the philosophical thesis of inconsistency toleration in science. Hence the importance of addressing both questions together.

Here I will argue that historical reconstructions, even if not historically accurate, can play another equally important role: to enhance our understanding of philosophical theses about science by clarifying some of their concepts or applications (Martínez-Ordaz and Estrada-González forthcoming). Furthermore, I will claim that even if the reconstructions presented to illustrate intertheoretic inconsistencies are historically inaccurate, as it has been claimed (Davey 2014), this inaccuracy is not problematic enough for rejecting the philosophical thesis about intertheoretic inconsistency toleration in science. I will argue that such reconstructions have significantly helped to select and modify the methodological criteria used for identifying cases of intertheoretic inconsistency toleration, and thus, helped philosophers of science to achieve a better understanding of the phenomenon of inconsistency toleration in science.

In order to do so, I will proceed as follows: First I will introduce the debate around inconsistency toleration in science and I will argue that reconstructions of scientific episodes have the main purpose of increasing our knowledge, although not only about the reconstruction's object of study, but perhaps also about a particular case study, a specific scientific context, or even about our philosophical approaches to science. Later on, I will introduce the case of intertheoretic inconsistencies in science. Finally, I will explain how the historical reconstructions of intertheoretic inconsistencies in science, even if not historically accurate, could help us to achieve a better understanding of the general thesis of inconsistency toleration in science.

[128] Kolmogorov's solution (1933) of the Borel Paradox (1909)

Redei, M. (*London School of Economics*), Gyenis, Z. (*Jagiellonian University*), Hofer-Szabo, G. (*Research Center for the Humanities, Budapest*)

Suppose we choose a point randomly with respect to the uniform distribution on a sphere in three dimension. What is the conditional probability that a randomly chosen point is on an arc of a great circle on the sphere on condition that it lies on that great circle? Since a great circle has measure zero in the surface measure on the sphere, Bayes' formula does not give the conditional probability in question. But one has the *intuition* that the conditional probability of the randomly chosen point lying on an arc is well defined and is given by the uniform distribution on the circle. This tension between the notion of conditional probability defined by Bayes' formula and our intuition is known as the Borel Paradox.

This Paradox was formulated by Borel in 1909 [Borel 1909] and it has been extensively discussed both in mathematics and in philosophy of probability. In his 1933 book Kolmogorov argues that the Borel Paradox makes explicit an insufficiency in naive conditioning that can be avoided by formulating conditioning based on the concept of conditional expectation determined by a σ-field:

"[The Borel Paradox] shows that the concept of a conditional probability with regard to an isolated given hypothesis whose probability equals 0 is inadmissible. For we can obtain a probability distribution [...] on the meridian circle only if we regard this circle as an element of a decomposition of the entire spherical surface into meridian circles with the given poles. [Kolmogorov 1933](p. 51)

The probability distribution Kolmogorov offers as a resolution of the Borel Paradox is counterintuitive however: it is not the uniform distribution on the great circle. But the uniform probability seems to be the intuitively correct conditional probability on a great circle. How can one then assess Kolmogorov's solution?

In the talk we recall Kolmogorov's 1933 concept of conditioning based on the concept of σ-field, and show how *both* the uniform *and* non-uniform probability distributions on a great circle can be obtained as conditional distribution with respect to *different* conditioning σ-fields. We will argue that the sensitive dependence of the conditional distribution on the conditioning σ-field is intuitively perfectly acceptable and that the intuition that the uniform length measure on the arc is *the* correct conditional probability on a great circle is fallacious. It will be seen that the error in the intuition is the lack of clean separation of probabilistic and non-probabilistic concepts and reasoning in connection with the Borel Paradox.

The conclusion will be that Kolmogorov's theory of conditioning based on the concept of conditioning σ-fields offers a precise and flexible framework in which the Borel Paradox can be fully resolved and in which fallacious intuitions about conditional probabilities can be corrected.

The talk is based on a forthcoming paper in *Synthese*.

[129] Mathematical vs. logical necessity: The case of Bernard Nieuwentijt

Pauw, S. (*University of Amsterdam/Ghent University*)

This paper argues that, according to the Dutch philosopher Bernard Nieuwentijt (1654-1718), mathematical reasoning is not the same as logical reasoning. I show that Nieuwentijt is implicitly committed to a view that Macbeth (2017) ascribes to Descartes, namely the view that mathematical truths have a different type of necessity than logical truths. I also use Nieuwentijt's case to evaluate Macbeth's reading of Descartes.

Nieuwentijt's 1720 work on the nature of pure and mixed mathematics, *Grounds of Certainty*, has led various commentators to claim that he regards logic and mathematics as intimately related (Ducheyne 2017b, 287n.; Petry 1979, 6; Beth 1954, 451-452). In fact, there are important differences between mathematical and logical reasoning according to Nieuwentijt. Both in pure and in mixed mathematics we discover truths by mathematically deducing propositions from *ideas*, he thinks. Deducing propositions from ideas is something different than logically deducing propositions from other propositions for Nieuwentijt. This becomes clear from a striking claim he makes in his analysis of the nature of mixed mathematics, namely that it is possible to mathematically deduce false propositions from true abstract ideas. Nieuwentijt explicitly denies that it is possible to logically deduce false propositions from true premises.

This paper explains how mathematical reasoning on the basis of ideas differs from logical reasoning according to Nieuwentijt. I show that Nieuwentijt regards ideas as objects that have what Descartes calls objective reality (cf. Ducheyne 2017a, section 2): they are objects that reside in our minds. According to Nieuwentijt, a proposition is deducible from ideas if it can be shown that it makes a true claim about the objects that these ideas are. Logical and mathematical reasoning differ for Nieuwentijt, because logical reasoning does not involve the examination of ideas, I show. According to Nieuwentijt, ideas allow us to deduce truths that cannot be deduced logically. This helps explain why he deems it possible to mathematically deduce false propositions from true abstract ideas. Although Nieuwentijt does not speak of such things as logical truths, he is committed to the existence of such truths. These truths must have a different status than mathematical truths. For Nieuwentijt, mathematical truths are, in a sense, our own creations. However, his views on logic imply that logical truths are not.

Recently, Macbeth (2017) has argued that Descartes's claim that necessary truths are created by God does not apply to logical truths. Logical truths did not need to be created according to Descartes, Macbeth claims, because even God could not have made their negations true (2017, 19). Non-logically necessary truths such as mathematical truths, on the other hand, are true in virtue of rules that God created, and that could have been created otherwise (Macbeth 2017, esp. 19-22). I doubt that Descartes consciously distinguishes between logical and non-logical necessity in the way Macbeth suggests. However, there is reason to believe that, like Nieuwentijt, Descartes is implicitly committed to this distinction. I consider whether this suffices to defend Macbeth's thesis that Descartes's creation doctrine is restricted to non-logically necessary truths.

[131] The reception of Durkheim's sociological theory of the a priori in France and Germany, 1900s–1930s
Strauss, M. (*University of Vienna*)

In their essay on "primitive forms of classification" (1903) Durkheim and Mauss formulate a research programme for a historical-sociological enquiry into what was formerly thought to be a priori, i.e. the categories of understanding and the concepts of space and time. Traditionally thought to be universal, necessary and immutable, the categories were now presented as being socially constituted and historically variable. Durkheim's *Formes élémentaires de la vie religieuse* (1912) left no doubt about the challenge this posed to philosophy. The sociological theory of the categories was presented as an alternative to the doctrines of empiricism and apriorism. A "new science" was called for that would replace the dialectical methods of philosophy. Durkheim's coup thus amounted to depriving philosophy of its jurisdiction in one of its core areas and constituted a major intervention in the established hierarchy of disciplines.

In the literature, the genesis of this sociological theory of the a priori has received some attention. Roots have alternatively been identified in the French neo-Kantian philosophies of Renouvier and Hamelin (Stedman Jones 2001) and in Kant's reception in eclectic spiritualism (Schmaus 2004). This paper focuses instead on the early Twentieth Century perception and reception of the "Durkheimian challenge" (Rawls 1997) in two distinct national contexts. Was this challenge indeed perceived as such and embraced as a possibly interesting new approach to long standing questions; or was it was generally dismissed as preposterous and ignored? I will reconstruct the respective arguments and the intellectual, disciplinary and social positions of their supporters (see also Kusch 1995, Chimisso 2008).

I proceed in two steps. First, building on existing research I focus on early debates on the Durkheimian challenge in France. This involves responses to the initial research programme and its execution by members of Durkheim's group (Hirsch 2016); differences within this group (Marcel 2012); Durkheim's confrontations with the philosophical community in the *Société française de philosophie* (Soulié 2012); and the first reception of the *Formes élémentaires* (based on the rich material gathered in Baciocchi/Théron 2012). In a second step, I trace the transfer of Durkheimian sociology of knowledge to the German-speaking world and its connection with the German debates of the 1920-30s. Although there have recently been works on Durkheim's reception in Germany (Keim 2013, Fitzi/Marcucci 2017), a reconstruction concerning his sociology of knowledge is missing. While positive references to Durkheim's theory are rare at the time (Jerusalem being a noteworthy exception), I will argue that his account of the categories played an important role as a negative "positivistic" foil (e.g. Scheler 1924). The paper thus questions the often-rehearsed thesis that French and German debates on the sociology of knowledge were basically unconnected (Merton 1957).

This strategy allows for comparisons of the reactions to sociological theories of knowledge in two different contexts. It sheds light on the tensions between philosophy, sociology and other disciplines at the time and might thus contribute to a better understanding of the terms of recent debates on the relationship between philosophy and sociology of science.

[136] Jean Piaget's unpublished causality manuscripts: An archival discovery complexifying the Kuhn connection

Burman, J. T. (*University of Groningen*)

Prompted by the fiftieth anniversary of the publication of *Structure*, Kuhn scholars were led to delve more deeply into his connection to Jean Piaget (1896-1980), the Swiss developmental psychologist. Thus, for example, Kaiser (2016) reported that Kuhn had read Piaget's (1927/1930) *The child's conception of physical causality* when he was a fellow at Harvard, in 1948-1951, and that the influence was sufficient that—according to Kay, Kuhn's wife at the time—Kuhn was nervous to meet Piaget during his visit to Berkeley in the 1950s. Galison (2016) also examined Kuhn's notebook of 1949 and found an intensive engagement with Piaget's (1946) book on *Les notions de mouvement et de vitesse chez l'enfant* (which was translated in 1970 as *The child's conception of movement and speed*).

When coupled with recent discoveries at the Piaget Archives in the University of Geneva, this combination of influences turns out to be potentially very significant. They tie the early inspiration that Kuhn drew from Piaget to the interests that catalyzed the last of Piaget's unpublished projects. And these also reflect some of the same sorts of interests that Kuhn indicated that he had returned-to in his own unpublished book (viz. *The Plurality of Worlds: An Evolutionary Theory of Scientific Development*). One is therefore justified in wondering if both unpublished books, by Kuhn and Piaget, engaged with similar issues.

Piaget's unpublished project was launched, in print, by a book that was published in collaboration with Kuhn (Bunge, Halbwachs, Kuhn, Piaget, & Rosenfeld, 1971). This in turn represented a return to the same issues examined in *The child's conception of physical causality* and extended in *The child's conception of movement and speed.* Kuhn's own contribution, "Concepts of cause in the development of physics," was then published in English as Chapter 2 in *The Essential Tension* (Kuhn, 1971/1977). But the results of Piaget's multi-year research program were thought to have been lost: the archivist responsible for his other posthumous publications was able to find only 61 of the over-100 completed chapters mentioned in a published précis (Piaget & Garcia, 1971/1974). So the found-chapters were boxed, and then forgotten in the archive's unaccessioned collection.

In a separate recent effort, a new archival team found a series of then-unknown manuscripts after having been given access to the personal papers that had been kept in storage—privately—at the Piaget Villa (Ratcliff & Burman, 2015). It then turned out that this collection is able to fill the gaps in the boxed causality project held back at the university.

This is exciting for Piaget scholars: it's his last book, alike in anticipation to Kuhn's *Plurality*. Yet among the found-papers we also find a return to the original ideas that inspired Kuhn's *Structure*. So perhaps there is something for Kuhn scholars in Piaget's unpublished book too; perhaps not a remedy for the gaps in Kuhn, as Tsou (2006) sought, but certainly a tantalizingly similar source that should be read alongside Kuhn's own final book (cf. Burman, 2007).

[137] Feyerabend's re-evaluation of scientific practice: The 1957 Colston Research Symposium in Bristol and its consequences

Kuby, D. (*University of Konstanz*)

While Paul Feyerabend is often thought of as a philosopher who worked in general philosophy of science—and, since 1970, openly advocated its demise, in this talk I want to bring out Feyerabend's origins as a philosopher of quantum physics. My goal is to show that his long-standing preoccupation with the development of quantum physics (past and future) had a deep impact on his overall philosophical theorizing.

In 1957 Feyerabend delivered a paper on the quantum theory of measurement at the Colston Research Symposium in Bristol to present an alternative to the wave function collapse (in the form of von Neumann's projection postulate) that he deemed to be physically unsatisfactory and philosophically dangerous. Not only the nascent measurement problem controversy, but also the main Rosenfeld-Bohm discussion at the Symposium has recently gathered the attention of historians as a watershed moment in igniting the dispute on the interpretation of quantum mechanics, leading to a new reception of Bohm's program and also to the myth of a unitary Heisenberg-Bohr interpretation ('Copenhagen interpretation'). Yet I will show that the Symposium prompted Feyerabend to re-evaluate Bohr's work in the opposite direction, leading him to appreciate Bohr as an independent and original theorist. Still a follower of Popper's criticism in the philosophy of quantum mechanics at the time, the 1957 encounter with Rosenfeld initiated a cascade of consequences in Feyerabend's philosophy.

Through an in-depth study in the following years of the original literature of the first quantum revolution, Feyerabend came to reassess the physical motivations behind Bohr's interpretation of quantum mechanics, leading him to distinguish the physical arguments presented by Bohr from the philosophical preconceptions adduced to him. (I propose to interpret this use of history through Mach's historical-critical method, combining historicism with a presentist concern.) Feyerabend's analysis of complementarity as a sound scientific move also fuelled his interest in alternatives to quantum mechanics, in particular Bohm's theory: if the standard interpretation of quantum mechanics was here to stay, only a genuinely alternative theory could challenge quantum mechanics and provide the means for a new interpretation—laying out the template for a strong theoretical pluralism.

I will use this case study to sketch how the reference to scientific practice evolved in Feyerabend's conception of methodology. My main claim is that it was the universal scope of methodological arguments that was slowly but steadily put into question. Starting with Bohr's case, Feyerabend recognized for himself specific instances of arguably scientific theories in which differing methodological demands were legitimate because they 'made sense scientifically', putting a dent into Feyerabend's top-down methodological argument scheme—for a specific research situation, we arrive at contrasting demands whether we look at it from a general-methodological or from a contextual-scientific point of view. This contrast became more and more strident, until Feyerabend was forced to give up the universality of the methodological argument.

[139] Descartes on the unification of arithmetic and geometry via the theory of proportions

Crippa, D. (*The Czech Academy of Sciences*)

The relationship between algebra and geometry in Descartes' mathematics has been the source of dilemmas for scholars: on one hand Descartes attempted to bring geometry and algebra to unity by providing a method to represent curves (geometrical objects) via equations (algebraic ones), on the other he clearly maintained the logical and epistemological priority of geometry over algebra, as it shines through his practice in solving problems (construction of equations) or through his argument to justify the exactness of curves and their acceptability in geometry.

However, I think that this idea of a tension between geometry and algebra in Descartes is the fruit of a misconception, largely due to an anachronistic understanding of the meaning and role of algebra in the *Géométrie* (1637), Descartes' most accomplished mathematical work. As I would like to show in this paper, the essential contribution put forward by Descartes in the *Géométrie* consists precisely of the constitution of a "geometrical calculus" i.e. an algebra which deals with segments, namely geometric objects, and whose range of applicability includes any kind of homogeneous quantities, magnitudes, numbers, times, velocities, which can be treated by Euclid's theory of proportions. Algebra was thus the key to unify the domains of geometry and arithmetic, precisely because of its structure: it was constituted by Descartes by specifying a set of fundamental geometrical operations between segments analogous to the five fundamental operations of arithmetic. While this achievement can be read as a stepping stone into modern science, because it opens the possibility of a mathematical physics, it can be also understood in the background of ancient mathematics, since it responds to a question which crossed all Greek mathematics: how can geometry, or the science of continuous quantity, and arithmetic, or science of number, be reconciled?

In my contribution I shall discuss, by analyzing in detail the constitution of the Cartesian calculus both in Descartes' *Géométrie* and in the work of the earliest Cartesian mathematicians such as De Beaune, Frans Van Schooten and Erasmus Bartholin, how Descartes' mathematics articulated a convincing answer to this question. In particular, I shall focus on two key elements. The first is Euclid's theory of proportions, which provided arithmetical operations a geometrical basis and determined the scope and limits of algebraic reasoning; and the second one is the symbolic language (namely, the use of letters to denote known and unknowns), which endowed algebra with universality and abstractness.

[141] New Theories for New Instruments: Fabrizio Mordente's Proportional Compass and The Genesis of Giordano Bruno's Atomist Geometry

Rossini, P. (*Scuola Normale Superiore di Pisa*)

The proportional compass can be considered the first calculating instrument of the modern age. Relying on the Euclidean proportion that similar triangles have proportional corresponding sides, the compass allowed to perform several mathematical operations, such as dividing a segment or a circumference into equal parts, or squaring a regular figure. These operations, in turn, could be used to render measurement instruments (e.g. astrolabes, sundials, etc.) more precise. Hence, the proportional compass had a wide range of practical applications, which explains the great deal of interest it attracted during the early modern period.

