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Abstracts

Thursday - 23 June (Parallel Session I)

Parallel Session I (9:00-10:30)

1.1 Symposium: Post-Aristotelian Science in the Fourteenth Century

Central to Scholastic Philosophy of Science is the role played by Aristotle. His scientific system compelled medieval thinkers to place it at the center of their educational system. Yet the Scholastics understood God and the universe in a fundamentally different way than Aristotle, and so they could not simply adopt Aristotelian thought as dogma; they needed to develop a framework within which they could use (or reject) the Philosopher's teaching. One particularly fruitful school of Aristotelian investigation was that of Franciscan thinkers working after John Duns Scotus. Scotus provided his followers with a unique interpretation of Aristotelian terms, and the means to apply Aristotelian epistemological and scientific doctrines to unique and often revolutionary ends. To these authors, Aristotle provided the tools, and Scotus showed how to use them, but they each achieved unique results that influenced subsequent generations of natural philosophers and theologians working in Latin and Hebrew. This symposium brings together recent research, much of it from manuscript, on three under-studied authors of the fourteenth century, all Franciscans working after John Duns Scotus: Peter Thomae, the Spanish Scotist whose rise to power in the Papal Court was tragically cut short by charges of Necromancy; Gerald Odonis, a Provençal theologian who became Franciscan Minister General at the moment of his Order’s greatest peril, and the Frenchman Nicholas Bonetus, a future papal ambassador to Kublai Khan. The approach these authors took to both maintaining and breaking with the Aristotelian tradition becomes evident through the presentation of their doctrines on Change, Place and Time.

Petrus Thomae and Early Scotist Theories of Analogy

Garrett R. Smith (University of Notre Dame)

Typical of many contemporary theories of the origin of the modernity is the claim that Duns Scotus' doctrine of the univocity of being signifies a major break with all previous philosophical and theological speculation, a break that initiated a decline in late medieval thought that led to a general collapse of late medieval society. Central to this narrative of a decline initiated by the univocity of being is the assumption that John Duns Scotus rejected the "traditional" doctrine of the analogy of being, and intended his theory of univocity to supplant it. There is also a widely held—but entirely unexamined—assumption that analogy and univocity are incompatible. This paper challenges that assumption. After some consideration of Duns Scotus' view on the compatibility of analogy and univocity, I will look at how his mature position was interpreted and developed by early Scotists such as Antonius Andreae and Petrus Thomae. One interesting feature of their accounts of the analogy and univocity of being is how they sidestep traditional interpretations of Aristotle that claim the Stagirite denied univocity and affirmed only analogy: the early Scotists drew a distinction between categorical univocity and transcendental univocity. The former pertains to the ten Aristotelian categories, the latter to God and creatures. Aristotle, the Scotists claimed, only denied categorical univocity, and indeed, contrary to common opinion, even affirmed transcendental univocity in an (obscure argument in Metaphysics II concerning the nature of truth. Another common distinction in early Scotism is that whereby the senses of the term 'univocity' are shown to be non-
univocal, with variations introduced depending on the perspective of various sciences. So for metaphysics and physics, univocity is “real,” that is univocal concepts have corresponding realities. But for logic, univocity is of concept alone, without a corresponding reality. This brings us to Scotus’ own ultimate solution of the problem of the relation of analogy and univocity, which is that analogy is a feature of things outside the mind, while univocity is a feature of concepts. Antonius Andreas and a number of other Scotists simply repeat Scotus on this point, but Petrus Thomae moved beyond Scotus and developed one of the most elaborate theories of analogy to be found during the entire medieval period. His theory is contained in the currently unedited Quaestiones de ente, written at Barcelona around 1325. In this work he makes the innovative claim that the same concept can be both analogical and univocal, for the terms ‘analogical’ and ‘univocal’ are both terms of second intention. This position proved to be at least historically successful, for the claim was repeated by various Scotists into the seventeenth century. Furthermore, Petrus Thomae devoted much space to close textual analysis in order to harmonizes the previous discussion of analogy in the Greek, Arabic, and Latin traditions. The result of this analysis was a theory of twelve degrees of analogy, various degrees of which were compatible with univocity or equivocity.

**Toward a Conception of Force: Peter Auriol’s Critique of the Aristotelian View that Action is Motion**

- Gloria Frost (University of St. Thomas)

This paper explores a crucial shift in medieval thinking about the nature of action and causality. Medieval thinkers accepted the basics of Aristotle’s hylomorphic ontology: On their view, the natural world is populated by substances composed of matter and form which cause and undergo change. There was widespread agreement that substances cause change in one another by acting through inherent causal powers. Yet, as this paper will show, there was considerable disagreement about what an action itself is. On the Aristotelian view adopted by Thomas Aquinas (d. 1274) the action of an agent was considered to be ontologically the same as the change which it causes in its patient. When, for instance, fire burns a log, the fire’s action of burning just is the change which the log undergoes. Peter Auriol (d. 1322) vigorously argued against this position. According to him, in addition to agents and the changes which they cause in their patients, there must be an exertion or impression of the agent’s causality onto the patient which is ontologically prior to and thus distinct from the change in the patient. He describes this action as the “flowing forth and going out of the mover to the moved” (effluxus et egressus motoris in motum). Auriol clarifies that actions are not substances nor do they inhere in substances. Rather they are fleeting and transitory causal “intervals” (intervallum) between substances. Auriol’s position marks a significant departure from Aristotelian ontology which maintains that everything which exists is either a substance or an accident modifying a substance. His “actions” can also be seen as a precursor to the notion of force in early modern thought in so far as they are non-substances which are explanatory of motion.

**Nicholas Bonetus’ Doctrines of Natural and Mathematical Space and Time**

- William O. Duba (Radboud Universiteit Nijmegen)

Nicholas Bonetus (fl. 1333) took the Franciscan approach to Aristotelian philosophy to its logical limit, and wrote his own natural philosophical corpus, comprising a Metaphysica, Physica, Praedicamenta and a Theologia naturalis. At least in many Central European studia of the fourteenth and fifteenth centuries, Bonetus’ work successfully replaced Aristotle as the introductory textbook to philosophy. Bonetus aimed
to present a philosophical system that embraced the core concepts and terminology of John Duns Scotus and subsequent Franciscan thinkers, starting with the doctrine of the univocity of being; when they appeared to conflict with each other, he would adopt a syncretic approach. Such is the case with the notions of place and space. Confronted with a conflict between the Aristotelian definition of place as the inside surface of a thing's containing body, defended by some of his colleagues, and that of place as being situated in three-dimensional space, held by others (such as Gerald Odonis), Bonetus in his Physica distinguishes between “Natural Place”, that is, place in the physical sense of being situated by a containing object, and “Mathematical Place”, place as being situated in an abstract three-dimensional extension. He then extends this distinction between Natural and Mathematical senses to time itself: Natural Time relates to some really existing motion, but in the Mathematical sense, time can be an abstract, continuous extension. Yet, in spite of the terms “Natural” and “Mathematical”, it is not clear which sense of Place or Time takes priority over the other. Moreover, this doctrine has major implications for Bonetus’ doctrine of Atomism.

1.2 Contributed Papers: Life Science

Ethology as a Goethian Science: Method, Intuition, and the Science of Life

• Shane Zappettini (Indiana University)

Johann Wolfgang von Goethe’s scientific work stands as somewhat of an enigma in the history of science. With no lack of breadth, his concerns ranged from problems of a general methodological nature to the development of specific theories in biology, geology, and physics. In spite of this Goethe's science saw a cold reception, with prominent 19th century figures accusing it of being amateurish, and its successes being seen as lucky accidents. This attitude has motivated contemporary historians and philosophers of science to reevaluate Goethe's “delicate empiricism,” especially in light of his theory of color, and contributions to developmental morphology. One unexplored line of influence Goethe's science had, or so I will argue, was on the theory of animal behavior developed by the classical ethologist Konrad Lorenz in the early-to-mid 20th century. Goethe considered epistemological questions pertaining to the relationship between the investigator of nature, and nature itself, to be central to science. At the heart of Goethe's approach to nature was his theory of intuition, which was developed, in part, as a response to Kant's idealism. Lorenz, too, was concerned with overcoming Kant's epistemology, and directly appealed to Goethe's theory of intuition in formulating his own evolutionarily informed response. For Lorenz, the solution to Kant’s idealism was to be found in looking at the means by which animals perceive and act in their environments, so as to uncover how their species-specific perceptual capacities enable and ensure contact with an extra-subjective reality. Goethe's intuitive method was a suitable place to look, as it was uniquely tailored to the investigation of dynamically unfolding processes like biological systems, and required that the investigator pay attention to how the organism and its environment form a unit of interaction that enables its form to unfold. Investigating the behavioral repertoires of animals, too, requires uncovering organism-environment interdependence, and so requires that the investigator approach his/her object through meticulous observation of it in its natural, ecologically valid, context. Both Goethe and Lorenz insisted that an epistemological analysis of the human observer would reveal the relationship between the knowing subject and the object of knowledge, which ultimately makes science epistemically possible. Lorenz was keenly aware that a philosophical defense of ethology's methods had to take into consideration the role of the observer in science, and like Goethe, attempted a phenomenology of nature that grounded the structure of the human mind in its dynamically unfolding processes. I will argue that Goethe's influence on classical ethology shows that his scientific pursuits had an impact on the history of
science in a way previously unexplored, and it is one that is fruitful for understanding how ethology is relevant to the history of philosophy.

Monkeys and Monads: The Unexpected Marriage between Darwinian Evolutionary Theory and Leibnizian Metaphysics

• Jeremy Dunham (University of Sheffield)

In 1893, whilst discussing the present status of metaphysics, F.H. Bradley prophesied that monadology ‘will increase and will add to the difficulties that already exist... [without supplying] a solution to any... of them’. Bradley clearly got something right. Monadology did flourish again at the turn of the twentieth century. In the first part of this paper, I explain why. But Bradley also got something wrong. At that time, the Darwinian revolution in biology introduced new philosophical problems. However, the new monadologists were able to solve them by modifying Leibniz's metaphysics into a template for articulating and justifying the metaphysical commitments of Darwinian evolutionary theory. At least, so I argue in the second part. The received view of the history of philosophy regards Leibniz’s monadology as the kind of metaphysics that was permanently abandoned after Kant’s Critique of Pure Reason. This is false. There was a plethora of twentieth-century monadologists. But why? My proposal is this: as philosophers began to grapple with the metaphysical implications of Darwin's theory of evolution, Leibniz's pluralistic metaphysics appeared to offer a better conceptual toolkit than the predominant metaphysical system on the market: absolute idealism. In particular, there is something to Ernst Mayr's (1982) controversial claim that Leibniz's monadology was the only available metaphysical system to have properly emphasized the importance of population rather than essentialist thinking. This is because, according to the Leibnizian system, the fundamental building blocks of the world are metaphysical simples, which Leibniz calls ‘monads’, and every monad is unique. Population-level patterns emerge from the relationships between these unique individuals. Even though this type of metaphysical thinking did not lead Leibniz towards a modern evolutionary biology, it suggests, given the role that population thinking had in the development of Darwin’s thought, that Leibniz’s monadology is appropriate for it. There is, however, still a problem with Mayr's story. Although Leibniz’s system does emphasise population thinking, it does not do so in a way that eliminates the need for the divine in the creation of population-level patterns, since the relations are the preformed products of God’s design. As such an elimination is one of Darwin's main desiderata, any successful post-Darwinian monadology must be able to redevelop the Leibnizian system in a way that eliminates this need for divine preformationism, but without undermining the monadology's overall coherence. The new monadologies of Howison and Ward are attempts to do just this. Howison and Ward knew each other well and they developed their accounts in correspondence. They believed that absolute idealism failed to account for individuals and to cohere with the changes in the understanding of life that evolution demands. Nevertheless, there are significant differences between the two. I argue that Howison made important developments to the monadology, but ultimately he failed to reconstruct the monadology in a way that was truly compatible with Darwinian evolution. Although Howison's monadic relations were no longer divine, they were still preformed. Ward successfully highlighted these failings in Howison's account, and showed, in response, that a truly evolutionary monadology implies epigenesis, understood as the theory that the progressive development of complex structures depends upon the contingent environmental interactions of individuals. It is for this reason that Ward's metaphysics should serve as a prolegomena to any future monadology.
1.3 Contributed Papers: Early Modern Perception and Reference

Perception by Patterning: Cavendish against Hobbes on Perception

• Marcus Adams (State University of New York, Albany)

Most of Margaret Cavendish’s criticisms of Thomas Hobbes in the Philosophical Letters (1664) relate to the disorder that would result if pressure caused perception. For example, she argues that pressure would more likely “annoy and obscure” than “inform” (1664, 20) and would cause a constant “war” (1664, 60). These criticisms relate to Cavendish’s desire to account for the order and regularity of natural phenomena in terms of bodies’ self-motions and perception by sensitive and rational matter. In this paper, I argue that beyond those already mentioned Cavendish advances a potentially more devastating criticism against Hobbes’s account of perception: Hobbes lacks the explanatory resources needed to accommodate commonplace, minor instances of differences in perception, such as between individuals who perceive one another at differing degrees of clarity. Her criticisms of Hobbes’s account of perception are of particular interest because of the centrality of his explanation of perception to his natural philosophy. Indeed, it is the foundational explanation that begins Part IV of De corpore and the first chapter of Leviathan. Thus, Cavendish’s criticism of the perception-as-pressure view attacks Hobbes at a crucial point. My argument proceeds in three stages. First, I discuss Cavendish’s view of perception as patterning, and I show the consequences that this view has for the “two men” thought experiment in Letter IV. Second, I discuss Hobbes’s explanation of degraded perception in old age in De homine to provide a potential Hobbesian reply to Cavendish’s criticism. Cavendish may have viewed such a reply made on Hobbes’s behalf favorably in the 1650s, especially since her explanation of “blindness” in the 1655 edition of Opinions is similar, but she removes the chapter on “blindness” in the 1663 edition of Opinions and explains perception by appealing to patterning self-motions, as well as to ways in which rational matter and sensitive matter can influence one another. Third, I argue that offering such a reply on Hobbes’s behalf, where differences in sense organs explain minor differences in perception between two perceivers, misunderstands Cavendish’s objective of showing the limited explanatory resources available in understanding perception as pressing. Cavendish’s claim is that pressure is too simplistic an explanans to account for the everyday occurrence of minor differences in perception, a fault which her patterning model with its intelligent matter avoids. Cavendish makes this claim in Letter IV by providing a thought experiment about two such individuals, arguing that “if perception were made by pressure, there would not be any such mistakes” in perception (1664, 20). It seems that Hobbes has an obvious reply to this criticism within his own optics, namely, that such differences in perception are due to hindrances of motion in media or in perceivers’ sense organs. However, I argue that we should understand Cavendish’s objection as showing that such a reply from Hobbes is bound to fail because of the lack of explanatory resources in the pressure model. In its place, I show how Cavendish’s sensitive and rational motions, and the regularity of their interaction, can better explain commonplace, even slight, differences of the sort that she discusses in the thought experiment.

Perception as a Two-Stage Process: A Reidean Account

• Marina Folescu (University of Missouri)

Thomas Reid is one of the first philosophers to think that primary qualities, some secondary qualities, and the material substances to which these belong are immediate objects of original perception. It is an interesting question how exactly substances (understood as bodies, primarily) are perceived, in particular, since Reid takes on board the Lockean arguments showing that we do not have, strictly speaking, simple ideas of sensation about the substances populating our environment. This paper explains how these very
different things can be immediately and originally perceived, by providing a detailed picture of the mechanism of original perception. I shall argue that Reid thought that perception is a two-stage process: at the first stage, one perceives qualities of objects (primary and some secondary ones); at the second stage, one perceives bodies, supplied by the perceptual mechanism as that to which the qualities of the first stage are bound. These two stages are sequential: one cannot perceive a body without perceiving some of its qualities first. The binding of features to objects is not an inferential process. Moreover, mental intermediaries do not mediate the process of binding features to objects. This is important, since it would be bad form for Reid if it turned out that the original perception of bodies makes use of ideas-like entities, given all of his arguments against “the way of ideas”. Both of these stages are stages of original perception; Reid’s distinction between original and acquired perception does not correspond to the distinction between these two stages. To develop this explanation of the mechanism of original perception, I draw on some contemporary psychological literature discussing the different stages of human visual perception and the formation of object files. Although Reid did not have access to the findings of these psychologists, I argue that this model is not only anticipated in his writings, but also that he would have accepted some of their experiments as supporting his conclusions. Moreover, this model explains how qualities of bodies and bodies are originally perceived, without this implying that the metaphysical asymmetry between bodies and their qualities is effaced.

Common Sensibles and Molyneux’s Problem

Andrei Marasoiu (University of Virginia)

Molyneux’s problem (Locke 1979; Essay II.IX.VIII) is not about sensations of sight and touch, nor about how concepts of shape apply in experience (Degenaar 1996). Rather, I propose that Molyneux’s problem is about non-conceptual perceptual representations. These are often construed as specific to sensory modalities and coordinated in thought (Martin 1992). I challenge this Berkeleyan assumption by advocating a common-sensible approach to Molyneux’s problem. I argue Locke could have endorsed it, and that it should be preferred. Recognizing shapes is a prerequisite for recognizing objects (Farah 2004). Molyneux’s problem concerns representing 3D shapes, the shapes of objects as perceived (Glenney 2012). The problem is properly raised when applied to ideas, viz. conscious representations (Yolton 1968). Molyneux’s question now becomes: ‘When the newly sighted first see a cube which they can recognize by touch because they already have an idea of touch, do they also possess (beforehand or formed on the spot) an idea of sight enabling them to recognize the cube visually?’ Answering this question presupposes the resolution of a debate about the nature of the senses (MacPherson 2011): believing that common sensibles exist runs counter to endorsing a heterogeneity of the senses. Berkeley persuaded many (e.g., Evans 1985, Martin 1992) that the senses are heterogeneous. If Berkeley is right, recognizing a cube may only occur when distinct ideas of sight and touch are identified in a judgment, or brought together under an amodal concept, or visual shape inferred by analogy with haptic shape. Thinking needs to intervene if shape-recognition is to occur. Berkeleyan heterogeneity implies then perceptual shape-recognition is cognitively penetrated (Brandt Bolton 1994, Raftopoulos and Müller 2006), an epistemologically counterintuitive view (Siegel 2012, Huemer 2013). In contrast, Locke believed ideas of shape are “simple ideas of divers senses” (Essay II.V). I suggest Locke is best interpreted as construing ideas of shape as common sensibles, perceptual representations which are crossmodal, not modally-specific: the same visuotactile representation is conscious (i.e., an idea) when recognizing a cube by sight as when recognizing it by touch. The newly sighted may recognize a cube visually only if she will have already formed a visuotactile representation of cube by touch. The common-sensibles and the heterogeneity traditions conflict (Muehlmann 2008). A choice must be made if answers to Molyneux’s problem are to be
justified (Jacomuzzi et al. 2003). Current neuropsychological evidence remains neutral between them (Damasio 1989, Treisman 1998, Farah 2004). Notwithstanding, we need to choose because foundational conceptions about the senses inform any empirical research which seeks to support them (Heil 1987). My argument for the common-sensibles tradition is that, unlike Berkeleyan heterogeneity, it doesn't need any top-down penetration of perception by cognition to account for shape-recognition. Since the common-sensibles approach to Molyneux's problem is more parsimonious (Sober 1975), it follows it should be preferred to Berkeleyan heterogeneity.