The authorship of the proportional compass has been debated even since its invention. In 1606 Galileo published a text in which he described the operations of the "geometric and military compass" he had invented in 1597. However, there is evidence that different kinds of proportional compass circulated in Europe even before 1597. One of the first examples was that invented by Fabrizio Mordente in 1567. The history of Mordente's proportional compass has been extensively studied by historians of science (see Rose 1968, Camerota 2000). In this paper, I shall focus on an episode which, however, has received less attention, that is, the encounter between Fabrizio Mordente and Giordano Bruno.

Bruno and Mordente met in Paris in 1585. Puzzled by the novelty of Mordente's invention, Bruno offered to write a Latin exposition of the compass in the form of two dialogues. Yet Mordente must not have liked what Bruno had to say about his compass, as he tried to acquire and burn as many copies of Bruno's dialogues as possible. In response, Bruno wrote two other dialogues, in which he accused Mordente of plagiarism and stupidity. More importantly, in the two latter dialogues Bruno explained what was for him the 'actual' significance of Mordente's compass. In Bruno's eyes, the compass proved that magnitudes had an atomic structure.

In the following years, Bruno would go on to develop an atomist geometry based on the concept of *minimum*. However, it was in the dialogues on Mordente's compass that Bruno's atomist geometry had its origin. To prove this point, I shall give a step-by-step account of the argumentative strategy by which Bruno derived the existence of a geometric minimum from the functioning of Mordente's compass. This will also shed light on the complex interplay between practical and theoretical knowledge in early modern science.

[142] De Morgan on Barrett and Tetens: A British–Continental analogy in the history of statistic thinking?

Heinemann, A.S. (*University of Paderborn*)

During past decades, the history of scientific thinking has benefited from substantive research on the emergence of statistics as related to the development of insurances and actuarial mathematics. Some decisive approaches can be marked out in the second half of the 18th century – which from a British point of view belonged to the period of a national 'decline of mathematics' from around the Leibniz-Newton controversies onwards. However, actuaries' need for standardization of vital rates seems to have generated equivalents of one method in British and Continental authors alike while perhaps unconnectedly. It was called the method of expected number of deaths.

Interestingly, Niels Keiding claimed in 1987 that in Britain, the method of expected number must have been forgotten fast enough for variants of it to be re-invented independently from national forerunners about less than 20 years later. But as there seems to be no evidence for references to the British forerunners, the question remains whether the British re-invention could possibly have been mediated by the reception of Continental works, amongst which the most decisive one is was published by the German-Danish Johannes Nikolaus Tetens in 1785/86. Indeed one of Tetens's variants of calculation seems to have an equally indigenous origin within the phase of British re-invention. It was devised by the Surrey schoolmaster and later London actuary George Barrett and published by Francis Baily.

Apparently, the question of whether Barrett knew of Tetens remained controversial. This may be inferred from the fact that in 1854, a paper was published in the *Assurance Magazine* to valuate Barrett's merits and to vindicate his originality. Its author was the London professor of mathematics Augustus De Morgan, who was (amongst many other things) both a historian of science and earning himself some extra income as a counsel to insurance offices. But De Morgan's argument rested solely on an assumption derived from biographical information about Barrett, namely that there are reasons to believe Barrett did not read German and could not access Tetens's writings.

De Morgan's inquiries are documented by manuscript material held by the London Senate House Library. The file contains various correspondence, but there is one letter from De Morgan to Babbage in which the former confesses that the similarities between Barrett's and Tetens's accounts are striking and can hardly be accounted except on the assumption that Barrett was somehow informed of Tetens's approach. Departing from this account, the presently proposed contribution to *HOPOS 2018* aims at a reconstruction of De Morgan's arguments and takes as its goal to test whether there is a possibility of indirect influences between British and Continental actuarial mathematics, as exemplified by the case of Barrett.

[148] Hobbes's Mechanical Science of Conscience: A Textometric Approach

Rebasti, F. (*ENS Lyon*) and Heiden, S. (*ENS Lyon*)

More than a century after the advent of modern Hobbes scholarship, a major gap is still to be filled in the literature. Although all commentators agree that Hobbes's growing interest in theological issues is the distinctive feature of his political theory (see, for instance, Goldsmith 1969, Johnston 1986, Milner 1988, Pacchi ed., Hobbes 1989, and Malcolm ed., Hobbes 2012), interpretations diverge as to whether Hobbes's extended religious discussions have a 'scientific' significance (Pacchi 1998) or an 'extra-scientific' one, i.e. a theological (Warrender 1957, Hood 1964, and Kodalle 1972) or a rhetorical (Johnston 1986, Tralau 2011, and Fiaschi 2014) grounding. Nevertheless, the changeable use over time the philosopher made of biblical exegesis and theological arguments from The Elements of Law (1640) to De Cive (the Latin expanded edition and its English translation appeared, respectively, in 1647 and 1651) and *Leviathan* (1651) has not yet been the object of a methodical survey. With a view to helping clarify the nature and causes of Hobbes's evolving 'theology', we advance a new, systematic approach, relying on the powerful textometric functionalities of the TXM-based corpus of digital diplomatic transcriptions of Hobbes's English political works built by the CACTUS research team (IHRIM – ENS de Lyon). We start with a brief presentation of the constitution of the corpus, outlining the way in which EEBO-TCP XML TEI P5 files have been refined and imported into the open-source and TEI-oriented TXM software. After expounding the specificities of the resulting edition, we illustrate the promise of the 'textometric approach' by testing it against one of the most relevant and puzzling cases of discrepancies characterizing Hobbes's production: the marked variability in the explicit theological references peppered throughout the political writings. We therefore show how, besides systematizing the study of Hobbes's substantial additions and modifications, the TXM software leads to an original interpretation of Hobbes's 'theological' incongruities, according to which they served the purpose of his scientific foundation of morals. In so doing, we firstly describe the TEI markup strategy adopted to index Hobbes's explicit biblical and theological quotations; then we introduce the advanced textometrical functions used to trace their developmental pattern, investigate the philosopher's vocabulary and contextualize his argumentative discrepancies. While shedding new light on the inner evolution of Hobbesian thought in its intellectual context, collected data will reveal that, in order to establish the certainty of scientific knowledge over the probability of dogmatism, Hobbes had to undermine the corruption of men's cognitive and motive powers perpetrated by rhetorical and casuistic thinking. By explaining how Hobbes rigorously naturalized Christianity so as to pave the way for his mechanistic reform of consciences, the 'textometric approach' will demonstrate its effectiveness with respect not only to Hobbes studies, but also to the history of philosophy and ideas.

[150] From notational change to substantial discovery: Leibniz, Bernoulli, and the exponential notation for differentials

Waszek, D.E. (*Pantheon-Sorbonne University/IHPST*)

The aim of this talk is to analyze the precise role of a notational innovation in a famous episode of mathematical discovery by Leibniz and Johann Bernoulli, and thereby to shed light on Leibniz's explicit philosophical views on notations.

In 1694, in a letter to Bernoulli, Leibniz introduces his exponential notation for differentials, writing d^2x for ddx, d^3x for dddx, etc. At first, this seems to be little more than a convenient shorthand. Yet in subsequent letters, Leibniz and Bernoulli are quickly led to a host of discoveries in which this new notation appears to play a central role. First, Leibniz realizes that this notation can be extended to negative powers, with d^{-1} corresponding to the integral, a modification which allows writing some general formulas valid both for differentials and for integrals. Then, he discovers the so-called "Leibniz analogy" between powers and differentials, which links the formula giving the n-th differential of a product, d^n (xy), to the binomial formula giving the n-th power of a sum, (x+y)^n. Finally, Bernoulli explores the possibility of actually manipulating the d^n symbols like usual powers, and discovers that this can, surprisingly, lead to correct results.

What is the exact role of Leibniz's new notation in these discoveries?

One should be careful not to attribute too much to the notation on its own, as if it had been introduced by luck and then automatically led to further progress. Leibniz did not choose it blindly: before introducing it, he already believed there was a kind of structural analogy between powers and roots on the one hand, differentials and sums (i.e. integrals) on the other. So his subsequent discoveries were partially the result of a conceptual insight. Moreover, I shall argue that this notational change did not, at least at this stage, increase our authors' expressive power: everything they did can be rephrased without using it.

Yet the new notation does seem to help. In fact, Leibniz even appears to introduce it as a deliberate research strategy, in keeping with his explicit methodological views on the use of symbols as an art of discovery. How can a new notation be a successful tool for discovery? Leibniz's methodological stance has sometimes puzzled commentators: Serfati (2008) even describes Leibniz's continual play with symbols as an "irrationalist" practice. By explaining how such a strategy can succeed, we can make better sense of his position.

I shall argue that Leibniz's notation makes our authors' subsequent discoveries more accessible in two main ways. First, it brings out patterns in the formulas which would be much less salient – much harder to notice – otherwise; it is such a pattern, I believe, that led Leibniz to his analogy between d^n(xy) and (x+y)^n. Second, it makes some relevant formulas and manipulation rules easier to guess and to remember, both relatively to Leibniz's and Bernoulli's preexisting experience with powers, and intrinsically, because it makes them shorter and more regular.

[153] Émilie du Châtelet and Christian Wolff on Hypothesis and the Foundations of Physics

Prunea-Bretonnet, T. (*University of Bucharest*)

The purpose of this paper is to analyze Emilie Du Châtelet's conception of hypothesis as it is articulated in *Foundations of Physics* (1740-1741) and to compare it to Wolff's treatment of hypothesis in the *Discursus praeliminaris* (1728) and in *De hypothesibus philosophicis* (1737). An important figure of the French Enlightenment, Du Châtelet played a major role (alongside Voltaire and Maupertuis) in the so-called 'Newton wars' against Cartesian thinkers and actively contributed to the dissemination of Newtonian ideas in France. Despite her admiration for the new physics, in the late 1730s she became increasingly dissatisfied with what she considered to be a lack of metaphysical foundation of Newtonian science. She believed to have found the necessary metaphysical complement to natural philosophy in the 'Leibnizian-Wolffian' speculative philosophy and especially in the principle of sufficient reason, regarded as the "compass capable of leading us in the moving sands of this science" of metaphysics, as it is argued in her *Foundations*.

It is the aim of this paper to study how her conception of natural philosophy, based on observation, experience and a Newtonian approach, is articulated to the metaphysical principles specific to Wolffian philosophy in the *Foundations*. Therefore, the paper first deals, in an introductory part, with her 'conversion' to German metaphysics as it is formulated in her correspondence with Frederic the Great, as well as acknowledged in the preface of the *Foundations*. It then examines Wolff's treatment of hypothesis in the *Discursus praeliminaris* and in *De hypothesibus philosophicis*. The third part of the paper is dedicated to the analysis of Du Châtelet's conception of hypothesis and of the relationship between hypothesis and the principle of sufficient reason in the *Foundations*.

The main claim is that despite striking similarities with Wolff's thought, Emilie managed to achieve an original and consistent synthesis (or, according to Wolff, a '*connubium*' or 'marriage') between experience and reason, between physics and metaphysics that is particularly manifest in her understanding of hypothesis and in the role conferred to the principle of sufficient reason in its treatment. However, even if she criticized the Newtonian rejection of hypothesis and endorsed metaphysical principles, Emilie Du Châtelet did not abandon her Newtonian convictions, as it is sometimes argued, but elaborated an ambitious attempt to bring together seemingly opposite doctrines within a philosophical project firstly traced by Wolff and later also taken up by Maupertuis.

[163] The Mind–Body Problem and Conservation Laws: An Outline in Light of the Growth of Physical Understanding

Pitts, J. B. (*University of Cambridge*)

The success of science, especially physics, is often invoked as contrasting with the degeneration of world-views involving immaterial persons, whether purely spiritual or embodied. A perennially popular question from the 17th century to the 21st is how, if at all, human souls can interact with bodies in light of physical conservation laws. (Recently popular property dualism, if not epiphenomenalist, faces a similar question.) This question has survived a transition from a time in which educated opinion generally took interactionist mind-body dualism for granted to a time in which that view, or any other involving souls, is widely rejected. Whereas initially this mind-body problem was something like a Kuhnian puzzle that must have some kind of solution, later it became a widely received objection against (interactionist) mind-body dualism. Leibniz was an early proponent of this objection in defense of a non-interactionist dualist view, pre-established harmony.

This work aims to survey how this conservation law issue has been treated over the centuries, especially how it did (or did not) reflect relevant theoretical and experimental knowledge pertaining to conservation laws, as well as how well it worked as an argument (which, *e.g.*, ought not to beg the question). Leibniz's *Theodicy* presents his objection as due to a growth in physical knowledge about conserved quantities since Descartes's day: whereas Descartes accepted a conserved quantity of motion, Leibniz accepted a conservation of a directed vector quantity (momentum) as well as *vis viva* (an ancestor of energy), which was controversial. In the 19th century, energy conservation was accepted. In the later 19th century with the rise of electromagnetic waves, the handful of global conservation laws associated with point particles acting at a distance was replaced (in serious physics) with *local* conservation laws for each part of the world separately; in favorable circumstances the local laws can be integrated into a global law. The local laws are (in those favorable circumstances) logically stronger, but they also permit milder failure modes. In the 19th and 20th centuries, a connection between conserved quantities and symmetries of physical laws came to be understood, especially in connection with the principle of least action culminating in Noether's work in 1918, which also included a converse: a symmetry implies a conserved quantity and *vice versa*. Also quantum mechanics appeared, with unclear implications.

Besides Leibniz, the issue engaged Euler, Kant, Maxwell, Helmholtz, Broad, and others, and continues to appear frequently in the contemporary philosophy of mind. While the understanding available from physics has grown or in some cases changed, the philosophical treatment has remained largely static in roughly the physics of the 1860s among both friends and foes of interactionist dualism (with occasional exceptions). General Relativity, now over a century old, also affects the discussion, albeit not in ways thus far proposed. This paper aims to survey the growth of knowledge on the conservation law mind-body issue.

[164] The relationship of early German-speaking sociology to philosophy

Kristinsson, D.G. (*Humboldt University of Berlin*)

The focus of my paper is the bifurcation of sociology and philosophy and its historical setting. In the German-speaking world this relationship has hitherto received scant attention individually or diachronically. Moreover, a comparative analysis of the stance of sociologists towards philosophy is still pending. What tasks did these pioneering sociologists envision for the philosophy of the future while simultaneously offering a blueprint for the design of their own nascent discipline? Pierre Bourdieu and his colleagues have done several studies on this relationship in France from a sociological perspective, mostly refrained from an in-depth reading of philosophical texts. In contrast, my study is anchored by in-depth reading of original texts. It is less a traditional contribution to the history of philosophy or sociology but rather to the history of their differentiation, an investigation of slowly emerging borders and the ambivalence of disciplinary border control.

Most of the sociologists who during the years 1883–1909 represented the emerging German-speaking sociology were qualified to teach philosophy at the university level (*Habilitation*), i.e. Stein, Barth, Eleutheropulos, Jerusalem, Tönnies, Simmel, or, as in the case of Gumplowicz, at least aspired a *Habilitation* in philosophy (of law and of the state). Many of them ultimately sought to recast the philosophy of society as a synthesis of the philosophies or generalizations of the numerous individual social sciences, echoing Wundt. The new discipline would be christened sociology (*Soziologie*), and was envisaged as a new kind of social knowledge: an inductive metaphysics of society. This rehabilitation did not entail establishing yet another independent circumscribed philosophy, as was the case for neo-Kantian theory of knowledge or phenomenology. On the contrary, during this foundational period the divisions between sociology and philosophy remained fluid and often the early sociologists were little concerned with a clear separation of sociology and philosophy, accordingly sometimes using the names of recent philosophical disciplines (*Geschichtsphilosophie*, *Sozialphilosophie*, *Kulturphilosophie*) as a synonym for *Soziologie*. A common past limited the drawing of boundaries between sociology and philosophy, however much these sociological pioneers tried to distance themselves from traditional speculative philosophy. They felt an urge to reform reigning academic philosophy and sociology was seen by many of its own practitioners as an extension and diversification of philosophy – a view shared by some traditional philosophers, although the majority held a skeptical attitude towards the young discipline. Sociology heralded a new scientific way of philosophizing. From the perspective of early representatives of German-speaking sociology the inductive basis and the analytical tool to observe the laws of social life was less social statistics than historical and everyday experience. Such a reading has clear historiographical consequences: because sociology and scientific philosophy were still kindred enterprises around 1900, both belong to the history of German-speaking philosophy, in marked contrast to the canonical picture presented by Herbert Schnädelbach who excludes sociology as well as philosophy of society from his *Philosophy in Germany 1831-1933*. In my paper I recall the forgotten fluidity of these disciplinary boarders by means of selected examples.