Thursday - 23 June (Parallel Session II)

Parallel Session II (11:00-12:30)

2.1 Symposium: After Newton and Leibniz: Kantian Foundations for Early Classical Physics

Kant was well placed to reflect on the philosophical footing of early classical physics, which by his time had exited its 17th-century infancy. Exegetes have long taken that physics to be a metaphysically unmoored, free-floating theory often called ‘Newtonian science.’ In his recent Kant’s Construction of Nature, Michael Friedman argued compellingly that Kant sought to reconcile a specific theory, viz. Newton’s gravitational dynamics, with a broadly Leibnizian metaphysics of composite substance. Kant thereby hoped to justify philosophically the application, qua exact science, of mathematics to nature. Our panel sets out from Friedman’s construal while aiming to extend and correct his agenda. We see the panel as making a dual contribution, historiographic and systematic, to the philosophy of early modern science. Historiographically, our papers contextualize better and refine our image of Kant as a philosopher of science. We do so by uncovering predecessors for his reconciliation project, and by broadening the range of his engagement with Newton and Leibniz. Katherine Dunlop’s paper compares Kant’s synthesis with Emilie du Châtelet’s earlier attempt to ground a broadly Newtonian science of motion in a metaphysics of Leibnizian extraction. Bennett McNulty’s paper explores the constraining role that a Leibnizian metaphysical principle, the Law of Continuity, played in Kant’s theory of matter and his rejection of hard-particle ontologies. Marius Stan’s paper then seeks to discern Kant’s place in the Newton-Leibniz controversy on whether the metric structure and equable flow of time are ‘absolute’ or are reducible to facts about material processes. On the systematic side, our panel follows Kant by engaging with truly central is- sues of early classical physics: the causal role and mathematical expression of force, the content and role of metaphysical principles in dynamics, and the nature of time. In particular, Katherine Dunlop’s paper asks whether Kant could hold a Newtonian concept of inertia and also a Leibnizian notion of moving force. Bennett McNulty’s paper uncovers the metaphysical premises behind Kant’s polemical denial of ‘absolute im- penetrability,’ or rigid bodies. Marius Stan’s paper examines whether Kant’s grounding of temporal metric is transcendental or metaphysical. We expect our panel contributions to yield collectively a sharper view of the architecture behind Kant’s natural philosophy; and an enriched picture of philosophy’s dialogue with exact science after Newton and Leibniz.
Reconciling Leibniz and Newton: Kant and du Châtelet

• Katherine Dunlop (University of Texas, Austin)

In this talk, I take for granted that both Kant, in Metaphysical Foundations of Natural Science (1786), and Émilie du Châtelet, in Institutions Physiques (1742), seek to base a broadly-Newtonian theory of motion on a broadly-Leibnizian metaphysics. A comparison of their works brings to light the different ways such a project can be pursued. Of particular interest is du Châtelet's espousal of both a Newtonian view of inertia and a Leibnizian conception of moving force (as either “living” or “dead”). This combination is surprising because Leibniz conceives moving forces as causes of motion, while it is distinctive of Newton's view that only changes of motion require an active force as cause. Indeed, in Kant's Construction of Nature (2013) Michael Friedman takes Kant's Newtonian conception of inertia as a sufficient basis to deny Leibnizian moving forces any place in his view. Yet du Châtelet's understanding of force — namely, as a ground of action — seems sufficiently broad to accommodate the Newtonian view (that force causes change of motion). So to the extent that Kant shares this understanding, he could also countenance Leibnizian moving forces. But I am less concerned to ascribe these views to Kant than to explore their joint tenability.

Continuity of Change in Kant's Dynamics

• Bennett McNulty (University of Central Arkansas)

Following an anonymous review of Kant's Metaphysische Anfangsgründe der Naturwissenschaft (MAN) in the Göttingische Anzeigen von gelehrten Sachen in 1786, the first proposition of the book's “Dynamics” chapter has been controversial. According to this proposition, matter fills space—resists the penetration of other matter—through its expression of a moving force. This claim is the centerpiece of Kant's dynamical theory of matter and his first salvo against the mathematical-mechanical theory of matter, according to which matter is essentially impenetrable and fills its space through its “mere existence.” The reviewer complains that Kant's thesis lacks reasonable justification, an objection repeated forcefully by Kenneth Westphal. According to Westphal, the failure of MAN to ground adequately Kant's dynamical theory of matter later motivated the project undertaken in his Opus postumum. In this paper, I reveal the implicit, metaphysical premise behind Kant's dynamism overlooked by the reviewer and Westphal, namely, the Leibnizian Law of Continuity. My interpretation emerges from that of Michael Friedman, who maintains that Kant's objection to mechanical philosophy rests on the theory's mathematical deficiencies. According to mechanical philosophy, matter fills space through its mere existence, and therefore matter instantaneously changes its motion when encroaching upon another's space. Yet in the Phoronomy of MAN Kant presents his method of mathematically constructing of motions, which represents all changes to motion as gradual and continuous. According to Friedman, mechanical philosophy is thus mathematically inadequate insofar as it depicts matter's filling of its space as involving an immediate change of motion. Kant accordingly endorses dynamism, which holds that bodies are elastic and gradually change motion via the impression of forces. Although Friedman's account rightly emphasizes the continuity of change as the backbone of Kant's arguments for dynamism and against mechanism, in this paper I explain that the nature of his criticism is not mathematical, but metaphysical. As Kant explains in the General Remark to the Dynamics, mechanical philosophy is mathematically adequate but succumbs to its metaphysical inadequacy. In particular, I explain that such instantaneous changes violate the Law of Continuity, which Leibniz marshalled against Cartesian mechanics and which Kant explicitly endorses in his Kritik der reinen Vernunft (KrV) and MAN. According to the Law of Continuity, changes in nature occur never immediately but only progressively through degrees. Kant maintains that a version of this principle is validated by the general metaphysics of nature described in the Analytic of Principles of his KrV, and
hence the mechanists’ account of the filling of space is metaphysically defective. This paper therefore exposes the implicit, metaphysical principles behind Kant’s argument for his dynamism that were neglected by the anonymous reviewer and Westphal. Furthermore, my argument supplements recent scholarship that has emphasized the Leibnizian aspects of Kant’s account of natural science.

**Absolute Time from Kant’s Metaphysics**

- Marius Stan (Boston College)

Kant denied Newton’s absolute time, but gave no substitute for it, though he did replace the Briton’s absolute space. In fact, a substitute for absolute time is sine qua non for Kant. He needs it indispensably in natural philosophy – as basis for chronometry and thus mathematical physics – and also to make good on the schematism of his quantity-categories. In modern terms, Kant needs to show that time has a metric, or congruence structure; and to explain whether the metric is intrinsic or extrinsic. In this paper, I solve Kant’s problem, from his resources. To do so, first I distinguish (in §2) topological from metric structure, and two kinds of (chrono-)metric properties, viz. (temporal) distance and length. Then I explain (in §3) two ways for time’s metric to be extrinsic: by being grounded in category-directed acts of the understanding (U-extrinsic); or in some fact about material behavior (M-extrinsic). I argue (in §4) that all the structure intrinsic to Kant-time as ‘sensible form’ is topological, not metric; that U-extrinsic grounding is not an option for Kant because time is continuous, hence uncountably dense; and that one M-extrinsic metric (due to Michael Friedman) has no actual facts to ground it. Then, in §5, I give an M-extrinsic temporal metric that meets Kant’s needs and constraints. I ground chronometry in a nomic fact from Kant’s metaphysics of matter; I call it Kant’s Area Law, KAL. The law says that, relative to the mass-center W of the physical universe, any volume of matter sweeps equal areas in equal times. Thereby, the metric relations (ratios) between such areas suffice to induce a congruence structure on time – viz. an equivalence class of equal-length time intervals. And, KAL is universal, necessary, and synthetic a priori, or so I argue. So, it is an acceptable replacement for Newton’s absolute time. It is also consilient with philosophical strategies, from Aristotle onward, of grounding chronometry in features of material change. I conclude that Kant seems able to withstand Newton’s other great challenge – namely, to ground inertial dynamics without resort to immaterial, ‘absolute’ time. (In contrast, it is a mystery whether Newton’s other early-modern opponents can do so.) Second, Transcendental Idealism appears incomplete: it cannot ground chronometry – and durative aspects of objective experience more broadly – from internal resources. Rather, it needs help from a non-trivial metaphysics of matter qua essentially endowed with action-at-a-distance, inverse-square force of attraction.

2.2 Symposium: What Drives Philosophical Progress?

The history of philosophy is often presented as a narrative driven by a dialectic regarding the fundamental nature of first-person knowledge. Insofar as the debate progresses, it is understood in terms of a dialogue between philosophers concerned with those issues. In this symposium, we ask whether the driving force in philosophical progress is captured by an epistemological focus in this sense, and thereby develop new accounts of what philosophical progress might be, what drives it, and how we should understand the history of early modern thought. The alternative narratives offered in this symposium provide a picture in which scientific achievement and technological innovation play a much larger role than on the traditional narrative. Traditionally, philosophers from the early modern period have been divided into Rationalists and Empiricists and this distinction (hereafter the ‘RED’) has dominated study of the early modern period.
Since the late twentieth century, however, it has increasingly come under attack. Critics of the RED hold that the distinction introduces three biases into early modern scholarship: The epistemological bias. Histories of early modern philosophy based on the RED tend to overemphasise the role of epistemological commitment in the central doctrines, developments and disputes of early modern philosophers. The Kantian bias. Histories of early modern philosophy based on the RED tend to overemphasise the lack of common ground between the rationalists and the empiricists, and Kant’s role in drawing the early modern period to a close. The classificatory bias. Histories of early modern philosophy tend to overemphasise the extent to which all or most early modern philosophers can be classified as either empiricists or rationalists. This has led to some unconvincing classifications and a failure to recognise the extent to which ‘rationalists’ were influenced by ‘empiricists’ and vice versa. This symposium explores the idea that narratives that focus on the RED overemphasise the role of epistemology (that is, a priori analysis of the nature of knowledge from a first-person perspective) in the development of early modern thought. One consequence of the epistemic bias is that the traditional narrative has focused on the contributions of the canonical seven—Descartes, Leibniz, Spinoza, Locke, Berkeley, Hume and Kant—largely ignoring the contributions of women and other non-canonical philosophers. The papers in this symposium offer to redress the situation by offering new frameworks and narratives which bring into focus figures whose contributions have been marginalised by the traditional framework, such as Sergeant, Cavendish, More, Browne, Du Châtelet, and even ‘scientists’ such as Boyle and Hooke. The first two papers contrast the traditional, epistemology-driven framework with alternative, methodology-driven, frameworks generating different narratives for the history of philosophy. The authors argue that these new narratives bring different figures, ideas, and arguments into focus, while also assisting our understanding of already-canonical philosophers such as Locke and Berkeley. The final paper addresses the meta-question of whether it makes sense, from a contextualist point of view, to talk about progress in the history of philosophy. The main case study of the paper is offered as a thought experiment: what if we read the history of the philosophy of science from the perspective of Margaret Cavendish instead of Kant? The author argues that progress is achieved on two counts: firstly, by her own lights, the history of philosophy of science was progressive; secondly, from our 21st-century standpoint, it is progressive to generate new narratives and examine the history of science from a different perspective. Such narratives are revelatory. By shifting the focus away from the canon, we start to see that foundational questions of epistemology are not the only driving forces of development of early modern thought.

Epistemology, Methodology and Philosophical Progress

• Kirsten Walsh (University of Bucharest)

Frameworks play an important role in the development of philosophy. They help decide the scope and direction of research, setting the terms for the history of philosophy. They also influence pedagogy: how and what we teach. Finally, they play a role in how we, as contemporary philosophers, see ourselves and our field’s development. Take the rationalist–empiricist distinction (hereafter ‘RED’), for example. The RED’s historical narrative has played a key role in setting the scope and direction of research in the history of ideas, the teaching of early modern philosophy, and even in the contemporary philosophical emphasis on ‘core’ epistemology. Recently, the ‘Otago School’ has offered an alternative framework: the distinction between Experimental and Speculative Philosophy (hereafter the ‘ESD’). To put it very briefly, speculative philosophy states that natural phenomena can be explained without recourse to systematic observation and experiment, and encourages the use of hypotheses and conjectures in the construction of metaphysical systems. In contrast, experimental philosophy states that natural phenomena can only be explained after observations have been collected and ordered—thus, experiment plays a vital foundational
role in natural philosophy. These are not simply new labels for old categories. Where the RED is an epistemological distinction focusing on knowledge’s ultimate source and justification, the ESD is methodological and explanatory: it asks how we go about generating knowledge and explaining natural phenomena (and, for a number of 18th-century philosophers, moral phenomena). The differences between these frameworks become clearer when we ask, what was the main driver of change in early modern philosophy? Insofar as the RED gives us an account of what mattered in early modern philosophy, it generates stories about foundational, a priori investigation into the nature of knowledge. In contrast, the ESD tells stories of philosophical progress driven by scientific achievement, technological development and methodological innovation. These are two very different narratives about the history of ideas. Moreover, they emphasise the contributions of different historical figures, the RED emphasising the canonical seven—Descartes, Leibniz, Spinoza, Locke, Berkeley, Hume and Kant—and the ESD emphasising figures such as Boyle, Cavendish, Hooke and Newton. On the RED, scientific advancement is at best a side-show, on the ESD it is (to misquote Newton) the main business of philosophy. Presumably it is a mistake to think that any one factor has played a privileged role in shaping intellectual history. Thus, arguing that the history of philosophy is methodology-driven, rather than epistemology-driven, creates far too stark a dichotomy. However, drawing out this distinction between the RED and the ESD generates new hypotheses about the history of philosophy. Here, I’ll explore one such hypothesis: that the scientific achievements of Newton’s Principia were a major driving force behind 18th-century developments in philosophy. Support for this hypothesis can be seen in the developments of Locke’s views on epistemology after the first edition of his Essay, many of which appear to be connected to his reading of the first edition of the Principia. Moreover, much of 18th-century philosophy can be seen as an attempt to deal with the epistemological, metaphysical, theological and even moral questions raised by Newton’s theory of universal gravitation and the Opticks queries.

Modernity and the History of Philosophy (1613–1779)

Kenneth Pearce (Valparaiso University)

In this paper I propose an alternative narrative for the history of 17th and 18th century philosophy. The aim is not to unseat the traditional narrative but to show how a different narrative lens can bring to light important and interesting points that are obscured by the standard narrative. Thus the methodology I here wish to defend is narrative pluralism: the best way to gain a thorough understanding of the history of philosophy in a given period is by looking at it through a variety of narrative lenses. One problem with the standard narrative is that it treats ‘early modern philosophy’ as a historical period, and therefore regards all philosophers writing between Descartes and Kant as ‘modern’. The rationalist-empiricist dispute is then seen as the key dispute within this period. However, examining the actual texts of the period, one finds something curious: by the middle of the 17th century, the phrase “modern philosophers” was in use to designate what was regarded within the period as a particular sect or school of philosophers. Other philosophers in the period, such as John Sergeant, write in conscious opposition to the “modern philosophers.” The description of John Sergeant and others like him as ‘early modern philosophers’ is thus extremely problematic. From Sergeant’s perspective, the standard rationalist-empiricist dispute is an internal dispute among philosophers whose views are really very similar. In just the same way, other thinkers, such as Edward Stillingfleet and Peter Browne, lump together Descartes and Locke among the “moderns,” failing to observe the (allegedly) all-important rationalist-empiricist distinction. My alternative narrative, designed to avoid these problems, proceeds as follows. In the Medieval period, a broadly Aristotelian synthesis was achieved which was seen as supporting certain important doctrines seen as necessary for religious orthodoxy, moral responsibility, and the social and political order. During the
Scientific Revolution, new methodologies for the study of nature began to undermine the Aristotelian foundations of the old synthesis leading to a major intellectual crisis that was seen as potentially disastrous for the European social, political, and religious order. Philosophers writing within this crisis can be seen as belonging to three broad categories: anti-moderns like John Sergeant and Peter Browne who seek to restore the old synthesis; moderates like Boyle, Leibniz, and Berkeley who seek to construct a new synthesis that will preserve the most important traditional beliefs within the framework of modernity; and radicals like Hobbes, Spinoza, Toland, and Hume who seek to overthrow the old world picture entirely. In section 1, I describe this distinction in more detail. In section 2, I give a thumbnail sketch of the history of philosophy from Galileo’s Letter to Castelli (1613) to Hume’s Dialogues on Natural Religion (1779) to show how replacing the rationalist-empiricist distinction with the anti-modern- moderate-radical distinction brings different figures, ideas, and arguments into focus. Finally, in section 3, I look more specifically, by way of example, at Berkeley to show how this alternative historiography can also assist our understanding of an already-canonical philosopher.

Progress, the Microscope, and Utopia in Margaret Cavendish

Michael Deckard (Lenoir-Rhyne University & University of Bucharest)

Every history of the seventeenth century entails a narrative, that is, a certain way of telling the story, which presumes including some actors and excluding others. And, depending on which actors are used, including some of their texts and excluding others. This paper concerns narratives in the history of philosophy of science. Examining the case of Margaret Cavendish, it asks why certain historiographical choices are made, how they can be updated, and whether progress can be made in the history of philosophy. When contemporary theories of philosophy of science affect or pose questions to the past, a new paradigm for how we read the past emerges. In A Nice Derangement of Epistemes, John Zammito writes, “Philosophy of science, if not an ancient branch of philosophy, has a discernible tradition that goes back at least to the nineteenth century and, arguably, to the seventeenth. Especially starting with Kant, philosophy of science played an important role in the self-conception of philosophy as a discipline: if it could no longer be the “queen of the faculties,” it could at least be the rational arbiter of scientific achievement. By contrast, the history of science was still a very young field even when Kuhn published Structure, and it had not fully established its disciplinary home.” Zammito considers Kant the Archimedean point for philosophy of science, and Kuhn for history of science. While these are not outlandish claims, if one were to change the Archimedean point to Newton or, as a thought experiment, Margaret Cavendish, for the philosophy of science, then one’s lens for viewing the past, and interpretation of methodology, entirely changes. Cavendish has been systematically excluded from narratives of seventeenth-century science. Consider, for example, David Wootton’s The Invention of Science: A New History of the Scientific Revolution (2015). Wootton asks whether the scientific revolution brought about a new way of thinking, and suggests that Boyle uses the hunt as a metaphor for progress. However, Cavendish’s The Blazing World, which tells a story of utopia and offers its own account of philosophical progress, is not mentioned. She is even excluded from more localised accounts, such as Laura Snyder’s Eye of the Beholder, Jutta Schickore’s The Microscope and the Eye, and Lorraine Daston and Peter Galison’s Objectivity, which concern the influence of the microscope on early modern science. The latter exclusion is particularly striking, given that Cavendish engaged with Hooke on the use of the microscope to learn about the natural world. Through these discussions of the microscope, Cavendish offered an account of progress and the aims of science. The narrative of early modern philosophy and science drastically changes if looked at from the perspective of someone like Cavendish, which raises several questions. Why is Cavendish
excluded? What would it mean to read her into this story? And finally, would such an updated narrative constitute progress?

2.3 Contributed Papers: History and Laws in Hempel and Kuhn

**HOPOA: Hempel's Impacts on Archaeological Theory and Practice**

- William Krieger (University of Rhode Island)

In works ranging from “Studies in the Logic of Explanation” to “The Function of General Laws in History,” Carl Hempel argued for both the ability and necessity for the so-called social sciences to follow the same rules as the physical sciences. He begins with history, arguing that general laws, necessary for Hempelian explanations, not only can exist in History, but that they are indispensable to the proper understanding (explanation) of the past. Seeing the purported successes of Hempel's methods, North American archaeologists made Hempelian explanation the backbone of the new archaeology. Hempel's impacts on archaeology were as wide-ranging as they were controversial (to archaeologists and to contemporary philosophers of science). First, Hempel's work was used to argue for the abandonment of culture history (new archaeology's theoretical predecessor), answering pre-existing calls for archaeology to become properly scientific by founding archaeology on general laws. Second, as a consequence of this new philosophical foundation, the new archaeology would require that archaeological practices must change. As a 'new' science, archaeology would need to concentrate on gathering massive amounts of data in order to create general laws if it had any hope to successfully create explanations and predictions, in line with archaeologists’ beliefs about the successes of the established sciences. This focus on data collection would lead different types of archaeology in different (and at times contradictory) directions, with little understanding of the original theoretical rationale guiding the changing science. In this paper, I will explore this episode in HOPOA (the History of Philosophy of Archaeology), focusing on the incorporation, interpretation, and continuing implications of Hempel's work on this old discipline and nascent science.

**Historicizing Historical Explanation: the Origins of Hempel's Covering Law Model**

- Fons Dewulf (Ghent University)

In this paper I shed light on the historical origins of the quest for a model of scientific explanation. Salmon's classic overview of the literature on explanation introduces Hempel's and Oppenheim's 1948 paper “The Logic of Scientific Explanation” as the first important contribution to the debate. The covering law model, however, was already introduced by Hempel in his 1942 paper “The Function of General Laws in History”. By comparing the framework and the goal of these two papers I aim to uncover an important aspect of how explanation became a central, independent topic in philosophy of science. Specifically, I argue that the first introduction of the covering law model in the 1942 paper was not meant as a model of explanation, because Hempel in that paper did not conceive of explanation as an independent aspect of scientific inquiry. I claim that Hempel introduced the model in his 1942 paper as a novel way to defend the unity of science against German historicists. Only in its second appearance of the 1948 paper, was it framed as a model of a fundamental, autonomous aspect of scientific inquiry, namely explanation. By distinguishing these two historical phases, it becomes clear that explanation has not always been a topic for philosophers of science. The topic is the result of an intellectual shift from Germany to the United States that has also characterized many other aspects of philosophy of science as a discipline. My argument centers around the claim that the 1942 paper was aimed against a trend in German philosophy to logically distinguish between the natural sciences and the historical sciences. This is not an obvious
reading of Hempel's paper, since he never mentions this German tradition explicitly. In order to recognize how Hempel exactly reacted against this tradition, I introduce three German philosophers who treated the conceptual and methodological specificity of the historical sciences: Wilhelm Windelband, Heinrich Rickert and Wilhelm Dilthey. Next, I show that Hempel's introduction of the covering law model critically engages with several ideas of these authors. Hempel's goal is to defend the unity of science, using a new logical tool, the covering law model of explanation that is purportedly shared by all sciences. As a confirmation of my reading, I introduce the first reaction against it, written in 1943 by Paul Oskar Kristeller. Kristeller was trained by Rickert and Heidegger in the German historicist tradition. In his reaction Kristeller rephrases some of the central arguments from the tradition to withhold any lawful generalization from the historical sciences. Kristeller thus understands Hempel's contribution as an attack on the German historicist tradition. In the 1948 paper Hempel does not pick up on this debate any more, neither is the unity of science mentioned. Instead, the covering law model is framed as a model of one of the foremost objectives of all rational inquiry, namely to know why something occurs. It is this last framework of the model that has shaped the specifically Hempelean tradition within philosophy of science.