[168] Matter in Motion: Francis Bacon on Action at a Distance

Rusu, D. C. (*University of Groningen*)

The aim of Francis Bacon's philosophy was the production of effects: the change and manipulation of bodies. A manipulation that did not require contact between bodies was superior and more powerful than the manipulation of those bodies that are in touch. This means that Bacon was particularly interested in those phenomena that were defined as "action at a distance." But, according to Bacon, in order to possess such a power to change distance bodies, two steps had to be taken. First, to eliminate those credulous stories of demonic magic or with Neo-Platonic and Ficinian influences. Second, to understand how this communication between bodies that seem not to be in contact takes place. For Bacon, there was no such thing as "pure action at a distance." No interaction could occur without communication of matter. Even if we talk about contagion, magnetical attraction or transmission of thoughts, all these imply the transmission of matter between the two bodies. It is only a very subtle pneumatic matter that can be communicated, and this communication took place only at very short distances. Through this kind of transmission of matter in motion, the human mind could affect, according to Bacon, other minds and bodies: it can induce thoughts, passions and diseases in human beings, and it can change the motion of growth and generation of plants and animals. This is based on Bacon's assumptions about matter: (1) that the human mind is material and (2) that this matter is not different than that of all the bodies in the universe except in subtlety and quickness of motion, which explain their interaction.

The aim of this paper is to show that Bacon's conception of matter, even though influenced by vitalist concepts, is closer to the corpuscularian theory. In discussing transmission of thought from one mind to another or the transmission of what could seem like qualities, Bacon concludes that matter in motion is the only thing that can transmitted. Put differently, the pneumatic matter entering the 'passive' body can set in motion the particles of this body, change the type of motion they have, or increase or diminish an existing motion. To give an example, in the case of contagion, the pneumatics from a putrefied body would enter a healthy body and start there a confused motion which would eventually lead to the putrefaction and dissolution of this body. According to Bacon, transmission of thoughts or passions work in the very same way. The study of Bacon's conception about action at a distance does not only contribute to Baconian studies, but also to the studies of the sources of corpuscularian matter theory.

[169] Stahl was often closer to the truth: Kant on animism, monadology and hylozoism

Pecere, P. (*University of Cassino and Southern Lazio*)

In the *Dreams of a spirit-seer elucidated by dreams of metaphysics* (1766), Kant remarks that Stahl, with his admission of "immaterial forces" for the explanation of organisms, was "closer to the truth than *Hoffmann* and *Boerhaave*, to name but a few" (AA 2: 231), although the latter adopted a "more philosophical method". This puzzling statement is very significant for the understanding of Kant's reception of animism, as it documents Kant's reaction to the issues raised by the Stahl-Leibniz controversy in the *Negotium otiosum* and a striking preference for Stahl's non-mechanistic account of organisms. Kant agrees with Stahl that organisms suggest the existence of immaterial thinking beings, but at the same time the example of this speculative hypothesis leads him to question the explanatory power of metaphysical hypotheses in natural philosophy in general, as well as the possibility of empirically distinguishing among different hypotheses, such as monadology, materialism and hylozoism. After the analysis of Kant's sceptical conclusions in the *Dreams of a spirit-seer*, I show how this earlier connection of medicine, life-sciences and metaphysics leaves traces in criticism, by analysing Kant's discussion of Samuel Soemmering's claim that matter "can be animated [*animiert*]" in *On the organ of the soul* (1796).

[176] From objects of wonder to “perceptive instruments”: The mathematization of natural magic

Jalobeanu, D. (*University of Bucharest*)

This paper investigates some of the traditional “objects of wonder” common to Renaissance books of secrets, natural history and natural magic from the perspective of one common function they sometimes share, i.e., their “perceptive power” to detect hidden virtues of natural bodies.These objects can be extremely diverse: a musical chord, a heliotrope, “the Moon-herb,” “the beard of the wild oat” or a cucumber were, at some point or another, playing the role of “perceptive instruments” in experimental scenarios aiming to detect, respectively, the nature of resonance, the “degree of humidity” in the air, the extent and limits of sympathy between some plants and the Sun, the powers of the Moon or the capacity of some plants to attract water at a distance. There are other, more familiar perceptive instruments; some classified as “mathematical” (such as the weatherglass or the magnetic needle), some as “philosophical” (such as microscopes or “sunflower-clocks”). My proposal is to show that many of these apparently diverse objects of wonder were used in experimental scenarios with a common purpose: to detect (and sometimes to measure) “borders” and spatial variations of particular powers and virtues. In more sophisticated experiments, such “perceptive instruments” were even used to chart particular orbs of virtue and various forms of “consent” between bodies.

Many perceptive instruments begun their career in books on natural magic; since the magician’s work with natural virtues was said to be governed by “number, weight, measure, harmony, motion and light” (Agrippa, 1650, 167). But the use of these instruments was not confined by disciplinary borders; thus, quite often, the same instruments were also used in mixed-mathematics, mechanics, navigation, geography or medicine. Perceptive instruments were used by people with very different theories about the nature of virtues they want to measure, or about the precise mechanisms of transmission and interaction. In this paper, I will focus on a number of such instruments which can be found, successively, in the works of Giovanni Battista della Porta, William Gilbert, Girolamo Cardano, Francis Bacon, John Wilkins and Robert Hooke. I will investigate some of the experimental scenarios in which these perceptive instruments were used with the explicit intention to provide the investigator with quantitative answers to his questions. My general claim is that a thorough discussion of perceptive instruments will disclose a variety of early modern “forms of mathematization” which have escaped, so far, thorough scholarly investigation. I will show that manipulations of perceptive instruments lead to sophisticated forms of quantification and mapping, sometimes raising interesting theoretical and methodological questions regarding the ways to record correlations and variations. Moreover, I will also show how the use of perceptive instruments made experimenters aware of issues regarding “precision” and “error,” leading sometimes to the development of sophisticated experimental techniques of measurement.

[177] Leonhard Euler on vibrations and the general solution to the problem of the string

Mihai, I. (*Ghent University*)

This paper takes up Leonhard Euler’s mid eighteenth century conception of the general mathematical solution to the problem of the vibrational motion of the taut string, and argues that its intellectual roots do not pertain to mathematical inquiry, but instead to natural philosophical conceptions of continuous motion. This interpretation goes against the widespread treatment in the scholarship according to which the general solution is what arises in the aftermath of having solved the problem formally, and, in the way Euler constructs it, the general solution is nothing more than a repository of functions written in mathematical formalism, satisfying the equations of motion, and then accepted as solutions.

The received scholarly treatment of this issue has fallen short of providing a philosophical understanding for Euler's singular (and to a great extent extravagant) position in the controversy of the vibrating string. Euler held that all curves, including the discontinuous ones, are suitable to model the shape of a string as it vibrates. His stance was highly debated and criticized by Jean d'Alembert, Daniel Bernoulli and Denis Diderot in the controversy of the vibrating string which spanned more than two decades.

In this paper, I reconstruct the way in which Euler's understanding of the general solution for the string is shaped by his engagement with earlier attempts at making sense of the motion of the string, which pertain less to mechanical or mathematical theorization and problem solving, but instead to the natural philosophical stances on the continuum. Thus, I show that (1) there is a debate on the nature of the vibrational process outside of the mechanical approach that (2) problematizes the limits of the mechanical approach to the problem of the motion of the string. (3) Euler is attuned to this debate, so that, despite his critical stance towards it, he comes to share concerns about the way in which his mechanical rendering of the motion of the string should incorporate descriptions of the vibrational process. (4) His conception of the general solution to the problem of the string is informed by this strand of thought, which builds up towards the inclusion of discontinuous curves. In the end (5) I turn to the formalistic view on the general solution to the string problem and reassess it as a legacy of the historiography of the vibrating string controversy.

[178] Gassendi vs Astrology. Corpuscularism and Action at a Distance in Early Modern France

Garau, R. (*Humboldt University of Berlin*)

Astrology was still a flourishing discipline in the seventeenth century, despite the decline of spherical cosmology which constituted its theoretical background and the criticisms of some major exponents of the Renaissance such as Pico della Mirandola, in part due to its application to medicine, meteorology, as well as to horoscopes and foretelling (Thorndike 1955, Wright 1975). In this framework, the role played by corpuscularian natural philosophies was ambiguous. On the one hand, the idea that causation is to be understood in terms of interactions of bodies or particles challenged the core of the astrological practice, which was based on the assumption that stars and planets could cast an influence at distance on the lives of men. On the other hand, other natural philosophers saw in the corpuscularian framework an occasion to explain on different grounds the causal mechanism of the astrological influence. While causation could not be anymore attributed to the communicating motion of the celestial spheres (whose existence was now rejected both by Keplerian and Tychonian astronomies), some corpuscularian philosophers endeavored to establish astrology on mechanistic terms.

Exploring Pierre Gassendi's argument against astrology, and reconstructing the context of the polemic on astrology in early modern France, this paper presents a case-study of how corpuscularian philosophers tackled the issue of action at a distance. Furthermore, it illustrates a significant example of the broader cultural impact of corpuscularian theories on the early modern intellectual world.

On the basis of his vision of nature inspired by Epicurean philosophy, Gassendi elaborated a theory of light based upon atomistic and vacuist principles. Such theory was then applied to astrology, with the goal of distinguishing its theoretical and legitimate use (which Gassendi names astronomy) from its predictive and illegitimate one (judicial astrology). While he did not reject the idea of an influence of celestial bodies on human lives (as the sun and the moon clearly show the contrary), Gassendi objected that the actions at distance of remote stars and planets were to be reinterpreted in corpuscularian terms, and, as such, he deemed them causally irrelevant and empirically unobservable.

This presentation then reconstructs the context of Gassendi's criticism of judicial astrology, with particular reference to his argument with the astrologer and mathematician Jean-Baptiste Morin. By analyzing this polemic, I show some of the broad cultural implications of Gassendi's rebuttal of astrology in seventeenth century France.

[179] The Concept of Species in Schelling's Philosophy of Nature

Azadpour, L. (*KU Leuven*)

In this paper, the main objective is to address Schelling's understanding of species from the perspective of the relation between the speculative character of his system and the role given to empirical research within his philosophy of nature. I will argue that Schelling's position on species gives a distinctive account of the relationship between his speculative philosophy of nature and the value of experimental data as the basis of the formation of a principle of species divisions. The paper will revisit the role of the *a priori* within this system, which cannot simply mean prior to and/or independent of experience. This will provide the context for understanding his proposed principles of classification, in which the key philosophical issues addressed will be as follows:

(A) The ontological status of various classifications of living beings: the extent to which universals exist apart from the individuals which instantiate them, the way individuals are able to instantiate universal concepts, given the relation between transcendental idealism and speculative philosophy of nature.

(B) The kinds of unity proposed for species: how the unity of a particular species differs from the unity of individual living organisms, the role of natural history in classification, and how the classification of living beings is distinguished from other classifications, e.g. inanimate natural kinds.

(C) Schelling's seeming adoption of two contradictory accounts of species within his discussion of embryological development in the *first Outline,* where he seems to be dealing with varying definitions of freedom – a discussion that cannot be made sense of without reference to both its philosophical and scientific sources.

In turn, this investigation will show the prominence of empirical research in Schelling's philosophy of nature, in order to combat readings that portray Schelling as anti-empirical and consequently characterise *Naturphilosophie* as having had a negative effect on the progress of science (in line with Nassar (2014), Richards (2002)). The project will assert that the developments in the emergent life sciences were significant for Schelling's philosophy, as will be evidenced not only by the fact of his treatment of the species issue, but also by the role played by the concepts of species and genus in the structuring his own philosophical positions.

Among scholars who address Schelling's philosophy of nature, the issue of species is often overlooked (e.g. Schwenzfeuer 2012, Küppers 1992). Matthews (2011) briefly discusses species, but only in the context of the early *Timaeus* commentary. Richards (2002) gives the most extended treatment, but does not note or discuss the significance of the juxtaposition of apparently contradictory accounts of species within Schelling's *First Outline*.

[180] Soul as Nature: The naturalist animism of Van Helmont and Stahl
Demarest, B. (*University of Amsterdam*)

Since the mid-18th century, Jan Baptist van Helmont and Georg Ernst Stahl have been presented as the clearest and most prominent Early Modern proponents of an extreme and untenable position in the life sciences: animism. On such a theory, the soul is directly or indirectly involved in producing and governing some natural phenomena. In this paper, I show that van Helmont and Stahl develop a similar strategy in arguing for this seemingly commitment to naturalism. Both authors offered a sustained analysis of Aristotle's definition of nature and his distinction anti-naturalist view. I argue that they both attempted to show that their animist perspective was required by a between kinds of causes. While these analyses are arguably ineffective as criticisms of Aristotle, they do form an attempt to reconsider the place of teleology and finality in nature. In van Helmont's case, the analyses serve to support the thesis that conceiving of finality as originating in an external, intentional agent reduces natural phenomena to supernatural or artificial ones. As a result, van Helmont insists that natural explanations in medicine require the assumption of internal teleological agency in natural entities. Stahl is led to a similar conclusion by his own transformation of the Hippocratic concept of nature as actively involved in sustaining and restoring the integrity of the body. He suggests that the soul is nature, and defends this by developing an account on which the immateriality of the soul does not imply that it is unnatural or supernatural. On this account, life itself consists in movement, and since movement is itself immaterial, the soul's immateriality does not imply that it is fundamentally removed from the movements governing the material and mechanical systems of which the body is composed. In his debate with Leibniz, it becomes evident that Stahl developed his concept of the soul in opposition to both the more mechanicist accounts of some of his contemporaries, and to earlier animist and vitalist theories. I argue that Stahl's objections to such rival theories of the soul and of its role in natural philosophy, reflect disagreements on the division between phenomena that can be regarded as natural, and those that are to be regarded as un- or supernatural. Hence, my analysis suggests that debates on animism and vitalism were often as much about the nature of "nature" and of naturalist explanation as about the need for non-natural explanations in the life sciences.

[183] Constructing the Organism in the Age of Abstraction
Chirimuuta, M. (*University of Pittsburgh*)

This paper examines the mutual influence between Ernst Cassirer (1874-1945) and his cousin, the neurologist Kurt Goldstein (1878-1965). For both Cassirer and Goldstein, views on the nature of human cognition were fundamental to their understanding of scientific knowledge, and these views were informed both by philosophical theorising and empirical research on pathologies of the nervous system.

Between the wars, Goldstein published a series of famous case studies on brain damaged WW1 veterans with the Gestalt psychologist Adhémar Gelb. This activity culminated in the book published by Goldstein in exile, *Der Aufbau des Organismus: Einführung in die Biologie unter besonderer Berücksichtigung der Erfahrungen am kranken Menschen* (translated for publication as, *The Organism: A holistic approach to biology derived from pathological data in Man).* In this paper I show how Goldstein's theory of nervous operation and cognition are knitted together with his epistemology of biological research, and how the latter is influenced by Cassirer's philosophy of symbolic forms. For example, I examine how Cassirer's notion of the conceptual as characteristic of scientific activity is extended by Goldstein (1934/1995, p. 307-8) who writes, "The attainment of biological knowledge we are seeking is essentially akin to this phenomenon –

to the capacity of the organism to become adequate to its environmental conditions…. [T]he cognitive process of the biologist is subject to practically the same difficulties of procedure as the organism in learning; he has to find the adequacy between concept and reality."

In contrast to Harrington (1996), I argue that Goldstein's methodological prescriptions are not straightforwardly holistic, but require the biologist to alternate between holistic and "dissective" ways of characterising living organisms (Goldstein 1934/1995, p.316). Following Cassirer, and in aggreement with the contemporary logical empiricists, Goldstein held that the physical sciences had progressed by arriving at abstract, mathematical forms to take the place of qualitative characterisations of empirical reality. Unlike the logical empiricists, Goldstein was not sanguine about the fruitfulness of the abstractive approach in biology. An interesting point of comparison is with the other famous *Aufbau* treatise of the era, Carnap's *Der Logische Aufbau der Welt*. Whereas Carnap constructed the scaffolding for a unified science operating according to mathematical and logical principles, Goldstein argued that biology must retain descriptions of the "qualities" that are excluded by mathematical abstractions (Goldstein 1934/1995, p.315).

As Friedman (2000, p.155-6) relates, the rejection of mathematical logic as the unifying language for natural and human sciences motivated Cassirer's philosophy of symbolic forms as a means to provide a systematic epistemology for the non-mathematical disciplines. Friedman points also to Cassirer's failure to adequately buttress his claims for the "underlying unity" of the symbolic forms in human cognition as a reason for the failure of his programme. I examine the ways in which the neurological findings of Goldstein and others provided inspiration, if not ultimate vindication, for Cassirer's project.

[184] Formal teleology and geometrization: The Principle of least action in the early 1900s

Stoeltzner, M. (*University of South Carolina*)

The two decades around 1900 saw significant progress in variational calculus and its physical counterpart, the principle of least action (PLA). On the one hand, Karl Weierstrass and David Hilbert found, for the first time, sufficient conditions for a minimum and put a field so often plagued by counterexamples on secure foundations. On the other hand, all newly discovered physical theories could be formulated in terms of a PLA. This revitalized, among some, the belief that there was something special about these 'optimal forms'. But they had to overcome a major burden of the thinking launched by Euler and Maupertuis: its association to physical theology. At the end of the 18th century, Lagrange and Kant had collected the ashes of the long polemics, focusing on the calculus itself and classifying the principles as one among several maxims of subjective formal teleology (*Zweckmäßigkeit*). The aim of this paper is to investigate to what extent a Kantian analysis is able to assess the thinking of two major players of the day: Max Planck and David Hilbert.