The Search for Kuhn-loss: A New Strategy for HPS

• Jamie Shaw (Western University)

The notion of ‘Kuhn-loss’, or the loss of puzzle-solving ability in a successive paradigm, has received extremely little attention in the secondary literature (cf. Chang 2012). This is surprising since it is one of the key points against the thesis that the history of science is linearly cumulative. My paper makes three contributions in this area: (i) I articulate a clearer conception of Kuhn-loss, (ii) demonstrate its theoretical and practical importance using two historical examples, and (iii) show the advantages the search for Kuhn-loss possesses over other strategies in HPS. Kuhn-loss is an extremely ambiguous notion. For instance, it is unclear whether Kuhn-loss means successive paradigms are initially or permanently unable to solve the puzzles of its predecessor. Kuhn's example of the recovery of phlogiston theory's explanation of the qualities of chemical substances suggests that Kuhn-loss can be recovered by succeeding paradigms but not that it is always recovered. This provides a distinction between recovered and unrecovered Kuhn-loss. Additionally, while Kuhn thought puzzle-solving was the primary virtue of a paradigm (and a marker of progress (cf. Laudan 1978)), it is unclear why we should conceive of Kuhn-loss in these terms rather than other epistemic virtues (e.g., explanation, prediction, etc.). Some puzzles are not worth solving from a contemporary standpoint (i.e. what the balance of the four humors is in a patient). Because of this, I argue that we should reformulate Kuhn-loss with other epistemic virtues to give us a notion of Kuhn-loss that is worth regaining. Several historical examples help develop the notion of Kuhn-loss and its contemporary importance. For example, consider the revival of the cosmological constant which was introduced to relieve the tension between Einstein's view of gravity and the static universe model, but has since been successfully revived in the study of dark matter. Or, consider Priestley's electrochemistry which was theoretically reconstituted in Lavoisier's combustion theory to explain its explanatory power (Chang 2012). These examples elucidate how regaining Kuhn-loss has been historically fruitful and contributed to the development of science. Thus, we have strong inductive reasons to continue this activity. For (ii), Kuhn-loss not only has important implications for how we conceive of the history of science, (i.e. as non-linearly progressive), but also suggests a new avenue for engaging with science. This provides a new set of tasks for historians and philosophers: to find instances of genuine Kuhn-loss, recover them, and apply them to contemporary frameworks. I go on to make sense of this task and motivate its importance within Feyerabend's account of pluralism (1970, 1975, and 1978). For (iii), I argue the search for genuine Kuhn-loss provides a better way of conceiving of the relationship between the
history and philosophy of science. Rather than using historical examples to confirm or disconfirm philosophical theories (Laudan 1981; Worrall 1989; Psillos 1999) or using historical examples to illustrate philosophical theories (Heidelberg and Stadler 2001 and DeWitt 2011), the search for Kuhn-loss provides a method for directly engaging with scientific practices and aiding in the development of theories.

2.4 Contributed Papers: Chaos, Geometry, and Objectivity

*Chaos Regained: On the Possibility of a New Era of Orbital Dynamics*

Isaac Wilhelm (Rutgers University)

I explore how the nature, scope, and limits of the knowledge obtained in orbital dynamics—the science concerned with the motions of bodies in the solar system—has changed in the past century. Innovations in the design of spacecraft trajectories, as well as in astronomy, have led to a new hybrid of theory and experiment, and suggest that the kind of knowledge achieved in orbital dynamics today is dramatically different from the knowledge achieved prior to these innovations. Thus, orbital dynamics may have entered a new era. I begin with some historical background, focusing in particular on what Neugebauer demarcated the Newtonian era (the period following Newton’s Principia). That era featured an extremely powerful methodology for constructing successively better theories of the motions of celestial bodies. Idealized theories (of the Moon’s motion, say) were refined by comparing calculations based on them with observations, and using systematic discrepancies to identify forces that made a difference to the motions but that the idealizations had left out. Those forces were incorporated into the idealization and the process was repeated, resulting in ever tighter agreement between theory and observation. When Poincaré discovered mathematical chaos in 1890, he exposed a shortcoming in this methodology: if the dynamics generated by the gravitational field are chaotic, then the exact trajectories of bodies are infinitely sensitive to initial conditions. In such cases, because the initial conditions cannot be known exactly, there will always be irreducible discrepancies between calculations and observations even if all the relevant forces are accounted for in the idealized theory. The discovery of the possible existence of empirical chaos—chaotic dynamical systems in the empirical world—unearthed a number of philosophical and evidential problems. Poincaré observed that irreducible discrepancies may render long-term prediction impossible. Hadamard and Duhem observed that such discrepancies may render certain empirical questions “ill-posed” or “meaningless”. In this presentation, I discuss two interrelated ways that orbital dynamics has adapted to the problems posed by chaos. The first adaptation involves a shift of focus: rather than describe dynamical systems in terms of forces acting on bodies, astronomers now use gravitational fields. The second adaptation came in the form of two new methodologies for conducting empirical research: one in astronomy and one in the design of spacecraft trajectories. Together, these two methodologies may eventually overcome the problems that chaos poses. The astronomy methodology can be used to identify empirical chaotic systems and to measure how ‘chaotic’ they are. The trajectory methodology can be used to interact directly with chaotic regions of the solar system’s gravitational field exploiting chaos in order to transfer spacecraft (like the Hiten) from one orbit to another while expending almost no fuel. Because of these adaptations, the kind of knowledge of orbital motion that can be achieved has changed. The development of contemporary orbital dynamics thus provides an excellent case study of how science evolves in response to philosophical and empirical problems. As I show, that evolution suggests that we may be in a new era of orbital dynamics.
Impurity of Methods: Finite Geometry in the Early Twentieth Century

• Douglas Marshall (Carleton College)

Purity of methods is often regarded either as an ideal of mathematical demonstration or even as a prerequisite for any demonstration to count as scientific. At a general level, we may characterize a demand for purity of methods in mathematics as a demand that the demonstration of a mathematical proposition should not employ concepts foreign to that proposition's content. Such a demand is made in the Posterior Analytics, where Aristotle individuates sciences (including arithmetic and geometry) by the kind they study and argues that there can be no demonstrations which “cross kinds”, i.e., employ predicates that do not belong to the kind under investigation. More recent proponents of purity allow for the existence of impure demonstrations but emphasize their shortcomings: e.g., they do not give objective grounds for the truth of the theorem (Bolzano 1817); they do not provide stable solutions to the problems being studied (Detlefsen & Arana 2011). My talk aims to assess the reasonableness of various demands for purity of methods in mathematics by means of a case study, namely the development of finite geometries in the early twentieth century. Work done in the foundations of algebra from 1900-1910 paved the way for corresponding advances in finite geometry. In particular, results by Dickson (1905) and Maclagen-Wedderburn (1905) on finite division rings allowed for the proof of the following Projective Theorem (Veblen and Bussey, 1906): Every finite projective plane that has Desargues’ property also has Pappus's. Artin (1957) and others note that no purely geometric proof of the Projective Theorem is known, and this is despite efforts to produce one (Tecklenberg 1987). The case is therefore of interest in that mathematicians have sought a pure proof, though it appears that the production of a pure proof has been infeasible. A principal result of the case study is to show that certain demands for purity in mathematics are unreasonable. First, it is unreasonable to demand a pure proof of the Projective Theorem on the grounds that otherwise the result is not demonstrated (scientifically or otherwise). Second, it would be unreasonable to insist that finite geometry should be reformed so as to purge it of algebraic concepts, as for example Bolzano attempted to purge analysis of geometry. Rather, finite geometry as it develops in the twentieth century is itself “an interplay of combinatorial, geometric, and algebraic ideas” (Dembowski 1968). The case study also highlights the ways in which impure proofs have characteristic epistemic advantages: they set up correspondences between the concepts of distinct branches of mathematics that can be systematically exploited; they make possible novel contents of thought; they allow for the confirmation of a mathematical result by multiple independent means (Dawson 2006).

Redrawing Mathematical Objectivity: Jean Cavaillès and Gilles-Gaston Granger; From the Philosophy of the Concept to a Philosophy of Style

• Jean-Paul Cauvin (University of Pennsylvania)

Jean Cavaillès (1903-1944) and Gilles-Gaston Granger (1920-) are key representatives of a novel epistemological program which emerged in Twentieth century France and which continues to transform the boundaries of epistemology and the philosophy of science. Cavaillès is often cited as a key figure in the tradition of historical epistemology; Granger, in his own vernacular, has characterized his research program over the last sixty odd years as a “comparative epistemology.” This paper examines the relationship between Cavaillès and Granger relative to the problem of constructing an adequate characterization of mathematical objectivity. The relationship between these two figures, as I shall argue, is not simply a matter of discipleship – although Granger is incontestably one of Cavaillès’ most influential interpreters (alongside Gaston Bachelard [1884-1962], Georges Canguilhem [1904-1995] and Jules Vuillemin [1920- 2001]) his characterization of Cavaillès is not simply one among others; it has determined the set of conversations proceeding from Cavaillès’ work to such an extent that it has proven impossible
Thus far) to give an account of Cavaillès’ critique of mathematical objectivity without passing through the interpretive grid of Granger’s own ambitious philosophical project. Accordingly, this paper has three interrelated goals: 1) To determine (in part) the range of epistemological problems Cavaillès and Granger are able to construct on the foundation of a shared locus classicus of epistemological reflection; namely, the nature of mathematical objectivity. 2) To identify a privileged network of encounters with Cavaillès in two of Granger’s most significant philosophical texts, Essai d’une philosophie du style (1968) and Pour la connaissance philosophique (1988). 3) To attempt a provisional differentiation of the significance of the objectivity of mathematical concepts for Cavaillès and Granger by an elucidation of what the former means by a “philosophy of the concept” and what the latter intends by way of a new “philosophy of style” as an epistemological program cutting across both the natural and the human sciences.

Thursday - 23 June (Parallel Session III)

Parallel Session III (14:00-16:00)

3.1 Contributed Papers: Newton

What Newton Meant by ‘True and Absolute Motion’

- Robert Rynasiewicz (Johns Hopkins University)

In the Principia’s Scholium to the Definitions, Newton speaks sometimes of true motion, sometimes of absolute motion, sometimes of true and absolute motion. The traditional view was that by ‘true and absolute motion’ Newton means, as a matter of stipulative definition, motion with respect to absolute space; and that he uses the rotating bucket experiment and the example of the globes to argue for the existence of absolute motion. Rynasiewicz (1995a, 1995b) showed that the bucket and the globes serve different, distinct purposes. The bucket argument is one of five—from the properties, causes and effects of motion and rest—that draw conclusions as to how true and absolute motion is appropriately defined. In contrast, the globes illustrate how, given that absolute space is unobservable, absolute motion can nonetheless be known and actually differentiated from merely apparent motion. Following Rynasiewicz, Huggett (2011) also assigns these functions to the bucket and the globes, but claims that Newton means different things by ‘true motion’ and ‘absolute motion’. Huggett follows the traditional view in taking ‘absolute motion’ to mean, by stipulation, motion with respect to absolute space, whereas ‘true motion’ is “the sense of motion implicit in the laws.” Although he levels criticism against the heterodox interpretations of Stein (1967) and of DiSalle (2002, 2006), he concludes “there is something quite accurate and very illuminating to Stein and DiSalle’s view that [Newton’s conception of true] motion . . . gets its meaning from the role of the concept in contemporary mechanics.” Contra Huggett, I argue here, from analysis of the text, as well as from the literature and traditions we can safely assume Newton was familiar with (including not just natural philosophers, such as Galileo, Descartes, Gassendi and Charleton, but also those in the astronomical tradition, such as Wing, Streete, Horrocks, Kepler, and Riccioli) that Newton does take ‘true-’ and ‘absolute motion’ to be exact synonyms. On Huggett’s hypothesis, severe incongruities arise in the text, and, as even Huggett admits, one of the arguments from the properties of motion makes no sense at all. The error derives from the traditional premise that Newton gives a stipulative definition when he introduces absolute motion as transference from one absolute place to another. Rather, this (just as Descartes definition of true motion) is what Locke would later call a real definition, whose adequacy Newton attempts to secure with uniformly intelligible arguments from properties, causes and effects. Although Huggett’s criticisms of Stein and of DiSalle remain on-target, the
conclusion that “there is something quite accurate and illuminating to Stein and DiSalle's view” loses force. One need not digest “contemporary mechanics” in order to understand what's meant by ‘true motion’ in the Scholium’s arguments. It suffices to grasp what is at stake in the Copernican-Ptolemaic debate, and that was fully comprehensible in antiquity.


Craig Fox (Western University)

Between late 1684 through mid-1685, Isaac Newton turned to developing and refining the conceptual foundations presupposed by his emerging physics of motion. Analysis of his manuscripts from this period reveal that Newton's understanding of the relativity of motion, as ultimately captured in Corollaries 5 and 6 in the Principia, led him to search for a spatiotemporally invariant quantity of matter. This search not only led him to the concept of mass but also to investigate and provisionally establish what has come to be known as the equivalence principle. Indeed, the search for invariant quantities in light of the relativity of motion led to multiple possibilities for the appropriate quantity of matter. This led to an experiment designed to discover their relationship, which then required distinguishing a new notion of force. By the end of this episode, Newton had a remarkably deep understanding of quantity of matter, force and motion. In recent times, it is common to see discussions of the distinction between inertial and gravitational mass. And, indeed, the distinction goes back at least as far as Kant, who further distinguished a third quantity of matter. What I show here is that Newton not only saw that the two are potentially distinct, but he also provisionally established their equivalence. This understanding of quantity of matter, moreover, put him in an interesting position regarding the mechanical philosophy. Since he countenanced forces abstractly, independent of the question of mechanism, his contemporaries complained that his was a theory of “occult” causes. But what has been under-appreciated is that he had established that, although it is in some sense mysterious, gravity acts in accord with the same quantity of matter as mechanical forces. The requirement, that proposed forces act in proportion to the inertial quantity of matter, is a strong condition of intelligibility. It is more than that any proposed mechanism of gravity must be compatible with the laws of motion. What it means is that any hypotheses about the causes of motion within, say, the solar system, must be compatible with the fact that, in order to not disturb the dynamics of the system, they must equally accelerate everything in it. In other words, any force that is acting on the solar system, as in, say, an invisible cause holding the Earth at rest in the center and making the sun and planets revolve around it, must be one that does not affect the dynamics so far identified by Newton. And, in order to do that, such a cause must be an accelerative force, i.e. gravity.

Newtonian Idealizations

Monica Solomon (University of Notre Dame)

In light of recent work (see, for instance, George Smith's 2014 “Closing the Loop”, William Harper's 2012 “Isaac Newton's Scientific Method”, Steffen Ducheyne's “The Main Business of Natural Philosophy”), there is growing evidence that the landscape of philosophy of science found in the Principia and Newton's writings more broadly are richer than previously thought. In this paper I argue that Newton's mathematical inferences rely on specific idealizations and models and that Newton himself emphasizes the significance of this approach for natural philosophy. To this end, I analyze two distinct types of idealization encountered in the Principia. The first type of idealization can be best described as models that help
conceptualize common phenomena invoking Newton's newly minted definitions. For example, at the beginning of the Principia Newton offers his famous thought experiments involving the bucket and the revolving globes. I show that both thought experiments involve idealizations as conceptualizations that are crucial to their purpose, this purpose being to illustrate the inferences that integrate new concepts of circular motion, force, and composition of motions. However, although these models are deliberately simplifying a complex set of motions, they do not serve the purposes of approximation. For instance, when explaining the shape of the Earth, and, in particular, its oblateness, (Proposition 19, Problem 3, Book 3), Newton uses a particular model in which Earth is considered composed of channels of water. The hypothetical situation described in the model is an idealized situation, but Newton is not interested in finding how far this idealization is from the model the Earth's internal structure. The aim of his idealization is to find from the variations in gravity on the surface of the Earth the corresponding shape, as predicted by his theory. The second type of Newtonian idealization is a novel empirical type conceived of as a series of approximations used to identify the physically significant sources of discrepancies provided by data. Here I rely on the account that has been developed by George Smith (2014). Unlike the previous cases, the hypothetical situation described by conditional statements such as “the planetary orbits would be ellipses if no other forces were at work besides the inverse-square forces directed toward the Sun.” can be used for making inferences based on the contraposed version: “if the actual orbits are not perfectly stationary Keplerian ellipses, then other forces are not at work.” In doing so, we are taking the Newtonian gravitation theory to hold exactly: these idealizations consist of “approximations that, according to the theory, would hold exactly in certain specifiable circumstances, including provisos of no further forces or density variations.” (Smith 2014, 277). By pointing out the significance of these Newtonian idealizations, I bring to light a new facet of Newton as philosopher of science. In particular, in the third section of my paper I combine my analysis with Newton's own careful wording of the role of mathematical reasoning in natural philosophy as encountered in his Optical Papers, some of the scholia in the Principia and some references in his correspondence.

The Parallelogram Rule from Pseudo-Aristotle to Newton

• David Miller (Iowa State University)

The parallelogram rule—the composite effect of two uniform and independent motions or forces is represented by the diagonal of the parallelogram formed by sides representing the components—is a distinctive feature of modern physical science. It is fundamentally important in classical mechanics; crucial, for instance, in the first proposition of Newton's Principia. Moreover, the rule encapsulates a range of assumptions about space, motion, and their theoretical representations that contradict pre-modern views. In particular, use of the rule necessarily presupposes that motions and forces can be represented mathematically as directed magnitudes, not merely scalar quantities. It also assumes that directions are parallel among themselves, indicating that motions and forces are not inherently directed toward or away from given locations. The rule is therefore intimately connected to the mathematization of nature and the rejection of teleology characteristic of the intellectual practices that became modern science. This paper traces the history of the rule from its first suggestion in the pseudo-Aristotelian Mechanica, through its rejection by the Renaissance mechanical tradition, its partial recovery by Stevin, its definitive restitution by Fermat and Hobbes in their responses to Descartes’s Optics, and its introduction into the textbook tradition leading to Newton by Rohault, Wallis, and others. Throughout, the focus is on the conceptual obstacles that prevented adoption of the rule and how they were overcome. In particular, the paper addresses the recurrent discussions of the apparent oddities that two motions or forces can sum to a magnitude that is less than their scalar combination, and that a change of direction can alter the
magnitude of a motion or force. The narrative shows how crucial developments in the fundamental understanding of mathematical representation, motions, and forces transcended disciplines from astronomy to mechanics to optics. The story also challenges the modern claim that the parallelogram rule “was well known in the 17th century … and presented no technical difficulties.”

3.2 Symposium: Hylomorphism and Extension in Late Medieval and Early Modern Natural Philosophy

The New Conception of Material Substance in the Fourteenth Century

• Henrik Lagerlund (Western University)

The fourteenth century saw some radical changes in the way substance was conceptualized. Ockham challenged the Aristotelian or Thomistic way of thinking about material substance and body as he systematically rethought metaphysics. According to Aquinas, a substance had no parts that are prior to it. A composite substance, an animal or a human being for example, comes to be out of another substance, but only what they call prime matter remains the same during this generative process and prime matter has no existence on its own. Since matter was their principle of individuation, form could have no existence before its union with matter. Hence nothing in an individual composite substance pre-exists its existence in nature. To insist, as Ockham does, that a substance is nothing but its parts is contrary to Aquinas who held that although substances have integral parts these parts depend ontologically on the whole of which they are parts. Each part of a substance is actual and not dependent on anything to make them actual, Ockham argues contrary to Aquinas. Every substantial form in nature (other than the human intellectual soul) is extended and composed of parts, according to Ockham. Hence all the properties of the whole form are going to be derivative upon its parts and furthermore all the properties of the whole composite substance will be derivative of the properties of the parts of the form and the matter. Since the integral parts of the form parallel exactly the integral parts of the matter, and since the matter is something existing in its own right, the view defended by Ockham is a fraction away from the abolition of the whole distinction between form and matter. His view appears similar to the later seventeenth century view of substance wherein the properties of the parts of the material body are all properties which have the actual material parts of the composite as their subjects. The view of material substance defended by Ockham was developed further by his followers in the nominalist or the so called via moderna tradition, that is, by thinkers like John Buridan, Albert of Saxony, Marsilius of Inghen, Nicholas Oresme and Peter of Ailly. In this talk I will look more closely at Albert of Saxony and Nicholas Oresme. They develop distinct views on matter and material substance within this new tradition. I will look at their respective commentaries on Aristotle’s Physics, particularly a number of questions in the beginning of Book I where they discuss the notion of a whole and the relation it bears to its parts. I will then compare their view with Ockham’s and Buridan’s. The aim is to present a fuller picture of this distinctly new conception of material substance.