By 1900, even empiricists, among them Ernst Mach and Joseph Petzoldt, had to acknowledge the PLA's success. Their solution consisted in the idea of unique determination that was taken from Leibniz, though bereft its metaphysical embedding. Planck tried to avoid the historical baggage by a two-tiered strategy. On the one hand, he argued that a PLA was only meaningful once all the possible motions and the boundary conditions had been specified. On the other hand, he diagnosed that the PLA had weathered all scientific revolutions by being an abstract form that – for each new scientific theory – had to be specified by a Lagrangian and a new constant of nature. This allowed Planck to combine a somewhat Kantian outlook on the principle with his realism concerning the physical world picture.

Hilbert typically listed the PLA as an example of a non-Leibnizian pre-established harmony between mathematics and physics. In his ambitious 1916 "Foundations of Physics", combining

Einstein's general theory of relativity with a simple model of electrodynamics, he believed to have achieved a complete geometrization of physics by way of a single PLA. Did Hilbert this venture back into metaphysics and Platonism – as Vienna Circle members believed? My paper argues that this is not necessarily the case. Hilbert's understanding of the principle of least action might also be seen as a return to the objective formal teleology that Kant illustrated at the example of Euclidean geometry. This sounds counterintuitive at first, given that it was precisely Hilbert's axiomatic foundation of geometry that ultimately denied geometry the status of synthetic a priori, which Kant had so prominently endowed it with. But we should remember that, for Kant, teleology was only a regulative principle and that Hilbert considered geometrization as a principle in mathematical physics. Even though geometry was one of the primary objects of his foundational program, it never was part of it.

[185] Resisting the Mechanization of Nature

Rediehs, L.J. (*St. Lawrence University*)

In 1668, the Quaker Isaac Penington wrote to the Royal Society of London, "Some Things Relating to Religion proposed to the consideration of the Royal Society, So Termed." On the surface, this document seems to be exactly as its title describes: a treatise written by a controversial religious radical chastising scientists for drifting away from true religion. But read carefully and interpreted from within an understanding of the context of the time we can see this document as a template for an alternative philosophy of science.

Penington and other early Quakers such as George Fox, William Penn, George Keith, and Robert Barclay were somewhat aware of the philosophical discussions of their time, and appreciative of the emergence of what we now call modern science. They described their own religious epistemology as "experimental," and thus saw continuities between their religious views and their understanding of the natural world. Through Anne Conway, some of the early Quakers were in communication with Henry More and Francis Mercury van Helmont, both of whom in turn were somewhat influenced by Quaker thought. Van Helmont carried that influence into his conversations with Gottfried Leibniz, and perhaps with John Locke as well.

Thus, we can find resonances among aspects of Quaker thought, Cambridge Platonism, and van Helmont's and Leibniz's philosophies suggesting an alternative philosophy of science that assumed a vitalist understanding of the natural world, rooted in an expanded empiricist epistemology. These thinkers were united in their opposition to the mechanization of nature. They saw eliminating final causes from science and narrowing efficient causation to mechanical explanation as harmful and distorting oversimplifications, a view also shared even a little later into the modern period by George Berkeley. In addition to mechanical forces, they believed that other kinds of forces ("virtues and powers") were also inherent in nature. They further believed that humans' sensory powers do not just include the external senses, but also an additional kind of internal sense that could be cultivated to perceive these other internal forces, giving people the ability to acquire a deeper knowledge of nature than merely its surface appearances and mechanical interactions.

While some of the early Quakers became suspicious of too much theorizing and were therefore reluctant to establish consensus on their metaphysical views, making it difficult to generalize a "Quaker" philosophy of science, this same reluctance also kept open the possibility of a different vision of science where mainstream intellectual thought closed down around a materialist-mechanistic conception. While modern science did reject final causes, limited its empiricism to the external senses, and became naturalized, traces of a more open attitude maintained a subtle parallel existence though the presence of Quaker scientists such as Arthur Eddington, as can be seen in his 1929 Swarthmore Lecture, "Science and the Unseen World."

In this paper, I aim not only to sketch this undertold history, but also to explicate its implicit alternative philosophy of science and consider whether some version of it might be relevant to our world today.

[187] Kant's Reciprocal Causality and the Problem of Holobionts

Wilks, A.F. (*Acadia University*)

This paper considers the possible application of Kant's notion of *reciprocal causality* to our understanding of a perplexing type of living system that continues to confound current biologists − holobionts. Kant identifies reciprocal causality as the fundamental feature of living systems (Teufel, 2011; Toepfer 2012). This type of causality is attributed to things when they may be conceived *as if* they were *natural ends*. Kant maintains that "a thing exists as a natural end if it is cause and effect of itself (although in a two-fold sense)" (Kant 5: 370). This two-fold sense is to be understood as follows: "first, that its parts . . . are possible only through their relation to the whole," and "second, that its parts be combined into a whole by being reciprocally the cause and effect of their form" (Kant 5: 373). I argue that Kant's notion of reciprocal causality exhibits a close affinity with a particular feature of a very current strain of thought. Specifically, I propose that a variation of Kant's formal principle is exemplified in Doolittle and Booth's treatment of the contested view in current biology, that holobionts (living systems composed of various species) may be viewed as evolutionary individuals, and perhaps even as units of natural selection (Doolittle and Booth, 2016).

Taking Kant's notion of *reciprocal causality* as the basis for the functionality and identity of living systems, what may we conclude about the functional relations in holobionts? I argue that the extent to which a holobiont may be said to have a function as a collective (collective functionality) is determined by the extent to which *reciprocal causality* is manifested between the host and its symbionts. That is, to what extent do the *parts* (microbial communities) of the holobiont "cause," i.e., sustain and preserve, the whole (macrobe host), and *vice versa*. Although Kant views the organism as the paradigmatic living system that manifests reciprocal causality, his account, I think, is equally applicable to other living systems, including holobionts. What I see operative in Kant's notion of reciprocal causality is a functional relationship that may be replicated in different material forms, while still preserving its identity (Moreno and Mossio, 2007; Clarke, 2011). I maintain that Kant's position has a notable affinity with Doolittle's and Booth's claim that what it is that gets replicated in recurrences of holobionts is not the entire group of organisms *as a group*, but rather abstract functional relationships, i.e., the relevant interaction patterns (Doolittle and Booth, 2016).

I conclude from these considerations, that although we may mean different things when we ascribe a function to some living system or some part of it, depending on the kind of living system we are referring to, and the context in which we are operating (Godfrey-Smith, 1993), there is one kind of function attribution exhibited by any living system, including highly complex, ecological systems such as holobionts. This type of function, I argue, is best understood in terms of Kantian reciprocal causality.

[188] Ambiguity and Universality: Cardano's Philosophy of the Soul

Regier, J. (*Ghent University*)

There is an important tension in Girolamo Cardano's philosophy of the soul. On the one hand, individual souls of great variety are essential to the functioning of the cosmos. On the other hand, Cardano frequently seems to reduce all souls to celestial heat, flirting with a sort of pantheism. His celestial heat is itself exceptional. Not only is it responsible for spontaneous generation (a commonplace in the sixteenth century), it also seems to wield a perceptive ability and tendency to self-protection (De subtilitate), making it a forerunner of Bernardino Telesio's heat and cold. Moreover, Cardano's celestial heat is more or less indistinguishable from elemental heat: it is what we feel as hot. The soul, then, is localized and ubiquitous, quasi-elemental and immaterial. I will argue that these ambiguities point to an unsteady overlapping of several causalities in Cardano's work: from the astrological causalities of Averroes and Marsilio Ficino, to medical theories of disease as formal or total corruption, found in Girolamo Fracastoro and Jean Fernel. I will conclude that Cardano sacrifices consistency in order to maximize a vision of nature as causally fluid, lacking fixed borders, and of the living body as the preeminent site of universal forces.

[191] *Philosophy of Science*, the Journal: A Full-Text Topic Modeling Analysis 1934–2014

Malaterre, C. (*UQAM*), Chartier, J. F. (*UQAM*) and Pulizzotto, D. (*UQAM*)

Algorithms and methodologies of the digital humanities make it possible to analyze the semantic content of very large corpora of full-text documents (Aggarwal and Zhai 2012; Srivastava and Sahami 2009; Griffiths, Steyvers, and Tenenbaum 2007). Such text-mining tools have started to bear interesting results in the humanities, including history and sociology (DiMaggio, Nag, and Blei 2013; Mimno 2012), as well as linguistics, philosophy and cognitive sciences (Turney and Pantel 2010; Widdows 2004). In this communication, we show how such tools can be used to study the history of philosophy of science. The corpus that is the object of our analyses consists of all the articles published in the journal *Philosophy of Science* from its start in 1934 up to 2014 (a total of 4602 full-text articles retrieved in different forms from the JSTOR platform). The computational method we used is based on the construction of a semantic vector space which is a mathematical model of the meaning of the words present in the corpus. In such a model, words are represented by vectors and the semantic structures that organize these words are represented by different geometric, algebraic and topological structures that can be studied by means of algorithms. The analyses we conducted, after a series of standard pretreatment steps, included a full-corpus topic modeling over the whole period of publication as well as a dynamic topic modeling by 5-year increments. The modeling resulted in the identification of some 200 topics among which could be found many of the classical topics of philosophy of science, including topics about realism, explanation, induction, confirmation, but also a number of topics that are much more marginal, especially when compared to standard text-books. The dynamic analysis made it possible to identify different evolutionary patterns among topics, including bell-curve and oscillatory types of patterns, that reveal different dynamics among types of topics. By focusing on a couple paradigmatic examples, we aim to show how such patterns can be connected to well-known trends in the history of philosophy of science as well as how these text-mining results can be used as heuristics for further historical research.

[192] Digital humanities in studying German Idealism: Social network analysis, text mining, and author recognition

Van Miert, D.K.W. (*Utrecht University*)

In this presentation, I will briefly discuss three DH-methods which I have used in the recently completed project NWO-project "Thinking Classified: Structuring the World of Ideas aroud

1800". The three methods are social network analysis on the basis of metadata (Mapping German Idealist Correspondence NETworks: MaGIC NET), semantic field analysis on a corpus of complete works of Kant, Fichte and Schelling, and authorship recognition (disentangling authorship on paragraph level in the Kritisches Journal: Hegel or Schelling?). I will address the possibilities and limitations of DH-methods, in terms of organisation, legal constraints, and technical challenges. It turns out that methodological issues and technological challenges are perhaps less problematic than external problems on the level of finance, organisation and rights. When it comes to financing, it is essential to invest time in preparing datasets by cleaning them up, whereas funding bodies are not inclined to support such rather low-level mechanical work and instead require the development of 'innovative' analytical tools. When it comes to organisation, there is increasing anxiety over the responsibilities of sustaining the interoperability of datasets once projects are finished. When it comes to rights, scholars in the humanities have still limited experience with acknowledging different levels of authorship in case research output is based on the analysis of big data aggregated from multiple repositories that were assembled by third parties.

[193] Eleatic Occasionalism: Descartes, Geulincx, and Langenhert on Causation and the Infinite Force of Resistance of Body

Jaworzyn, M. (*KU Leuven*)

Scholars interested in Geulincx's occasionalism to date have tended to focus on the so-called epistemic condition for causation in Geulincx, the Quod Nescis principle. However, I suggest that such a focus has been at the expense of a crucial component of Geulincx's occasionalism, viz. the ascription of an infinite force of resistance to body, which precludes anything finite from bringing about motion. While a few scholars have in passing drawn attention to this line of argument (e.g. Sangiacomo 2014, Jordan 2015), I argue that it forms a key part of Geulincx's commitment to occasionalism, and that it arises legitimately from Geulingian and Cartesian principles. Some early evidence importance of this argument to Geulincx's project can be found in the space devoted to explicating and endorsing it in Langenhert's sometimes critical commentary on Geulincx's physics.

Pace Schmaltz (2016), then, I suggest that this line of argument means that Geulincx has a further reason to argue against the causal power of bodies, and one deeply rooted in features of Geulincx's metaphysics and physics; against Jordan (2015) I argue that one should not emphasise the parallels with a seemingly similar argument in Malebranche based on a commitment to an infinity of volitions in any bodily motion, because the foundations that lead to Geulincx's argument depend on his conceptions of body, motion and not the nature of mind.

Instead, I suggest that the argument is in fact based on considerations that bear comparison with those adduced by Lennon (2008)'s 'Eleatic' reading of Descartes. Lennon mentions two kinds 'problems of motion', physical and metaphysical, which lead him to suggest that for Descartes motion should not be considered real and the individuation of physical bodies is merely phenomenal. In the paper I point out that these problems of motion apply with a good deal more direct textual evidence to Geulincx than to Descartes himself. Rather than proving the impossibility of motion, however, for Geulincx they prove the necessity of an ineffable creator of infinite power bringing about motion.

Lennon does indeed mention that one way out of what he terms the physical problem is occasionalism; Geulincx embraces this. With respect to Lennon's metaphysical problem, Geulincx's solution is more complicated: compared to Descartes, Geulincx has a different account of the modal distinction and the act of abstraction, which enable to Geulincx, I suggest, to maintain that individual bodies are abstractions from the single, universal body without denying either the reality of motion or the force of the 'Eleatic' arguments against it.

[195] The Method of Hypothesis in the 19th Century: Whewell, Mill, Herschel, Jevons, and Peirce on the *Consilience* Criterion

Coko, K. (*Rotman Institute of Philosophy, Western University*)

The most important characteristic of 19th century philosophical discussions on scientific methodology, was the dynamic re-emergence of the hypothetico-deductive method (or the *Method of Hypothesis*, as it was called at the time). 19th century philosophers, especially those who were sensitive to the complexities of scientific practice, as demonstrated also by the study of the history of science, realized that traditional scientific methodology, which regarded scientific inferences as inductive generalizations from empirical facts, could not accommodate the new scientific developments, especially those related to the study of unobservable entities (the latter in the sense of entities that were difficult or impossible to directly observe, as opposed to things which were simply yet to be observed) (Laudan 1981). Amidst all the criteria for evaluating theoretical hypotheses about unobservable entities, the ability of a hypothesis to explain, successfully predict, and/or be supported by a variety of classes of empirical facts – especially facts that played no role in the original formulation of the hypothesis – began to be considered as the highest criterion which indicated the hypothesis' truthfulness. Support from different classes of facts was thought to give rise to a *no coincidence argument*; namely, wouldn't it be a remarkable coincidence if a hypothesis (usually about unobservables) can accommodate such a variety of (usually observable) facts, and yet to be false? This criterion of truthfulness is found more explicitly in William Whewell's notion of the *Consilience of Inductions*, but it can also be encountered in the writings of other 19th century philosophers like John Herschel, William Stanley Jevons, Charles Sanders Peirce, and even in the writings of *the* 19th century philosopher of Induction, John Stuart Mill (Whewell 1840, 1860; Mill 1843; Herschel 1830; Jevons 1874; Peirce 1878, c.1905).

In this presentation, my aim is twofold. First, I will look at the Method of Hypothesis in the thought of these 19th century philosophers: Whewell, Mill, Herschel, Jevons, and Peirce. I will focus especially on the reasons they give for the epistemic force attributed to the *Consilience* criterion; namely, *why the ability of a hypothesis to explain different classes of facts should be considered* (*or should not*, in the case of Mill) *as a criterion for its truth?* Second, I will use the (surprising) conclusions, to elucidate more recent philosophical discussions on scientific methodology, regarding the differences in structure and epistemic import between methodological strategies such as *Robustness* (understood as invariance of an experimental result to variations within the same experimental procedure), *Multiple Determination* or *Triangulation* (understood as the use of multiple, independent experimental procedures to establish the same local result), and *Variety of Evidence* (understood as the offering of multiple lines of evidence in favor of a general theoretical hypothesis).

[202] *Per opaca corporis ad Animæ penetralia*:The role of optics in Descartes' metaphysics

Mantovani, M. (*Humboldt University of Berlin*)

After having demonstrated the formation of an inverted picture on the rear of the eye, the founder of modern optics confessed he had no clues as how this luminous image could be transmitted beyond the retina, through the optic nerves, to the brain. By Kepler's admission, what was left to explain was of great significance, as the ultimate stage of the perceptual process. Early Modern anatomists had in fact discovered that the optic nerves were not hollow and concluded that light could not therefore creep through them by "glowingly travelling though the path of the spirits" as Ancient and Medieval theorists had been happy to assume. Early Modern thinkers were thus faced with the problem of accounting for color experience without counting on a continuous transmission of light and color from the object to the seat of perception in the brain.

Such a transmission, according to virtually all previous writers in optics, was a necessary stage of the perceptual process. They thought that in the case of color perception the Aristotelian assimilation model they subscribed to demanded the coloring not only of the external organ but even of the brain, inasmuch as this was regarded as the seat of the sensory soul. For the perceiver to "assimilate" the object's color, the Perspectivists argued that both the eye and the brain must become similar to the object in a literal sense, by turning red when faced with something red. The Perspectivists, accordingly, devised the visual system in a way as to ensure that these conditions were met: the arrangement of ocular humors they came up with was indeed more of a purely theoretical construct in service of their epistemology than a physiological reality established empirically.

When Early Modern anatomists – starting from Vesalius – started pointing out these shortcomings, a few philosophers promptly realized that the ideas of any actual coloring beyond the eyes level had to be abandoned. A few ways out of the predicament were proposed. Descartes, however, became convinced that the problem of a transmission of light and color "through the opacities of the body up to the inner cell of the soul" (as Kepler graphically phrased the conundrum) could not be eluded by simply *assuming* that the perceived and the physical red were one in kind, as suggested by late Scholastics such as Antonio Rubio. I will show that, in Descartes' view, the difficulty Early Modern theory of vision had stumbled upon, albeit apparently marginal and confined to this discipline alone, was in fact calling into question the face-value reliability of sense-perception and the metaphysical theory of bodies. I will argue that Descartes' argument that bodies are *nothing but* extended substances, and lack therefore the color-qualities of the sort advocated by Aristotelians, is in fact ultimately grounded on his empirical studies on the physiology of the perceptual (and, more specifically, visual) process. More than being concerned with the proper functioning of the eyes, Descartes' physiological optics ushered indeed in a fresh, radically diverse image of the world.

[203] Microrevolutions in Thomas Kuhn's Structure: How much revision [do] they require?

Moural, J. (*Charles University/Czech Academy of Sciences*)

In his *The Structure of Scientific Revolutions*, Thomas Kuhn repeatedly points out that what he calls scientific revolutions occur not only on the most general level of entire scientific disciplines, but on all lower levels of subdisciplines and sub-subdisciplines down to microspecializations (STS 6-7, 49, 52, 92). He even says that it is "a fundamental thesis of [his] essay" (STS 6). However, he keeps introducing only cases from the higher levels of generality and, what is worse, he never discusses two or more parallel processes on different hierarchical levels.

This is worrying, for very much of his exposition is based on a sharp opposition between normal science and revolutionary science. But as soon as we look at multiple layers of science at once, we need to decide how to deal with such cases that are genuinely revolutionary on a micro-scale but quite likely appear to be simply a success in normal-scientific puzzle-solving from a few layers above. Kuhn did not show us how to treat such situations.

So far, the problem has not been satisfactorily discussed. The basic options are:

(1) does a revolutionary step on a micro-level make the entire discipline revolutionary for the moment? (most likely not, for there would be very little normal science going on if we accept this option);

(2) should we neglect microrevolutions and insist that, within a normal science epoch, no genuine revolutions can occur? (I suspect this is the option adopted by the mainstream interpretation, but it runs against the allegedly "fundamental thesis" of Kuhn);

(3) or, finally, should we bravely do what Kuhn left unsolved, i.e. to enrich his theory so that it would accomodate revolutions on micro-level taking place in the context of what appears to be normal science several levels above?

I suggest Kuhn requires us to adopt (3). This requires, on the one hand, to develop a theory enrichment that would allow us to discuss how what is unexpected and non-cumulative for a small community of specialists somehow loses much or all of the unexpectedness and non-cumulativity when observed or only noticed by people from other areas of the discipline (some less and some more distant). And, on the other hand, it requires us to consider how much of Kuhn's theory as it stands will need to be revised. As far as I can see, we shall end up with a much more complex and nuanced picture, and I am afraid that we have to rephrase and sometimes weaken or relativize many of Kuhn's most striking characterizations of normal science.

[205] Cavendish on Why All of Nature's Parts Are Animate

Meyns, C. (Utrech University)

Margaret Cavendish holds that each part of nature must be animate, or have sense and reason. Cavendish accepts this point, because she deems it necessary for the explanation of how there can be order in the natural world. In this paper I focus on how exactly to understand Cavendish's explanation, which amounts to a form of panpsychism. So far several different ways to interpret Cavendish's main thesis have not been systematically distinguished. I distinguish three ways of conceiving of the panpsychist order Cavendish finds in nature: (1) centrally necessitated order; (2) centrally guided order; or (3) a distributed order account. I argue that a distributed order account, on which parts of nature coordinate their motions, best captures Cavendish's work. It avoids some difficulties facing its competitors, while properly capturing how nature divides into parts with agency, sense and perception. Hence, I conclude that a distributed model best captures how, according to Cavendish, panpsychism forms a basis for order in the world.

[208] The Activity of Cartesian Matter

Nelson, A. J. (*UNC–Chapel Hill*)

This presentation challenges the commonplace that Cartesian matter is fundamentally passive and inert. The textual considerations in favor of reading Descartes himself in this way are reviewed along with reasons for finding these texts either fully compatible with a strong sense in which matter is active or else inconclusive. This sense of activity strongly anticipates Spinoza's treatment of the divine attribute of extension without going so far as to make matter self-moving (as in Cavendish) or as arising from immaterial substances (as in Leibniz).

Among the apparently pro-passive matter texts considered are: a) The essence of matter is extension, i.e. the object of mathematics while created matter is the object of physics; b) geometrical objects have true and immutable natures whether or not they are considered as existing; c) the existence of matter requires proof; and d) God creates matter and "at the same time" establishes a quantity of motion.

These texts are re-assessed in light of the following: a) the status of matter as possibly existing rather than actually existing is an artifact of Descartes's method of doubt; b) a conceptualist interpretation of true and immutable natures; c) an analysis of essence as the "principal attribute" of a substance along with an interpretation of Descartes on attributes; d) an interpretation of quantity of motion as an attribute of the entire extended universe.

[211] Good Vibrations: Mechanical Optics at a Distance

Lawson, I. (*Humboldt University of Berlin*)

Robert Hooke marvelled at God's ingenious design for eyesight: "How could it have entred into the Imagination of Man to conceive, how it should be possible for such an Atom of the Universe as Man is, to be informed at the Instant that a thing is done, how and where it is done, though Million of Millions of Miles distant?" What was so marvellous to him was that despite the vast distances and exceedingly speedy transmission of information, vision nevertheless operated via mechanical means. In fact, despite being a clear case of influence over great distances, natural philosophers have never seriously considered vision a case of action at a distance. However, the contention of this paper is that in the seventeenth century it did become a particularly illuminating case of mechanical explanations of distant causation.

Following Kepler, optics stopped being the study of the propagation of visual rays or immaterial *species* and became about the motion of light. Keeping step with this change was a mathematical tradition that described optics in geometric, rather than physical, terms. Several philosophers, and Hooke is a prime example, talked about light as a sort of halfway case between these mechanical and mathematical traditions, on the borderline between the immaterial and material. Hooke went as far as to suggest that it, alongside gravity and magnetism, was as close to being an *anima mundi* as likely existed.

Drawing on recent scholarship in this area, this paper explores the peculiarities of certain broadly mechanical characterisations of light, given the relationship between the natural philosophical and mathematical optical traditions. It will explore various ontologies and mechanisms that were used to explain vision in early modern Europe, from vibrating media to emitted particles and projected rays, showing that the continuance of a mathematical tradition that ignored problems of obscure or occulted causes helps us to better understand the epistemic and ontic commitments of the shift from hylomorphic natural philosophy to mechanical.

[213] Physics and Simple Machines: Descartes and Roberval

Babes, O. (*University of Bucharest*)

This presentation concerns Descartes's mechanics, *i.e.*, the science of simple machines. I argue that his mechanics is in a negative way informed by his metaphysics, and I will illustrate it by a dispute that took place between Descartes and Roberval regarding oscillating bodies. The way in which Descartes's metaphysics informs his mechanics is that it restricts it from considering physical causes. As such, mechanical concepts, *e.g.* the center of gravity and the center of oscillation, only deal with relative heaviness and do not overlap within a single body.

The fact that Descartes's mechanics does not deal with physical causes lead to the idea that this intellectual pursuit was unrelated to his natural philosophy (Gabbey, 1993). The source of this division lies in Descartes's physics. Descartes metaphysically establishes the laws of motion (*Principles* II 37-40), from which rules of collision are deduced (*Principles* II 45-52). Yet many diverse (and quite contrary) effects can be inferred from them. Descartes's rules of collision could hardly account for our experiences, they cannot do much explanatory work. Descartes uses hypotheses—configurations of particles in motion—to bridge this explanatory gap, even if these hypotheses need not be, strictly speaking, certain. Hypotheses should explain real phenomena, while also being proven (and refined) by these phenomena. The heuristics of the hypotheses may depend on a standard of mathematical intelligibility (Domski 2009, 2017). Relevant experiments are necessary, however, for their observable confirmation (Garber, 2000).

Mechanics deals with observable experiences. It is based on one principle, "An effect must be equal to the action that produces it" (AT I 436). This principle resembles a causal containment axiom (Schmaltz, 2008), it is not a principle of physics. Thus, we have a science of mechanics that is about real effects which have hitherto unknown causes.

The non-physical status of mechanics is manifest in the debate between Descartes and Roberval about the center of oscillation of pendulums. Their geometrical accounts of establishing this center differ. One central disagreement was about relating the center of oscillation to the axis connecting the suspended body to the center of the Earth. Roberval claimed that the right geometrical account has to include the direction of movement of each part of the pendulum in relation to this axis. The reason is that the body's center of gravity would also contribute to the movement of the pendulum. The centers would act on one another. Descartes disagreed: The two centers are about relative, not absolute weight of a body, and do not constitute distinct forces that act at the same time. He denied the need to include the direction of movement of each part of the pendulum. Even if he accepted that multiple movements can coexist within a body (*Principles* II 31), we could not conceive them as distinct, and mechanics should not treat them as separate. Mechanics must leave room for more possible (and maybe incompatible) physical explanations. This way, it can have a corroborative role in Descartes's physics.

[214] Infinite probability: Brentano's justification of physics

Ierna, C. (*University of Groningen*)

Brentano claims that all sciences are based on a shared method and that "the foundations of psychology as well as of the natural sciences are perception and experience" (1874, 35). Sensations form the starting point for natural science, which deals with physical phenomena and "establishes laws in so far as they depend on the physical stimulation of the sense organs" (1874, 127). However, causes, understood as forces that generate sensations in us, are merely an "ascription", since such external forces in nature are quite "unpresentable" (1874, 161). Hence, on this view, we could never be completely certain of external experience: "As little as I am inclined to doubt the existence of the outer world, yet we have no certainty about it." (Q 10, 17-10). Then what kind of foundation can an empiricist like Brentano provide for physics? Besides the inductive sciences, we also have deductive sciences. For Brentano mathematics would be analytical, deductive, and a priori. Moreover, according to Brentano mathematics is foundational for all other sciences in various respects, such as being logically and chronologically prior to physics, being required by physics for measurements, and by providing the deductive foundation for the inductive reasoning in physics. I will concentrate on the latter point. Brentano's claim is quite unequivocal: "Mathematics is not an inductive, but a purely deductive, and in this sense, a priori science. Indeed, were it not, then there would be no science at all, neither deductive nor inductive. Because it is not induction that sanctions deduction, but deduction, and specifically mathematical deduction, that sanctions all rational scientific justified induction." (Megethology 40025 f.). What Brentano means here, is that induction yields merely probable knowledge, not knowledge that is absolutely certain. Brentano distinguishes between the mathematical calculus of probability and the mere subjective feeling of likelihood, accusing Hume of having confused the two, leading to his skeptical conclusions. According to Brentano, however, the mathematical calculus of probability is capable of yielding knowledge. Something is knowable, if we can judge with “certainty” about its existence, but “certainty” can be had in various forms, absolute (mathematical) certainty, probable (moral) certainty, and physical certainty. The latter is defined as being infinitely probable, and it is the kind of certainty that we can have about the laws of nature and the external world as well as about god and other minds.

[215] Science as a Practice of Enrichment: Dewey's Philosophy of Science
Mostajir, P.C. (*University of Chicago*)

In 1934, John Dewey wrote, “Such is the newness of scientific statement and its present prestige (due ultimately to its directive efficacy) that [it] is often thought to possess more than a signboard function and to disclose or be ‘expressive’ of the inner nature of things” (*Art as Experience*). Science is not as ‘new’ as it was in 1934, but a belief persists in its singular capacity to reveal the essential nature of the world. The pragmatists of the late-19th and early-20th centuries, however, present a radically different perspective. Rather than being revelatory of reality, all human practices--science included--are thought to exist for the sake of resolving experienced problems as they arise, and enriching the quality of experience.

For Dewey, reality is not a static entity to be revealed by being probed and perceived in a scientific process, but is constituted in the very process of experience as an “inclusive integrity”. Dewey’s concept of experience dissolves the division between the world of objects experienced, and the acts of experiencing them. The familiar divisions of mind-matter, subject-object, real-illusory, are *a posteriori* elaborations achieved within the “immediate flux” of experience, intimating Hegel’s influence on Dewey’s philosophy.

Dewey reminds us that “if experienced things are valid evidence”, then we must accept the ontological reality of features we find therein. We cannot, like many empiricists, take ‘experience’ as evidence of what exists in nature, yet conveniently narrow our conception to exclude everything that cannot be exhaustively defined in terms of qualities relevant to the five senses and logic. Aesthetic and moral experiences have “metaphysical import” as well as the properties discovered and manipulated by science; “Empirically, things are poignant, tragic, beautiful, humorous, settled, disturbed, comfortable, annoying, barren, harsh, consoling, splendid, fearful” (Dewey, *Experience and Nature*).

Science, within this framework, is transformed from a practice which possesses a unique capacity to reveal the hidden nature of reality into one activity, among many, undertaken for the sake of enrichment of lived experience. Science is nothing but a “signboard”--a set of directions on how to produce, alter, or destroy the objects of its theories, according to human need. It is therefore not the most essential, fundamental, or real aspects of a static, independent world, but the most stable, universal, simple properties of the objects of our fluctuating experience, which are harnessed in science and technology as instrumentally valuable for collaboratively and reliably producing desired effects in our lived experience. Crucially, the qualities with which science deals are no more constitutive of reality than the less stable, inchoate, and complex aesthetic, moral, or emotional properties dealt with by the arts, humanities, and social sciences.

Dewey’s philosophy of science, elaborated almost a century before the infamous Science Wars, pragmatically straddles the divide between scientific realism and postmodern calls to subsume science under a humanistic theoretical framework. His perspective necessitates a profound respect for the richness and complexity of human values and practices, while affording science a highly respected (but not monopolistic) position in the advancement of human goals.

[218] Émilie Du Châtelet's Contribution to the Metaphysics of Forces: How to Ground Newton's Laws of Motion in Leibnizian Forces

Solomon, A. M. (*University of Southern California*)

This paper focuses on one crucial tension in the epistemological order of Du Châtelet's project in *Institutions de Physique* (1740) and follows its consequences in her commentary to Newton's *Principia*. The main argument here is that the real and apparent (or virtual) states of motion and rest that are supposed to ground an ontology of forces, already presuppose the framework of living and dead forces.

The *Institutions* have a clear Leibnizian influence: the ontology of dead and live forces (and their sub-taxonomies) is developed explicitly from Leibniz's works (for instance, Du Châtelet draws from Leibniz's 1686 letter in *Acta Eruditorum*). At the same time, Du Châtelet presents Newton's three laws of motion without using the pair of *vis insita-vis impressa* and, among other things, recovers Galileo's results about the phenomena of gravity such as the uniformly accelerated motion and the parabolic path of the motion of projectiles. In this paper I follow Du Châtelet's challenging project of reformulating Newton's laws of motion on the framework of Leibnizian forces. I then delineate the consequences of cashing out the action of gravity in terms of living forces (and her original derivation of the quantity of living force of a free falling body as mass times the square of the speeds). Finally, I show how her arguments rely on the implicit metaphysical distinction between real (actual) versus apparent and virtual motion and rest (along with the accompanying strict distinction between rest and motion). This distinction however turns out to also depend on the ontology of living forces.

In the first section I compare Du Châtelet's formulations of the laws of motions (Chapter 11) with Newton's statements of the same law. The second section uses the previous analysis of Du Châtelet's two laws of motion in order to answer the following question: how does she account for the action of gravity? Du Châtelet considers that "gravity acts equally on bodies at each instant, whether they be at rest or in motion". Therefore, if we ask what the effect of gravity is, the answer depends on whether the body is truly at rest or in motion. In particular, when the body is in motion, Du Châtelet derives the quantity of *vis viva* by an original analogy between gravity and an infinite spring (§566-§568).

In the third section, I show that the previous arguments invoke two distinctions: there are real (actual) rest and motion and (1) they are distinct from each other but also (2) distinct from apparent and virtual motion or rest.

[220] John Dewey on Values in Science: Four Theses

Brown, M. J. (*University of Texas at Dallas*)

John Dewey is an interesting figure in the history of philosophy of science for many reasons. Among them, he is generally taken as a key early defender of the view that science is value-laden. But as one examines his writings on science, the specific role of values in science is far from straightforward. I will demonstrate that there are four ways in which Dewey discusses the role of values in science, two that are relatively familiar in current discussions of values in science, and two that are relatively novel and more radical than most contemporary philosophers of science hold.

First, two relatively familiar claims:

1. We must have democratic input into science, if not democratic control, directing science towards social goods.

2. Science has social and cultural consequences that must be considered in the course of scientific inquiry.

The first is much the same idea defended by Kitcher in *Science, Truth, and Democracy*, that the research agenda of science in a democratic society ought to be geared toward democratically-determined common goods. As a result of his particular historical context, Dewey tends to take the idea in a more explicitly anti-capitalist, anti-militaristic direction than Kitcher, but the core claim is shared. The second thesis is related to the arguments for the value-ladenness of science from inductive risk and underdetermination, though Dewey's arguments in this vein tend to be brief and non-technical.

Dewey also espouses two more radical ideas:

3. Advances in scientific inquiry, both method and content, can drive changes to our social and ethical values.

4. Scientific inquiry is the same in kind as practical reasoning (what Dewey calls "judgments of practice"), and thus the role of values in both kinds of inquiry will be similar.

These two ideas together imply a strong sense of unity between basic and applied science as well as pure and practical reason. The former allies Dewey with certain feminist pragmatists, like Elizabeth Anderson and Sharyn Clough. The final claim is a distinctive pragmatist contribution to the discussion which has received relatively little attention. Unpacking the import of this fourth thesis will be my focus.