John Buridan on the (Meta)Physics of Extension

• Francesca Bruno (Cornell University)

In this paper, I examine John Buridan’s strongly realist account of extension (magnitudo), as set forth in his question-commentary on Aristotle’s Physics, secundum ultimam lecturam (henceforth QP). Like Ockham, Buridan thinks that (i) extension is an accidental feature of matter (and material substance, more generally) in the sense that extension is a feature matter can lack (at least by divine power); and (ii) without extension, matter still has distinct parts even though such parts are not spatially extended. It is because of
extension that matter has “partem extra partem,” as the dictum goes: parts “outside of” parts or spatially contiguous parts. However, Ockham and Buridan disagree on the ontological status of extension: whereas Ockham thinks that there is no real distinction between an extended substance and its extension, Buridan denies that. To wit: Buridan holds that extension is a “real accident”: it is a genuine entity (res) really distinct from the (material) substance in which it inheres. In this paper, I examine the main argument Buridan offers in QP I.8 in support of his view, centering on condensation and rarefaction (C/R). In particular, I am interested in two questions: (i) whether Buridan's argument is successful against Ockham's reductionist account of C/R in terms of parts and (local) motion; and (ii) whether Buridan's account of C/R modeled after the intension and remission of qualities gets around Ockham's critique of quantity realism. As Ockham explains in his (expositio) commentary on Aristotle's Physics, Book IV, chapter 17, one reason for positing extension as a real accident (as defined above) is to explain how a material substance (or body) comes to be “bigger or smaller” without any addition or loss of substantial parts, namely, how a body rarefies and condenses. Ockham thinks that we can explain C/R simply in virtue of a body's parts moving closer or further away from each other, that is, in terms of the local motion of its parts. By contrast, Buridan argues that we cannot “save all phenomena” (salvare omnia) connected with C/R on this model. Rather, we must posit extension as some really distinct res just as we posit a body's color as something distinct from its substance. Interestingly, Ockham and Buridan agree that such physical qualities of a body as its color and temperature are real accidents and changes in color or temperature are to be explained as involving the generation or corruption of some (accidental) form(s) or parts thereof. They also agree that some qualitative changes, such as a change in shape, are to be explained in terms of local motion of the parts alone. However, they disagree when it comes to C/R (change in extension). It is the goal of my paper to explore the reasons underlying this key disagreement between them and draw some more general lessons about the limits of mechanical explanations of physical changes in these two authors.

Crathorn on Extension

Magali Roques (University of Geneva)

In this paper, I analyze William Crathorn's view on extension and compare it to William Ockham's reductionist view, according to which extension is not really distinct from substance or quality. William Crathorn is an English Dominican theologian known for his radicalization of Ockham's nominalism. He was also one of the very few fourteenth-century theologians to argue that continuous entities such as lines, surfaces and bodies, but also time and motion, are composed of a finite number of unextended, indivisible particles. In this, he departed from Ockham's radical anti-atomism, according to which extended magnitudes are not composed of indivisibles and indivisibles are not necessary to account for topological relations such as touching, contiguity and continuity between magnitudes. In my reading, Crathorn departs from Ockham's view of the relation between extension and impenetrability in order to defend the idea that there is a one-to-one correspondence between parts of a unified whole and the space they occupy. Indeed, Ockham thinks that two extended qualities can be in the same place at the same time, because qualities are corporeal only in a broader sense. Crathorn thinks that this idea is nonsensical: if two qualities can be in the same place at the same time, it is because their respective extension is nothing other than an incorporeal extended space which can overlap with any other such space, on the ground that space is nothing. Thus, Crathorn's finitist atomism is grounded on a view of extension which leaves room for a new conception of the structure of matter. This new conception is based on a reductionist mereology, according to which a whole is nothing other than its parts. It allows him to solve the paradox of touching that Aristotle opposed to the atomists and thus to defend the thesis that there can be spatially extended atoms contiguous to one another. The interaction between mereological concepts and
topological concepts in Crathorn's atomism has already been studied by Aurélien Robert. According to Robert, Crathorn does not limit himself to a mathematical analysis of the divisibility of the continuum, but puts forth the foundations of a genuine atomist physics. In my view, Crathorn's atomism is not necessarily to be thought of as a genuine physical explanation of the world. I believe that it is a metaphysical investigation of the principle according to which things perfectly match their location, under the assumption that things are composed of a finite number of indivisibles. To make my point, I will proceed in four parts. In the first part, I will present Crathorn’s mereology, in the second part his atomism, in the third part his theory of extension, and in the last part his view of impenetrability.

Matter, Quantity and Extension in Gorlaeus and Descartes

• Helen Hattab (University of Houston)

The Dutch Calvinist philosopher David Gorlaeus (1591-1612) was among the first early moderns to develop a systematic atomist natural philosophy. He became forever associated with Cartesianism when Descartes’ follower, Henricus Regius, publically defended the accidental union of mind and body, a heterodox doctrine attributed to Gorlaeus. Whereas Descartes publically affirmed the essential union of mind and body, his metaphysics resembles Gorlaeus' in other respects. Descartes denies Scholastic real accidents that are separable from substance and instead develops a substance/mode ontology. Gorlaeus denies all real accidents except light (it alone does not inhere in a substance) and recasts them as modes. His definition and treatment of modes is very similar to Descartes'. Both philosophers reject the prevailing Scholastic view that quantity is the first accident of matter and argue instead that there is only a conceptual distinction between quantity and the thing that has quantity and extension. Descartes' arguments are by now well-known. This paper will delve into Gorlaeus’ arguments and show that, despite reaching the same conclusions as Descartes’, the concerns they address are continuous with those that occupied late medievals. Gorlaeus' account of the relation between matter and quantity thus provides an important bridge between late medieval and early modern philosophy and science. In the Sixth Exercise of his Exercitationes Philosophicae (1620) devoted to quantity, Gorlaeus argues that quantity is not a real accident, and that it differs only conceptually from substance. His first argument is that quantity is always a unity or a whole composed of unities. Unity, an affection of being, is not distinct from being. Real being is limited to God and atoms; wholes composed of atoms are not substances but mere aggregates, or ‘accidental beings’. Gorlaeus concludes that quantity is not distinct from each atom, and there is no quantity in composites but the quantity of atoms. His second argument that quantity is substance proceeds from the premise that each body must have one corresponding extension in space. I will closely examine these arguments in connection with Gorlaeus’ account of accidents and modes. At first, Gorlaeus’ atomism appears at odds with Scholastic philosophies of the period. Nonetheless, Gorlaeus defends his atomism with theological arguments. Further, his claims about accidents and their implications for quantity are also found in a standard 17th century metaphysical text of the Dutch Calvinist Scholastic tradition. It likewise denies real accidents, argues that matter is extended per se and equates matter with quantity. These revisions of key Scholastic metaphysical concepts are designed to support the Reformed view of the Eucharist by precluding the real and physical presence of Christ in the host. This suggests that Gorlaeus, unlike Descartes, developed his atomist account of matter and its relation to quantity largely for theological reasons. Gorlaeus thus provides us with a striking example of how the need to reconcile the core Scholastic Aristotelian metaphysical concept of substance and its accidents and modes with theological doctrine contributed to the redefinition of matter as essentially extended and the very collapse of Aristotelian hylomorphism.
3.3 Contributed Papers: Mid-Twentieth-Century Philosophy of Science

Defining the Field: Early Venues for the Philosophy of Science

• David Stump (University of San Francisco)

The Monist and Revue de Métaphysique et de Morale were two journals that served as venues for the philosophy of science prior to the formal establishment of the field as a sub-discipline within philosophy, both being founded in the 1890s. Both published the work of philosophically-minded scientists as well as scientifically minded philosophers, notably Peirce and Mach among many others in The Monist, and Poincaré and Russell in both journals. They were nevertheless founded with quite different principles and in very different contexts. By studying the programmatic intentions of the founders of these journals and by comparing what they actually published to other philosophical journals at the time, we can paint a portrait of the self-understanding of philosophers of science at the turn of the 20th century and provide strong evidence that the relation of philosophy to the sciences and mathematics was taken as essential in these journals, and that there was a self-conscious attempt to maintain these relations. Although it was founded as a journal promising to publish the most competent authorities in science and philosophy, it was not until its eighth year of publication that The Monist suddenly appeared with the subtitle “a journal devoted to the philosophy of science.” The Revue de Métaphysique et de Morale, by contrast, did not advertise itself as scientific, but rather as a rationalist alternative to Comtean Positivism on the one hand and to mysticism on the other. In his introduction to the journal Philosophy of Science in 1934, Malisoff quotes many of the authors from The Monist and the Revue de Métaphysique et de Morale, when trying to define the philosophy of science for his readers. These three journals show a continuity of topics and approaches to the philosophy of science, despite the diversity within and among them, which helped to define the philosophy of science as a sub-discipline in philosophy.