Of particular interest is the relation of Dewey's version of the pragmatist theory of truth to the discussion of values in science. Dewey describes the truth conditions of a judgment with success or satisfaction in fulfilling what the judgment "intends." This is clear enough in the case of judgments of practice, which affirm particular courses of action. The truth of a judgment, "I should engage in course of action A rather than B," depends (at least in part) on whether A successfully met my goals and didn't result in any further problems. In this case, values, in the sense of my goals as well as side constraints, act as conditions on the truth of my practical judgment. Because scientific judgments are the same in kind as judgments of practice, they work the same way. That is, the truth conditions of scientific judgments are forward-looking and value-laden.

[221] Complementarities Beyond Bohr's

Gomatam, R.V. (*Bhaktivedanta Institute, Berkeley*)

Historically, Bohr's notion of complementarity has played a crucial role in the philosophy of quantum mechanics. Complementarity vaguely justified the contradictory use of classical concepts to pragmatically apply the quantum formalism in the lived world. The exact physical origin of his notion has remained elusive. Generalities abound. Even Bohr indulged in them: "Phenomena are complementary in the sense that taken together they exhaust all information about the atomic object which can be expressed in common language without ambiguity."

But Bohr did have a specific physics-based idea of 'inseparability' on which he based complementarity: "The main point here is the distinction between the *objects* under investigation and the *measurement instruments* which serve to define, in classical terms, the conditions under which the phenomena appear . . . these bodies **together** with the particles would in such a case **constitute the system to which the quantum mechanical formalism is to be applied**." (Bohr [1949], 1970, emphasis mine)

Thus, in the standard two-slit experiment, if there are two physically different experimental arrangements — such as placing the detectors close to the two slits and far away — they form, along with the particle involved, two *different* and *mutually exclusive* or complementary composite 'quantum systems. All other complementarities derive from this. For example, if two different wave and particle visualizations are to be applied to the observed behavior of these two quantum systems, then they are also complementary visualizations. I shall discuss the full range of Bohr's interpretation from this starting point.

Bohr devised his complementarity to remain within classical kinematical conceptions and avoid overt contradiction. But it also prevented quantum realism. To undo this epistemic inseparability, we would need a range of notions about objects and causality in everyday thinking itself. This is precisely the possibility that Bohr (1934) denied: "In this connection we must remember, above all, that, as a matter of course, all new experience makes its appearance within the frame of our customary points of view and forms of perception." As a result, his notion of complementarity has remained a limiting interpretive idea.

I shall motivate the possibilities for invoking a different quantum-compatible range of notions that do exist in commonsense thinking to interpret the quantum observations. These notions will undo the 'inseparability' imposed upon us by classical concepts, and open the way for a realistic interpretation of quantum mechanics. Implicit here is the idea that there are two complementarity ranges of ordinary language thinking — the classical and quantum — and that both can be applicable to the quantum formalism. Many further complementarities follow, such as the complementarity between microscopic and macroscopic quantum mechanics, classical and quantum space-time pictures and so on. We will explore this further world of complementarities that lie beyond Bohr's complementarity.

[225] Leibniz on the Instincts of Machines of Nature and Souls

Noble, C. P. (*Villanova University*)

Over the last roughly fifteen years, scholars interested in Leibniz's approach to the life sciences have shed light on Leibniz's conception of living bodies as machines of nature. For Leibniz, living bodies are infinitely complex mechanisms whose organic structure is preformed by God. In this regard, there is no genuine generation or corruption within the course of nature; events such as bodily conception and death are moments within the larger development of a pre-given and infinitely complex mechanical structure. Leibniz, for the machine to truly be infinite in structure, each part must be a further machine of nature, in turn, such that there are infinitely many smaller machines of nature nested within the initial mechanical structure. Not only does Leibniz's concept of the machine of nature represent an empirically informed model of the living body consistent with contemporary microscopical findings, it represents a mechanical account of life that is amenable to traditional natural theology insofar as it essentially incorporates divine design.

Commentators treating the machine of nature in Leibniz have not, however, sufficiently treated its correspondence with the immaterial soul. Overlooking this correspondence risks neglecting a crucial dimension of Leibniz's account of the living being insofar as in Leibniz's metaphysics, body and soul operate in parallel to each other according to a harmony preestablished by God. On this account, insofar as the body is an infinitely complex machine of nature whose movements are subject to divine preformation, the soul's perceptions unfold alongside these bodily motions in a way subject to divine preformation in turn. Further, since the movements of the bodily machine of nature are in harmony with the perceptions of the soul, Leibniz argues that the soul corresponding to a particular machine of nature represents everything taking place within the machine's organic structure. Thus, the soul confusedly represents an infinite number of bodily motions.

In this presentation, I argue that Leibniz develops a concept of instinct to connect soul and body and explain how the soul represents the infinite structure of the body. In short, I argue that both body and soul carry out their divinely preformed operations by virtue of an instinct implanted in them by God. I focus on the role of instinct in two texts: Leibniz's *Animadversions* with the medical philosopher Georg Ernst Stahl – one of the most significant expositions of Leibniz's conception of organic body and its relation to the soul – and the *Theodicy.* I show that Leibniz attributes the way that both body and soul change in harmony with one another to instinct. In the case of the body, instinct explains the infinite mechanical unfolding of organs, whereas in the soul, it explains how the perceptions of a finite soul represent the infinite number of events taking place at any given time in the body. The presentation thus contributes to ongoing discussions of Leibniz and the life sciences by drawing attention to Leibniz's concept of instinct as an integral part of the living being that underpinning the correspondence of soul and body.

[227] There must be a tub to amuse the whale: Joseph Black's Methodology Reconsidered

Creel, K. (*University of Pittsburgh*)

Joseph Black is often considered the paradigmatic Scottish Enlightenment experimentalist. In the laboratory, he was renowned for his steady hands, innovative furnaces, and clever experimental designs. The manufacturers of the early industrial revolution consulted Black on improvements to mining, ceramics, dyes, bleach, and ironworks from the earliest days of his career, and his discoveries were crucial to his friend James Watt's invention of the steam engine. Black's dual reputation as a brilliant experimenter and a lucid lecturer drew students from across Europe and America (Perrin, 1982), even as his classes became fashionable among Scotland's ruling class (Brougham, 1855). The chemical discoveries that made his reputation, of fixed air and latent heat, were accomplished by the end of his years at the University of Glasgow.

In addition to presenting his chemical findings to his students, Black also passed along his interpretation of good scientific methodology during his forty-four years of lecturing. What this methodology was, however, is difficult to ascertain. Black is often mentioned as an example of the careful 18th century experimentalist who shunned theorizing and speculation (Cantor, 1971; Chang, 2004; Lawrence, 1982). Commenters characterize him as refusing to commit to interpretations of his discoveries in terms of the scientific debates of the day, preferring to let the experiments speak for themselves.

This received view of Black's methodology was created by John Robison, who edited and revised Black's lecture notes for publication. A former student of Black's, Robison had developed his own scientific and methodological views by the time of Black's death. He also had an anti-French sentiment that opposed him to any influence of Lavoisier or the Continental chemistry. Robison took the liberty necessary to eliminate material he found distasteful and insert material where he found the existing lacking.

The recent publication of Black's letters and archival lecture notes, however, makes it possible to see just how drastically Robinson edited Black's work. In particular, handwritten copies of student notes from Black's final lectures, which Robison sometimes used to supplement Black's own notes, have survived. The differences between these notes and their analogues in Robison's edition illuminate Robinson's editorial choices.

Robison portrayed Black as a consummate but cautious experimentalist, a master of technique who would not theorize beyond the evidence or commit to any claims broader than those necessary to explain the phenomena observed. However, although he valued both experimentation and application, Black gave theory a substantial role in his scientific methodology. Using his letters and archival student notes, this paper outlines Black's positive view of theory. In his definition of chemistry, Black postulates a substantial theory of heat, one that also served him as a fundamental principle for the purposes of reduction. He believes the world to have a general structure organized by God for its proper functioning, a structure which supports using the principle of simplicity to select among theories and the principle of analogy to extend methodological findings. Finally, Black made theoretical claims which he had good reason to extrapolate before confirming said claims with experiments.

[229] The relativity of motion and the mathematical method of Newtonian physics

DiSalle, R. J. (*Western University*)

Philosophical discussions of Newton's theory of absolute space and and motion generally focus on questions that were raised after he completed the Principia, especially questions about the ontological status of space and time and related questions about the relativity of motion. Such discussions generally obscure the fact that, during the development of his dynamical theory, Newton was deeply concerned with the relativity of motion. From a modern perspective, this is usually seen as a concern that Newton took less seriously than contemporaries such as Huygens and Leibniz. But Newton pursued the problem of the relativity of motion further than his contemporary critics realized. While they defended the relativity of motion as a general principle, only Newton developed what may be called a theory of relativity: first, a systematic account of what is objective in the description of physical interactions, and a principled distinction between the objective properties and those that depend on the choice of a frame of reference; second, a critical analysis of accepted concepts, revealing the extent to which they represent relative perspectives on objective quantities. Thus Newton articulated, more clearly than his contemporaries realized, the revisions imposed by the relativity on prevailing notions of force, inertia, and causality. We can see this from his evolving understanding of the Galilean relativity principle—eventually stated as Corollary V to the laws of motion—and his evolving conception of causal interaction. In the course of this evolution he developed a new approach to the motions of the solar system as a system of interacting masses within a relative space, culminating in the theory of universal gravitation.

Moreover, as he came to a deeper understanding the peculiar nature of gravity, he grasped its peculiar bearing on the problem of the relativity of motion, which emerges in his development and use of Corollary VI. But Corollary VI was not, for Newton, a true extension of the relativity of motion. Rather, it allowed him to treat certain special states of accelerated motion—a shared acceleration of all bodies in a given relative space—as approximately equivalent, precisely because the accelerative forces acting on all could be mathematically combined with those they exert on each other. Newton claimed the right to treat such forces in a purely mathematical way, without regard to their "physical seats and causes." But his empirical success in so treating them revealed physical aspects of gravity that competing mechanical theories of gravity were unable to comprehend.

Newton's thoughts about relativity only become clear from a study of their history, especially the profound changes in his views between De Gravitatione and the Principia. Relativistic thinking became essential for separating the problem of "true motion" for the solar system from the inherently insoluble problem of how that system moves in absolute space. Indeed, the history shows that Newton introduced the theory of absolute space precisely in order to articulate his theory of relativity.

[232] Bolzano in Ones and Zeros: A quantitative study in 19th century philosophy of mathematics.

Van den Berg, H. (*University of Amsterdam*)

Researchers in history and philosophy of science tend to be little involved in digital humanities projects. This is regrettable, however, because valuable contributions are obtained by applying even rather simple, well-known computational techniques to texts relevant to the work of researchers in history and philosophy of science (van Wierst et al. 2016). In this paper we substantiate the point by relying on a quantitative, computational analysis of texts in addressing an open question in the study of an epochal turn in the history of scientific ideas. The turn in question concerns the emergence of a radically objective account of the concept of a scientific statement in terms of a mind-independent, language-independent and time-independent entity, known to present-day philosophers as a proposition (more specifically, a Fregean proposition). In philosophy, the position taking scientific statements to be propositions is known as Platonism. Mainstream scholarship in 19th century philosophy of mathematics commonly identifies the first emergence of Platonism in this sense with Bernard Bolzano's introduction of the notion of Satz an sich (proposition-in-itself) in the 1820s. Yet a minority of interpreters of Bolzano's thought deny that Bolzano was a Platonist in this sense (Cantù 2006). In this paper we endeavour to provide new quantitative evidence to help assessing the open question of Bolzano's Platonism by relying on SalVe, a text-mining software developed by our team to the specific goal of aiding philosophers in the analysis of unusually extended textual corpora.

[233] Carnap and Wittgenstein on psychological sentences: 1928–1932. Some further aspects of the priority-dispute over physicalism

Ambrus, G. (*Eotvos Lorand University*)

The origin of physicalism is a complex question, to which an unambiguous answer may not even be available as there were different formulation of the doctrine, thus *the* inventor of physicalism may not be possible to identify. Nonetheless the received view is that the main actors were Neurath and Carnap: Neurath proposed earlier (his versions of) physicalism, but it was Carnap who first published an elaborated formulation of the (metalinguistic) doctrine according to which the universal language of science ought to be the physical language. It is also known, however, that in 1932 Wittgenstein accused Carnap with plagiarism concerning physicalism (ignoring Neurath's contributions completely). There is considerable literature on the origins of physicalism as well as on the priority-debate between Wittgenstein and Carnap (e.g. by Haller, Hintikka, Manninen, Stadler; Stern, Uebel and others); my paper aims to contribute to these investigations. However, I address the topic from a somewhat different angle in the following sense. Examinations of the diverse early physicalist doctrines as well as of the priority claims concerning physicalism tend to focus on the accounts of the "primary language" or the "protocol language" (in Wittgenstein's or in Neurath's and Carnap's terminology), i.e. on the question whether observation sentences ought to be formulated in phenomenalistic or physicalistic language (or in material mode, whether perceptual reports refer to physical objects or experiences). In contrast, I will concentrate on Carnap's and Wittgenstein's views on psychological sentences, in particular on heteropsychological sentences, of which they both proposed a physicalistic account, from the late 1920s. I will examine their views between 1929 and 1932 in detail and query their connections and the arguments put forward in favor of them. I will argue that Carnap's rejection of the analogical inference to other minds, or more precisely his rejection of the possibility to formulate heteropsychological sentences in phenomenalistic language (first published in "Psychology in Physical Language" in 1932, but proposed already in 1930), marks an important shift from the Aufbau-view concerning other minds/heteropsychological sentences. I will also consider different reasons for this change, among them the possible effect of Wittgenstein's views on heteropsychological sentences put forward from late 1929 (the "despot view" and his arguments to the point that it is "logically impossible" to know others' conscious states). Presenting Carnap's and Wittgenstein's rather similar views as well as their rather different motivations and background assumptions will, I hope, cast further light upon the emergence of physicalism in the early thirties (and possibly also on Wittgenstein's troubled relationship with Carnap).

[234] The parting of the ways of two Fechnerians: Wundt's and Mach's philosophy of science compared

Heidelberger, M. (*University of Tübingen*)

The physicist and founder of psychophysics, Gustav Theodor Fechner (1801-1887), had a significant, albeit covert, influence upon the history of the philosophy of science. Because of his panpsychist fantasies, it was not so easy for his followers to openly profess themselves to his ideas. In this talk, I will show that both Wilhelm Wundt (1832-1920) and Ernst Mach (1838-1916) started out from Fechner in their methodological views and their understandings of the philosophy of science. For both, Fechner's *Elements of Psychophysics* of 1860 served as a point of departure for their mature philosophy of science. Fechner followed an anti-metaphysical program in his psychophysics. In order to avoid an early disruption by metaphysical views on the min-body relation, he restricted the object realm of his new theory to "appearances" (*Erscheinungen*) and propounded an abstention from any causal interpretation of the "functional relations" between them.

From early on, Fechner had defended atomism and claimed that it could be obtained through inductive (and thus non-metaphysical) reasoning from experience. Initially, Ernst Mach was an ardent follower of both Fechner's atomism and his anti-metaphysics. When he found out, however, that Fechner used similar arguments to defend belief in panpsychism and other seemingly metaphysical features he became deeply confused. Ironically, it was in Fechner's *Elements* that he found a remedy: What Fechner had proposed for psychophysics should be transferred to the whole of natural science. The prize to be paid for this was to give up atomism and causality and to put the relations between appearances center stage.

On the other hand, Wilhelm Wundt accepted that science has to rely on objects and events that are not given in direct experience. He was attracted by Fechner's psychophysics because he thought it could be used as a starting point for an experimental *psychology* (and not only for psychophysics relating the psychical and the physical). From early on he followed Fechner in the opinion that a metaphysics based exclusively on experience is possible and desirable. Both Wundt and Mach worked out their ideas in the late 1880s/early 1890s. Wundt wrote a voluminous *System of Philosophy* in which metaphysics in the sense of Fechner loomed large. The term "inductive metaphysics" was coined for this position and it quickly gained wider acceptance.

Wundt claimed that the real metaphysics of the day did not originate from philosophers but from scientists. He especially dealt with Ernst Haeckel, Wilhelm Ostwald and also with Ernst Mach as prime examples. He gave a remarkable and searching critique of Mach's "metaphysics" that, to my knowledge has so far escaped the attention of scholars. There is no symmetrical critique on Mach's side of Wundt's philosophy of science but one can try to reconstruct a possible reaction of Mach to Wundt from his other writings.

In the conclusion I point out that certain features of Mach's views are represented by Carnap and others of Wundt's views can be found in Reichenbach's work.

[236] The evolution of notations for the algebra of logic

Schlimm, D. (*McGill University*)

The 19th century tradition of the "algebra of logic" played a crucial role in the development of modern logic. Inspired by previous work on symbolical algebra, George Boole began this tradition by formulating an algebraic calculus for logic and the theory of probabilities (1847 and 1854). Subsequent work by W. Stanley Jevons, C.S. Peirce, Hugh MacColl, Ernst Schröder, and others led to numerous reformulations, refinements, and extensions. The individual conceptual contributions of these authors have been studied extensively, but relatively little attention has been paid to the particularities of their notations (with the exception of Peirce's graphical notation). Thus, instead of focusing on the conceptual developments in symbolic logic in the second half of the 19th century, I will take a closer look at the motivations behind the notational changes during this period and at the debates surrounding them.

Some of the milestones in the evolution of the notation discussed in this talk are the following. Boole (1848, 1854) explicitly chose the arithmetical symbols '+' and 'x' to highlight the analogies between logic and algebra. Moreover, to allow for his formulas to be interpreted by the numbers 0 and 1, he was forced to admit disjunctions of the form 'A + B' only in case A and B were exclusive. Otherwise, this expression was meaningless in his system. Thus, for Boole, conjunction and disjunction were not dual to each other. His choice of notation reveals that he valued analogies between theories (logic and arithmetic in this case) higher than analogies between the operations within a theory. This was criticized by Jevons (1874), who suggested the symbol '.|.' for disjunction. This still resembles the plus sign (indicating that there are some analogies), but is not identical to it (indicating that there are also important disanalogies). Moreover, Jevons disagreed with Boole's need for exception in the interpretation of disjunction and understood it inclusively (1864), as we do today. This led to a more symmetric presentation of the theory that was taken up by Schröder (1877) and developed further into a two-column presentation that was popular at the time for projective geometry. The differences between logical and arithmetical meanings of the symbols were highlighted also by Peirce, who augmented the logical ones by commas, writing '+,' and 'x,' for the logical analogues of '+' and 'x'. In regard to the choice of symbols, the treatment of negations was the most varied: not-A was written by Boole as 1-A, by Jevons as *a*, by MacColl as A', by Schröder as A_1.

In this talk, I will present the motivations underlying the individual choices of notation described above and argue that they were not arbitrary, but based on careful, deliberate considerations. In particular, these choices did not aim primarily at increasing the expressive power of the theories, but they reflect both pragmatical considerations regarding the ease of use as well as philosophical views about the nature of logic and of formal theories in general.

[238] Popperian Roots of Feyerabend's Theoretical Pluralism

Collodel, M. (*Independent*)

Based on both published and archival sources, this paper provides an account of the origins and development of Feyerabend's Theoretical Pluralism (TP) including a detailed reconstruction of the arguments which Feyerabend offered in its support in a series of papers published between 1962 and 1968.

Contrary to recent interpretation, the context of composition and the argumentative structure of the essay in which Feyerabend introduced TP in 1962 as well as further textual evidence from Feyerabend's correspondence and later work show that Feyerabend considered TP as a proposal of a method for science. More specifically, TP is the normative counterpart of Feyerabend's descriptive Incommensurability Thesis (IT). Both IT and TP bear some debt to Popper's views, which Feyerabend imbibed throughout the 1950s. In particular, TP displays its Popperian roots in that its core principles – the principle of proliferation, the principle of tenacity and the principle of the heteronomy of facts – result directly from the combination of two tenets of Popper's Falsificationism. First, (i) the methodological preference for maximally falsifiable scientific theories, where the degree of falsifiability of a theory is measured in terms of the size of its empirical content, i.e. the class of its potential falsifiers. Secondly, (ii) the idea that observation sentences are low-level hypotheses and that observational evidence is ultimately theory-laden. A radical understanding of (ii), influenced by stimuli that Feyerabend received from the quantum physicists David Bohm in the late 1950s, led Feyerabend to deny that facts contrary to a scientific theory T can always be recognized and described from within T's conceptual framework. Accordingly, and elaborating upon (i), Feyerabend claimed that the empirical content of T is partly dependent on theories that are alternative to it and that only the strongest possible alternatives to T, i.e. theories semantically incommensurable with it, could assure T's highest possible degree of falsifiability.

Given TP's distinctively Popperian pedigree, it is not surprising that Feyerabend initially thought of it as an enhancement of Falsificationism in the spirit of Popper's Critical Rationalism. In this respect, two points are worth noticing. On the one hand, it must be emphasized that Feyerabend's attempt to push Popper's Falsificationism to the extreme ultimately fails as it stretches Popper's views beyond rupture point. Indeed, incommensurable theories as conceived by Feyerabend turn out to be logically disjoint, i.e. radically incompatible beyond the expressive capability of negation as a logical operator. As commentators made clear in the second half of the 1960s, this eventually undermines the falsificationist rationale behind Feyerabend's argument. On the other hand, it is remarkable that what urged Feyerabend to abandon TP by the late 1960s was not so much the critical reception that TP had met as Feyerabend's sweeping disillusion with philosophy of science as a normative discipline in general together with the psychological burden of his intellectual debt to Popper. However, Feyerabend did not drop pluralism itself, but he later supported it on the grounds and within the framework laid down by J.S. Mill, who became Feyerabend's new philosophical father figure.

[239] Abstraction and generalization in Charles S. Peirce's graphical logic: A study from the context of nineteenth century scientific practice

Cristalli, C. (*University College London*) and Pietarinen, A. V. (*Tallinn University of Technology*)

Abstraction and generalization are important elements of Peirce's graphical logic. Developed as an analytical method to aid the scientist's analysis of data, graphical logic is built on the intuition that the objects of scientific inquiry are not things in themselves but their relations (Peirce 1906: 494-4). Graphical logic matured in Peirce's 1903 Lowell Lectures to incorporate expressive tools such as higher-order logic of potentials (R 468, R 478). Our question concerns the scientific context that saw the emergence of abstraction and generality as key drivers for Peirce to develop this unique higher-order logic. We show that abstraction and generalization in graphs are an elaboration of the experimental method exemplified in Helmholtz's studies in optics and in the inferential nature of perception; we then contrast such processes with the nineteenth century development of photography and its scientific use.

On the one side, Helmholtz's influence on Peirce is a problematic chapter in the history of philosophy and science. Peirce's comments on Helmholtz are scant and Peirce does not engage with Helmholtz's theory of the unbewusster Schluss (unconscious inference), which nevertheless is very relevant in Peirce's own theory of perception. Yet inference is for both thinkers strictly related to the problem of the meaningfulness of scientific results and consequently to the role of abstraction and generalization in science.

On the other side, the relation between Peirce's thought and photography has been studied with generalization technologies such as Galton's composite photographs (Ambrosio 2016) and with the generalising power of drawn diagrams and lines is investigated (Hoel 2012). The case of Helmholtz and the analysis of Peirce's higher-order logic can further unpack the tension between devices to describe thought and its generalising/abstracting faculty and devices to translate thought into another meaningful system. The first activity, Peirce would claim, belongs to the department of psychology and physiology; every mechanical representation of a thought-like process, like the use of photography to embody a generalization, would fit into the "description" category. Graphs are not descriptions but rather translations of such activities into a formal, graphical language. As in every translation, something is lost and something is gained. What is gained in higher- order graphs is a substantive dimension of possibility which is not found in the actual, particular thought; such process is called by Peirce "hypostatic abstraction." We venture that hypostatic abstraction provides the link from the activity of abstraction to the generalised possibilities that constitute the objects of experimental science.

Peirce's investigation of abstraction and generalization significantly predate the conceptual research that in the recent years has re-emerged in the context of modelling, simulation and philosophy of scientific practices. Beginning with certain problems that the phenomena of abstraction and generalization in the 19th-century posed to the practitioners of science, our discussion is calculated to propose a new insight into how contemporary tools of logic can help in securing meaningful scientific inferences.

[240] Citation analysis as a tool to study the recent history of analytic philosophy

Petrovich, E. (*University of Milan*)

Citation analysis is the core area of quantitative studies of science (scientometrics). Citations are currently used (and sometimes abused) in science to gauge the scientific impact of journals, institutions, research teams, even individual researchers, with a proliferation of indexes and metrics (Mingers & Leydesdorff 2015). However, evaluation is not the only purpose of citation analysis. In fact, interesting features of scientific fields, such as their morphology and evolution, can be traced by citation analysis methodologies, shedding light on topics which could be hardly addressed by qualitative methodologies (Chen 2003).

In the paper I would like to present at HOPOS 2018, I will discuss the potentiality as well as the methodological issues involved in using citation analysis to study the evolution of analytic philosophy in the late twentieth century. Applying to a philosophical field a methodology originally designed to study science is an interesting challenge, because it allows to check the validity of the comparison between analytic philosophy and Kuhnian normal science, which was recently proposed by Levy (2003).

In the first part I will focus on a crucial methodological issue: the meaning of citation scores in the case of analytic philosophy. Specifically, I will distinguish the bibliometrical notion of impact (i.e. the number of citations a document or an author collects) from the meta-philosophical notion of quality (which implies the reference to meta-philosophical standards).

Then, I will present three citation analysis studies of analytic philosophy, to give an idea of the kind of topics that might be addressed by this methodology.

The first study makes use of co-citation networks (the so-called "science-maps") to track a feature of contemporary analytic philosophy which is hard to capture by traditional historical-philosophical means: the increasing *specialization* of analytic philosophy, i.e. the progressive fragmentation of the field in distinct sub-disciplines. Specialization will be recognized as a specific pattern in subsequent co-citation networks.

Specialization will be the target also of the second study. I will present a "Classic Index", which calculates how many classics (i.e. highly cited reference) are cited, in average, in the bibliographies of analytic philosophy papers. This index may be interpreted as a measure of the "locality" vs. "generality" of a paper. Tracking its evolution over time sheds light on the specialization process from another perspective.

Finally, the third study integrates traditional close reading with descriptive statistics, focusing on the reasons behind citation behavior in analytic philosophy. Whereas in the case of science the main aim of citations is supporting the claims of the citing paper, in the case of analytic philosophy citations serve diverse purposes. In this study, citations are classified according to the function they serve in the citing papers. Working with a representative sample of analytic philosophy papers from 1950s to 200s, this study clarifies the balance between consensus and disagreement in analytic philosophy, illuminating the measure in which analytic philosophy is comparable to a Kuhnian normal science.

[241] Hermann Cohen on history and the universal validity of knowledge

Edgar, S. W. (*Saint Mary's University*)

There are two strains in Cohen's mature account of knowledge that sit in apparent tension with one another. First, in Cohen's 1902 *Logic of Pure Knowledge* he argues that philosophy aims to account for knowledge's validity [*Geltung*], a validity he takes to be timeless and universal. Indeed, for Cohen, the fact that philosophy aims to account for knowledge's validity ultimately explains the position he defends as early as his 1871 *Kants Theorie der Erfahrung*, namely, that empirical psychology is irrelevant to a philosophical theory of knowledge. But second, Cohen's accounts of knowledge are undeniably *historical*. This strain of his thinking is most clear in his *Principle of the Infinitesimal Method and its History*, where he traces the origins of the principle of continuity as a principle of mathematical and philosophical theorizing from ancient Greece to the eighteenth century. But even Cohen's mature *Logic of Pure Knowledge* opens with an introduction that at least partly functions as a history of theories of knowledge in the modern period. That is, even in the same work that stresses his concern with knowledge's timeless validity, Cohen's work retains its historical orientation.

The apparent tension is this. If the validity of knowledge that Cohen aims to account for is timeless, then it seems as though the history of that knowledge cannot have any philosophical relevance to its validity. If that is right, then there is no philosophical interest in tracing that knowledge's historical origins. Conversely, if tracing the history of an item of knowledge is necessary for a philosophical account of that knowledge, then it seems that the knowledge cannot have the kind of timeless validity that Cohen thinks it has. As Wilhelm Windelband argues, historicism -- that is, treating knowledge as bound to the particular historical conditions in which it is produced -- makes just the same mistake as the psychologism that Cohen wants to reject: like psychologism, historicism fails to account for the timeless validity of knowledge.

This paper aims to explain the significance of the history of science for Cohen, given that he thinks knowledge has a validity that is timeless. Using Cohen's *Principle of the Infinitesimal Method and its History* as a case study, the paper defends the view that, for Cohen, a historical view of the development of science is necessary to identify that science's most fundamental methodological commitments. Consequently, even if those methodological commitments express truths that have timeless validity, philosophers can identify them only by tracing their historical development. Finally, it is consequence of this view that Cohen has a conception of the history of science on which there is no robust contingency in how that history unfolds, and thus no real possibility of relativism with respect to different historical eras.

[242] Christiaan Huygens and the Animals: Notes on their Role in his Epistemological Considerations on Natural Philosophy

Marinucci, L. (*University of Cagliari*)

Christiaan Huygens' some late writings – written in the period from 1686 to 1695 and collected in the volume XXI of the *Œuvres Complètes* by the *Société Hollandaise des Sciences* – bear witness to very interesting philosophical and theological reflections. In his *Cosmotheoros*, which was intended for publication, and in the manuscripts: *Verisimilia de planetis*, *Pensees meslees*, *Quod animalium productio*, *Que penser de Dieu?*, etc., which could be regarded as preparatory drafts for the *Cosmotheoros*, Huygens deals with themes such as God's power, divine and human intelligence, probabilistic epistemology, natural theology and plurality of worlds.

Reading between the lines of these main topics, Huygens' reflections on animals play a key role in his epistemological considerations on natural philosophy. In fact, even though they should be considered "machines", the animals have a soul that differs from ours only by virtue of the rational side and the "miracle" of generation is part of the mechanical laws of nature, as there has not been a unique creation, but many of them. These reflections are able to highlight Huygens' religiosity, and demonstrate his interest in and standpoint on a still open post-Cartesian debate. Therein, I may include Huygens among the "empiricists", who, in seeking an explanation of miracles in the context of mechanical philosophy, try to use them as practical evidence of the Christian truth and as *a posteriori* proof of God's existence.

In my paper, I will explain how the issue of animals' generation is crucial to identifying the elements of continuity between the scientific topics of Huygens' wider works – especially his inventions and studies on microscopy – and the philosophical conjectures elaborated in his last writings. Since the "mystery of generation" would be evidence of divine will and purpose in the universe, Huygens' observations and reflections on animals may be considered as the point of intersection between his understanding of mechanicism and the teleology of nature. Furthermore, in exploring his correspondence, I will try to highlight his interest and involvement in this debate, thereby adding a new philosophical perspective on the probabilistic arguments on the nature of other planets and their inhabitants in the *Cosmotheoros*.

The challenge will involve, firstly, demonstrating that Huygens' final writings on philosophical and theological reflections on mechanistic philosophy are not anomalies within his wider scientific work and, secondly, showing that these are indications of his involvement in one of the most relevant theoretical debates of the second half of the seventeenth century.

[243] Mathematical controversies around Cartesianism: Clerselier, Fermat, Rohault

Dobre, M. (*University of Bucharest*)

This paper explores the mathematical controversy between Claude Clerselier, Jacques Rohault, and Pierre de Fermat. The debate was generated by Fermat's position concerning Descartes's Optics, which was reopened in 1658 by Clerselier. While the discussion was already started before Descartes's death, it received renewed attention in the milieu centred around Clerselier, Descartes's literary executor. For example, a contemporary note in the *Philosophical Transactions* testify that "M. Clerselier and M. Rohault took up the Gantlet, to assert the Doctrine of the deceased Philosopher, exchanging several Letters with M. Fermat, all inserted into this Tome [*Lettres de M. Descartes*, Paris: Ch. Angot, 1667]." I take this case as a privileged example for the evolution of Descartes's legacy in the second half of the seventeenth century. In the paper, I plan to examine the controversy with Fermat, but, instead of analysing it in the light of the earlier exchange between Descartes and Fermat, I shall broaden the focus by looking at Rohault's mathematics. While Rohault is most famous for his Cartesian experimental physics, his mathematical writings are largely neglected. Nevertheless, he was a "professeur de mathématiques" in Paris and Clerselier introduced him to Fermat as a "tres-sçavant Mathematicien." My paper will explore the connection of this early episode of mathematical correspondence involving Fermat with Rohault's writings on mathematics, which were published only posthumously in 1682. The key advantage of such approach is that it can offer a fresh perspective on the problem of the spread and the use of mathematics in Cartesian philosophy. It can further shed light upon the reception of Cartesianism in various other philosophical contexts, especially in those paying attention to the application of mathematics to particular problems. For example, as a consequence of such approach, the positive reception of Rohault's physics in England would be better understood under the framework of a common methodology. I shall argue that the early controversy in which Clerselier had urged Rohault to enter, would offer insightful hints for the way in which natural philosophy and mathematics blended in the works of the latter. My research hypothesis is that Rohault was applying a method derived from his early activity as private teacher of mathematics. In order to test this hypothesis, I shall compare Rohault's writings on mathematical topics with his celebrated *Traité de physique* (1671). I aim to analyse the argumentative structure of several explanations in Rohault's *Traité*, which I shall trace back to his mathematical writings. It is through this reconstruction of Rohault's method that I return to the starting case and examine the role of mathematics in the reception of Descartes's philosophy.

[244] How Atoms Became Real

Ivanova, M. (*University of Cambridge*)

This paper revisits the debate on the reality of atoms. At the turn of the 20th century, many physicists treated the atomic hypothesis with substantial scepticism, claiming that atoms were fictional entities. While many, such as Wilhelm Ostwald and Henri Poincaré, changed their minds after the publication of J.J.Thompson's and Jean Perrin's experiments, some, such as Pierre Duhem and Ernst Mach, continued to oppose the reality of atoms despite the experimental support. I argue that at the heart of this debate are methodological arguments that influenced physicists' stances both before and after experimental evidence in favour of the reality of atoms. Ostwald and Poincaré were able to accept the reality of atoms since the atomic hypothesis became scientific on their terms in light of being experimentally testable, with the multiple ways of calculating the number of atoms in a volume being particularly convincing. However, contrary to accepted wisdom, this acceptance did not indicate a shift from an instrumentalist to a realist attitude towards science in general, rather it indicated the shift in how the atomistic hypothesis was perceived. In particular, Ostwald and Poincare argued that the atomic hypothesis because an empirically testable rather than metaphysical hypothesis, being able to be subjected to experimental tests. I argue that Poincaré continued to doubt whether the fundamentalist outlook on the composition of reality was justified in science, claiming that the atom of the chemist is not the irreducible particle the metaphysicians were after when initially evoking atoms. While Ostwald and Poincare were able to accept the atomic hypothesis in light of the new experimental evidence in its support, Mach and Duhem continued to reject the reality of atoms. Even after Perrin, they continued to hold that science should not explanation observational phenomena by appealing to unobservable entities. Both offered similar arguments, claiming that the atomists of the time were subordinating physics to metaphysics and violating the integrity of empirical science.

I argue that the debate pre and post Perrin's and Thompson's experiments illustrates how philosophical considerations regarding scientific methodology influenced physicists' decisions as to what constitutes genuine scientific evidence in favour or against a hypothesis. While Ostwald and Poincaré initially opposed the atomic hypothesis, they were able to accept it as scientific after the experiments of Thompson and Perrin, endorsing also a broader notion of observability, claiming that Perrin made atoms 'observable' since he could count them. On the other hand, Mach's and Duhem's criterion for acceptability remained unchanged, demanding only direct observation as constituting genuine scientific evidence.

[245] Poincaré Read as a Pragmatist

Stump, D. J. (*University of San Francisco*)

The French reception of pragmatism in the early 20th century is complex. The French associated pragmatism almost entirely with William James, who was by far the best known and who had multiple connections with important French philosophers and psychologists. Therefore, some in France who rejected pragmatism were actually just rejecting James. Although there are scant direct connections between Poincaré and the pragmatists, he has been read as one from early on, for example by René Berthelot (1911). Berthelot's idea was to present Poincaré as the most objective of the pragmatists, while presenting Nietzsche as the most subjective. The idea of a book on pragmatism based on two authors neither of whom are typically put in the cannon of pragmatism may seem bizarre, but there is a compelling logic to looking at the extremes in order to define what pragmatism is and to find common themes throughout the movement. Poincaré certainly shares some themes with the pragmatists, especially the idea of a human element in knowledge that can be seen in his theory of the role that conventions play in science. Poincaré also emphatically rejects a metaphysically realist account of truth as correspondence to an external reality. Perhaps wisely, he does not specify precisely what he does mean by truth, but he frequently uses the language of "useful" or "convenient" theories. Of course, for Poincaré there are limits to the conventions that we add to our knowledge. First, he holds that these conventions are guided by experience so that we are more likely to choose certain alternatives. Second, he directly and forcefully rejects LeRoy's interpretation that conventions are found everywhere in science. Poincaré insisted that there are empirical facts, along with conventions. His position is easily comparably to Dewey's insistence that science is objective even if we reject the metaphysical realist account of representation and hold that values and aims play a role in defining scientific knowledge. Besides clarifying Poincaré's philosophy of science, reading him as a pragmatist puts his writings into a larger context. The development of 20th century philosophy was influenced heavily by dramatic developments in mathematics and physics. Poincaré was a pioneer in incorporating these developments into philosophy of science and his pragmatic attitude towards the development of non-Euclidean geometries and relativity in physics was a profoundly influential contribution to the philosophy of science.

[246] Pragmatism and the Analytic

Patton, L. K. P. (*Virginia Tech.*)

The roots of the search for an operational definition of "analyticity" are deep within pragmatism and its nineteenth-century encounters with empirical psychology and empiricist logic. In 1882, Josiah Royce's essay "How Beliefs are Made" deals with the question of distinguishing attention from occurrent sensation. Royce relates this question, which preoccupied Wilhelm Wundt in his work on empirical psychology (cited by Royce), to the question of how to distinguish the results of free choices of where to put one's attention from sensation passively received in experience. By making this distinction precise, one might try to build a behavioral characterization of analytic reasoning, as consisting of freely chosen orientations toward experience using concepts as 'schemas' (in C. I. Lewis's terms). Royce and Lewis were influenced deeply by their association with William James (see Kegley 2016). The paper will trace James's empirical psychology (and his reception of German work in that field), which was an early stimulus of the abandonment of attempts to provide an operational distinction between analytic and synthetic reasoning. (German scholars, including Friedrich Lange, recognized that no such distinction could be justified as a natural kind of inference in empirical science or in empirical psychology.) Jacob Loewenberg, the editor of Royce's *Lectures on Modern Idealism* and *Fugitive Essays*, published an essay in 1956, "Royce's Synthetic Method", exploring Royce's views on analysis and synthesis. Loewenberg argues that Royce, for logical and methodological reasons, does not accept an absolute distinction between analytic and synthetic reasoning. Lewis, on the other hand, struggles to reconcile his views on analyticity with his pragmatism. Both are wrestling with the heritage of James.

A paper in this session will explore the complementary question of the impact of Deweyan naturalism on Quine – certainly, Quine's search for an operational definition of the analytic (as emphasized by Creath) can be read within this framework. This paper's aim is not to trace the background of Quine's work, but to situate it in a much larger tradition, whose questioning of the notion of analyticity had been going on long before Quine, and continued after 1951. Quine's "attack" on the analytic-synthetic distinction in "Two Dogmas of Empiricism" is not sui generis. Quine's philosophical training took place in the tradition of Harvard pragmatism, in the wake of the encounters between James and Royce, especially its impact on the work of their student, Lewis, who was Quine's professor and mentor. (Lewis's *An Analysis of Knowledge and Valuation* is key to the context of Quine's essay; Sinclair and Ben-Menahem emphasize the Lewisian and Jamesian influences, respectively, on Quine's work.) Even though Quine was no fan of Royce, as Misak observes (2016), long before 1951 Royce had provided an influential pragmatist account of analysis and synthesis as operations of the mind, an account that influenced Morton White and Loewenberg. Quine's essay is not an outlier, but a work within a pragmatist tradition that had been preoccupied since the late nineteenth century with analyticity as an *operational* notion. This tradition deserves its own history.

[255] Wittgenstein's philosophy of mathematics in the context of philosophy of science: Engineer approach

Sokuler, Z.A. (*Lomonosov Moscow State University*)

Wittgenstein influenced two antagonistic branches of philosophy of science: logical positivism and postpositivism. His influence on the logical positivism is well studied, but we have to remember that logical positivists did not follow Wittgenstein in some crucial issues of his philosophy of science: his treatment of mathematics and theoretic propositions of science. Wittgenstein's influence in post-positivism is less studied. But it was admitted by Quine and Feyerabend; Kuhn also cites Wittgenstein in his "Structure of Scientific Revolutions".

Although Wittgenstein interpreted language differently in "Tractatus" and "Philosophical investigations", there is an important common approach in them: the holism. For the early and as well as for the late Wittgenstein language is understood as a complex system, an integral network of forms und rules.

Wittgenstein's holism can be slightly traced in R. Carnap's notions of "conceptual frameworks" and is obvious in post-positivist conceptions, that admit, that the whole context of theory determines each term within it.

But there is another idea in Wittgenstein's philosophy that has important implications for philosophy of science, and is less known than his ideas concerning language. It is known that most notable impact in philosophy of science belong to theorists of physics and pure mathematics. Till now, there was no influence of scientists of applied science. So, philosophy of mathematics was determined by pure mathematics: Frege, Russell, Hilbert, Gödel, Brouwer and others. Wittgenstein is unique, because of his engineer background. This fact determines the peculiarity of his thought, which manifests itself in his philosophy of mathematics. While for Frege, Russell, Hilbert, Gödel mathematics is a corpus of true sentences, for Wittgenstein it is an aggregation of different calculi. Mathematical propositions are not propositions at all, because they cannot be true or false. They are the rules of certain kinds of activity. They play this role only within some systems.

For pure mathematics the rigor was an absolute ideal and value. That is why the problem of foundations remained central till lately. Wittgenstein's sees mathematics differently, not concerning its foundations. He seeks relevant descriptions of how mathematicians practice their work with calculi. In this way, Wittgenstein presents a standard of descriptive, not normative, philosophy of science. He follows changes in how mathematicians use their calculi, concepts and rules. He demonstrates that the discoveries of unexpected mathematical facts were determined by changes in practices of using certain concepts. He draws attention to the historical variability of mathematical practices, which results in shifts of the meaning of the notions used. In that way Wittgenstein disproves mathematical platonism and introduces the understanding of mathematics as a (collective) institutional activity. It is controlled by certain rules, which are subject to changings.

No doubt, mathematical facts are constructs for Wittgenstein. At the same time he points out that the set of rules implicates what is possible and what is impossible in the system. He also showcases the role of the practical applications of mathematical calculi in establishing of their status.

[260] Transfer principles and Klein's group-theoretic structuralism

Schiemer, G. (*University of Vienna*)

An important development in nineteenth-century mathematics towards a "structuralist" conception of the field is related to work on invariants. Classical invariant theory was established as an algebraic research field in the second half of the nineteenth century in work by A. Cayley, J. Sylvester, and D.Hilbert as the study of polynomial functions that remain invariant under transformations from given linear group. In the context of geometry, invariants became of central importance in work by Felix Klein, in particular, in his well-known Erlangen program. Klein's programmatic article Vergleichende Betrachtungen über neuere geometrische Forschungen (1872) presents a new methodology for geometrical research. Roughly put, the central idea is to classify geometries group-theoretically, that is, in terms of the properties of spatial objects that are preserved under the transformations of a given symmetry group. Given this approach, different types of geometrical spaces (i.e. Euclidean, projective, spherical, affine, etc.) can each be characterized in terms of their transformations groups and the resulting invariants. Moreover, given that the transformation groups corresponding to such spaces are often related by group inclusion, Klein observed that the geometrical theories describing them can be ordered and classified in terms of their corresponding groups.

In the talk, I will give a closer discussion of Klein's group-theoretical approach in geometry and its structuralist underpinnings. It has often been stated that the Erlanger Program has contributed significantly to a "structural turn" in modern mathematics. But what precisely is the structural character of this research program? To address this question, the talk will focus on a central conceptual assumption underlying Klein's proposal to redefine geometry as a form of invariant theory: geometry is no longer conceived here as the study of particular figures in space but rather as the study of the properties of figures that remain invariant under structure-preserving permutations. Given this account, one can say that the subject matter of a given geometry is fully specified by its corresponding group of transformations and thus by the abstract structure encoded in this group. Moreover, as Klein showed in his work on "transfer principles" between different spaces or manifolds, two geometries can describe very different basic spatial elements but nevertheless be structurally equivalent in case their corresponding transformation groups are isomorphic. This is, as I will argue in the talk, clearly a structuralist approach, similar in several respects to modern thinking about mathematics in category-theoretic terms and to categorical structuralism more specifically.

[264] The Integration and Disintegration of the History and Philosophy of Science in Princeton University, 1961–1981

Reiss Sorokin, O. (*Princeton University*)

While scientists surely do science, they almost never deal with "Science". However, this upper-case "Science" is an important object for other discourses such as: journalism, policy-making, and - in the academic setting – (sub-)disciplines as the History and the Philosophy of Science. "Science" is, in fact, a knowledge-object of the Humanities.

In my paper I will tell the story of the History and Philosophy of Science of Princeton University, from its foundation by Charles Gillisipie, Carl Hempel, and Hilary Putnam in 1961; through the 1971 crisis caused by Thomas Kuhn's resignation from his role as the head of the program; down to its reorganization as a History of Science program in 1981. Drawing on archival documents as well as published work by the protagonists, I hope to explain the 1981 breakup. I plan to argue that the term "Science" changed its meaning from the 50's to the beginning of the 80's. While at the beginning the idea of a shared HPoS program seemed both reasonable and desirable, it was no longer conceivable in the end since the two disciplines no longer studied the same object. Princeton's HPS program can be an interesting case study for observing the changes in the field at large, and thereby can be used as a "historico-transcendental" framework for understanding past, as well as current, works in the field.

[273] Proof Transformations in the Work of Charles L. Dodgson and Christine Ladd-Franklin

Abeles, F. (*Kean University*) and Reichenberger, A. (*Paderborn University*)

Influenced by Boole and his followers, Dodgson solved problems exemplifying the central problem of the symbolic logic of his time, known as the 'elimination problem', i.e. determining the maximum amount of information obtainable from a given set of premises. These problems usually appeared in the form of a sorites, a linked set of syllogisms, often presented as a puzzle problem. He took problems to solve from books and articles authored by important contemporary logicians. Many of these problems came from Ladd-Franklin's article, "On the Algebra of Logic", her doctoral dissertation, in Charles S. Peirce's book from 1883, Studies in Logic. An advocate of Boole's logic, Ladd-Franklin wrote many articles on logic, often with coauthors, several of which appeared in Baldwin's Dictionary of Philosophy and Psychology (DPP). Those that relate to the topic of this paper will be examined.

Both Dodgson (1832-1898), and Ladd-Franklin (1847-1930) dealt with syllogistic arguments and both belonged to the calculus ratiocinator school of logical thought. In this article, I explore the connections between Dodgson's tree method and Christine Ladd-Franklin's antilogism. By interpreting a syllogistic argument in the form of a conditional, and setting up a test of the inconsistent triad of its propositions, both Ladd-Franklin and Dodgson changed the interpretation of the relationship of the conclusion to the premises of a syllogism.

The essential feature of the tree method is that when a conclusion following from a set of premises is assumed to be false, then if reasoning from it together with all the premises results in a contradiction, the original argument is proved to be valid. To test an argument given as a syllogism, Ladd-Franklin described a form into which all valid syllogisms can be cast: If its triad (two premises and conclusion) has that proper form, which she calls an antilogism, an inconsistent set consisting of the two premises and the negation of the conclusion, the syllogism is valid. Using a tree to test the validity of a syllogism requires that an antilogism be assumed, i.e. the triad of the two premises and the negation of the conclusion is assumed to be an inconsistent set at the outset.

Dodgson's Method of Trees is a sound, complete, and decidable proof system for soriteses which are complex syllogistic arguments. The concepts of soundness, completeness, and decidability of proof systems were first applied in the twentieth century. In "Symbolic Logic or Algebra of Logic", Ladd-Franklin's coauthored article with Louis Couturat in the Dictionary of Philosophy and Psychology (DPP), she included her antilogism in their survey article.

[274] Wilma Papst on Frege

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As a matter of course, today's 'logical community' builds on the premiss that Gottlob Frege's contributions are indispensable to the constitution of their domain. However, this has not always been the case. Recent research has made it evident that a broad turn towards Frege did not happen until 1936, and that it was driven by an expanding US-American logical community around Alonzo Church.

In German-speaking areas, the ‘logistic’ school of Münster may be viewed as an isolated forerunner to the propagators of an increased interest in Frege. However, the very first German doctoral dissertation on Frege was submitted to the University of Berlin in 1930. The author is Wilma Papst, a Berlin-born female student applying for a doctoral examination in Philosophy, Mathematics and Physics.

In her book, Papst explicitly states that it is her intention to dispel the lack of appreciation credited to Frege’s merits. Departing from the distinction between sense and reference, Papst first discusses Frege’s earlier thought. In this context, she assesses Frege’s critique of ‘psychologism’ and his critique of ‘empiricism’ in the philosophy of mathematics. Papsts claim is that it is the combinations of both critiques which enables Frege to think of predication in terms of functions and to develop his “Begriffsschrift”, and that both critiques converge in Frege’s critique of ‘formalism’. Apparently, Papst thinks that Frege’s critiques of ‘psychologism’ and of ‘empiricism’ are connected to his critique of ‘formalism’ via a missing link exposed in a section ‘on the derivation of the concept of number and the essence of arithmetic’.

Unpublished archival research, which we will present for the first time, has pointed out that both of Papst’s referees’ reports come to critical conclusions: In particular, the referees seem to agree that Papst’s exposition is not to the point. According to the reports, the author is somewhat ‘floating freely amongst her materials’, stumbling upon important issues without properly discussing them.

By re-addressing Frege’s concept of number and his account of arithmetic in terms of logical thought from Papst’s point of view, the presently proposed contribution will test whether the referees’ judgments are justified – or if they should be interpreted as an expression of the bias that women’s thinking is not apt to capture such fields as logic and the foundation of mathematics.

[275] Marie Deutschbein's and Walther Brand's "Introduction into the Philosophical Foundations of Mathematics”

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In 1929 the German book “Introduction into the Philosophical Foundations of Mathematics” appeared. The authors were Marie Deutschbein and Walther Brand. In the first chapter titled “Mathematics and Logic” the authors give an overview of the foundational debate (“Grundlagenstreit”) between intuitionist and formalist that developed along the decade of the twenties. My contribution deals with Deutschbein’s and Brand’s interpretation of the controversy.

At first glance, the author’s presentation recapitulates the standard reading: Cantor’s naive set theory seems to offer a common foundation to all the fields of mathematics. However, it treated infinity incautiously and boldly which resulted in logical paradoxes. Because Cantor’s set theory was unable to eliminate them, formal logic was engaged. David Hilbert developed a program as response to the “foundational crisis” in mathematics as a pure formalistic approach. His “program” required a logical formalization of all of mathematics in axiomatic form, together with a proof by “finitary” methods that this axiomatization was consistent.

I will present Deutschbein’s and Brand’s interpretation of (i) Hilbert’s program, (ii) Brouwers criticism, and (iii) the decision problem in the realm of Kantianism, focusing on two dominant themes through the foundational debate: First, the meaning of “existence” in mathematics; second, the status of the principle of the excluded middle, respectively the principle of contradiction.