Churchman and Philosophers, or Some of the Average-Sized Mortals who Founded Mid-20th-Century American Philosophy of Science

• Alan Richardson (University of British Columbia)

Despite Timothy Williamson’s recently expressed worry that the history of analytic philosophy might be being written by losers (such as Richard Rorty and Robert Brandom), in fact the disciplinary historians of analytic philosophy (such as Scott Soames) are still writing winners’ history. There are two salient and interrelated features of this sort of history: its narratives are highly individualistic (on Soames’s view there are four founding giants of analytic philosophy) and intensely epideictic—we are meant to evaluate the contributions of these geniuses for the purpose of praising their accomplishments and puzzling over their noble failures. The value for understanding what actually happened in individualistic and epideictic history is limited, especially when dealing with time periods and fields in which geniuses and their astonishing philosophical achievements are thin on the ground. So it is with mid-20th-century American philosophy of science. In this paper I wish to take us back to the post-Second-World-War period and to look at some of the organizational activities of those who strove to shape American philosophy of science: Ernest Nagel, Herbert Feigl, (briefly) Hans Reichenbach, Philipp Frank, and C. West Churchman. Since I am most interested in organizational activities that set the framework for philosophy of science at this period, my chief interest will be in the founding of centers such as Frank’s Institute for the Unity of Science in Boston and Feigl’s Center for Philosophy of Science at Minnesota and the failure of Churchman and his young colleague, Russell Ackoff, first at Penn and then at Case Institute of Technology in Cleveland to found a Center for Experimental Method. The history of philosophy of science in this time period is a history of
Beginning in the late 1960s, the fields of systems theory and cybernetics became a topic of political concern in both East and West Germany, with factions in each country viewing these fields as leading to a dystopian technocratic society. Interestingly, despite far greater funding and research in these fields provided by the cold-war powers of the U.S. and U.S.S.R, no similar debate appears to have occurred in these countries, or indeed anywhere else. In West Germany, this discussion occurred most visibly through a prominent decades-long debate between Jürgen Habermas, the most influential philosopher of post-war Germany, and Niklas Luhmann, probably its most influential sociologist. In this paper, I will address the philosophy of science that informed each thinker's position, and the consequences for the philosophy of science of this debate. The debate between Habermas and Luhmann was initiated in a book originally published under the title: Theory of Society or Social Technology -- What Does Systems Research Do? (1971) The title alone suggests why the book would become surprisingly popular and why their debate would be so sustained. Systems theory, or as it is here called systems research, is described in the title precisely as its audience saw it: as a shorthand term for social technology, or what we might call technocracy. The very high stakes of the Habermas-Luhmann debate focused on how to deal with the relation between the complexity of the modern scientific and technological world, particularly as it defined human nature, and the possibility of democracy. Luhmann argued that society and the sciences have become so complex that there is no longer any possibility for a single perspective or theory to understand them, such as had earlier existed in the Enlightenment idea of nature. Without such a foundation there was no basis for a democratic debate about many contemporary issues. Instead, Luhmann held that we need to understand the complex interaction of different institutions, forces, and worldviews; the best way of doing this is a systems theory, that is a study of the interrelation of systems, that recognizes a multiplicity of perspectives and often incommensurable sciences. Habermas argued that as useful as systems theory might occasionally be for understanding structures of knowledge and action, democracy demands at least an ideal common view of society and nature in order to assume a dialog among people about society, and to be able to fight for norms and values of behavior. Luhmann's thought, he thus contends, represents "the highest form of a technocratic mindset." Curiously, the understanding of science at the core of each thinker's argument proves to be pivotal for understanding both the stakes and outcome of the debate.

Today, it is rare for philosophers of science to interact with philosophers working in what is often perceived to be “core” areas of analytic philosophy like the philosophy of language. But this is a change from the state of professional philosophy in the fifties and sixties (and before). As seen in Richard Rorty’s
The seminal 1967 anthology *The Linguistic Turn*, "ordinary language" philosophers and philosophers of science usually sympathetic in some way to a then widely accepted understanding of the logical positivism associated with the Vienna Circle regularly engaged one another. Rorty's anthology attempts to offer a fresh look at a debate that had died down in the years prior to its publication. Rorty frames the anthology around a metaphilosophical divide between ideal language philosophers—philosophers of science like Rudolf Carnap, Gustav Bergmann, and Herbert Feigl—and ordinary language philosophers—Oxonians like Gilbert Ryle, P.F. Strawson, and J.L. Austin. This divide existed despite both camps having made the "linguistic turn": a commitment to, as Rorty understands it, "the view that philosophical problems are problems which may be solved (or dissolved) either by reforming language, or by understanding more about the language we presently use" (1967: 3). The metaphilosophical divide grew, partially, out of a debate concerning the proper unit and aims of philosophical analysis in light of the linguistic turn. Should philosophers, following the lead of Bertrand Russell, focus on articulating the underlying logical structure of our otherwise inexact natural languages with the help of constructed ideal languages? Given the remarkable successes of the physical sciences, should they extend this method to elucidating the logical structure of scientific language, as Carnap attempts? Or, assuming that ordinary language is already in good working order, should philosophers focus on, instead, understanding the different features of ordinary language, including those related to context and use that affect meaning and underwrite communication? With this as a background, this paper examines contours of the criticisms of logical positivist doctrines by ordinary language philosophers like Strawson and Austin—including those philosophers of science influenced by ordinary language approaches, e.g., Michael Scriven and Stephen Toulmin. In understanding the criticisms of logical positivism by ordinary language philosophers, we can start bridging the gap between central episodes in the history of analytic philosophy of language—e.g., Strawson's criticism of Russell's theory of denoting phrases—and central episodes in the history of philosophy of science—e.g., Scriven's pragmatic challenges to deductive-nomological accounts of scientific explanation. Given recent complications to the standard narrative about the demise of logical positivism at the hands of W.V. Quine and Thomas Kuhn, these criticisms prove worthy of attention in histories of the development of philosophy of science in the 20th century.

3.4 Contributed Papers: Revolutionary HOPOS

*Of Ballungen and Anarchism: The Influence of Otto Neurath on Paul Feyerabend*

- William Wilson (Virginia Tech)

Only in recent years has Otto Neurath’s place in the history of the philosophy of science been properly appreciated and his influence and relationship with the other members of the Vienna Circle been explored. Similarly, Feyerabend scholarship has exploded in recent years and has gone a long way towards reconstructing Feyerabend’s philosophy of science as expressed in his early, middle, and late periods, as it were. It is widely acknowledged that Neurath and Feyerabend denied any secure foundation for knowledge and argued for a pragmatic characterization of science whereby science is understood as being a socially, culturally, and politically conditioned discourse. Despite the philosophical similarities between the two, a survey of the literature reveals that there has been virtually no analysis of the possible influence of Neurath on Feyerabend. In this paper I argue that there are important conceptual affinities between the two thinkers that have hitherto gone unnoticed or underappreciated. I focus explicitly on their anti-foundationalism and attack on method. I contend that Feyerabend’s own foundationalist views were heavily influenced by what Cat and Cartwright (1996) call ‘Neurath’s general principle’, and that the common thread between their respective attacks on method is their mutual denial of meaning atomism.
Their rejection of meaning atomism arises from their mutual disdain and dismissal of the conceptual conservatism expressed by the logical empiricists. There are important limitations to this paper to consider. The first is that although I believe there to be interesting historical connections between Neurath and Feyerabend, I do not have the space to study these in detail. The second is that there are pertinent similarities in their political philosophy of science, an area in which Feyerabend was almost assuredly influenced by Neurath in some way. These are topics in the larger project of studying the two in depth; my paper is simply a preliminary exploration and sketch of an argument. I suggest that there is much more interesting work here to be done.

Flippant Anarchism: Feyerabend and the Student Movement

Matteo Collodel (MIUR)

This paper spells out, on the basis of hitherto unexplored archival documents, the context in which Feyerabend's epistemological anarchism took shape, focusing on the influence of the 1960s student movement on the development of Feyerabend's later thought as epitomized by his influential book Against Method (1975). Feyerabend’s exposure to the student protests in the 1960s was extraordinarily wide, timely and momentous. As a regular member of the faculty at Berkeley since 1958, Feyerabend could appreciate the evolution of the US student movement throughout the following decade in one of its main centres of propagation. In addition, Feyerabend taught in London (1967-70), Berlin (1968) and Yale (1969) on joint visiting professorships right at the time when the wave of protests reached Europe and the movement radicalized, turning increasingly violent on both sides of the Atlantic. The stimuli caused by the its impact on Western academic institutions and society at large occurred at a crucial stage in Feyerabend's intellectual life as he was reconsidering his allegiance to the Popperian School and striving to articulate an original theoretical position. Feyerabend's reaction was complex and the overall effects proved to be deep and lasting. Neither the bursts of student protests that repeatedly stormed the Berkeley campus between 1960 and 1968 nor the 1967 sit-in at the LSE that intensified the student struggle in the UK left significant traces in Feyerabend's published or private papers, including his autobiographical remarks, correspondence and diaries, despite the fact that Feyerabend's name ended up on FBI records and that he was close to some of the student movement's leaders. Indeed, Feyerabend apparently did not become directly involved in the movement until 1968, when he actively sympathized with the student protesters in Berlin, contributing to the critical discussion of political issues during his classes, partly on the grounds of the historically-based anti-methodological views that he was developing in the philosophy of science. It was within this strongly politicized context that Feyerabend promptly appropriated the “anarchistic” label which the radical students attached to his stance. He also became rapidly acquainted with the revolutionary literature, including the works of Marx, Trotsky, Mao, and Cohn-Bendit, with whose heterodox version of dialectical materialism Feyerabend started flirting. Moreover, he committed himself to the cause of the new left by signing the book contract that eventually resulted in Against Method. In 1969, however, as violence escalated in the confrontation between the students and police forces and as the movement split into factions, Feyerabend became scathing of the students’ strategies and of their intellectual leaders within academia, such as Marcuse. Fascinated by a major target of Marcuse’s criticism, Mill's On Liberty, Feyerabend made its tenets a cornerstone in the development of his own Dadaistic approach. The latter combined Feyerabend’s Millian understanding of pluralism as the most sensible implementation of critical thinking within a fallibilist framework with the acknowledgment of the ultimate impotence of arguments to settle disputes, the firm rejection of violence and a promotion of ridicule as an effective rhetorical means of theoretical and social change.
‘Scientific philosophy’ is a term of art, varying among philosophical schools, as well as among philosophers, regardless of how closely or distantly they might be related. Roughly speaking, it is intended to identify philosophical approaches to understanding science that, in some way or another, incorporate concepts and methods drawn from scientific activity and its products. Thus understood, it is enlightening to attempt to unpack the views of various philosophical thinkers in terms of their scientific philosophy, in terms (that is) of their understanding of the relations of their philosophical thought about science to its subject-matter – that is, science itself, particularly the science of their day. The project of my presentation is to initiate a discussion of the precise sense in which the philosophical thought of the Schlick Zirkel, the group of intellectuals who gathered around Moritz Schlick in the early Twenties, was scientific. The Zirkel members turned to science itself for the insights and ideas that illuminated the scientific enterprise rather than rely on an autonomous philosophical position, articulated independently of scientific thought and then applied to it. Thus they relied on the ideas and insights of scientific thinkers than they did on the notions that populated scientific texts. And while many of the ideas that they borrowed required extension and modification to suit their intended purposes, the result was nonetheless a radical departure from previous philosophical efforts to comprehend science. While each of the initial members of the Zirkel brought their own scientific philosophies with their own distinctive points of emphasis, they shared many broad themes. Principal among these was the positivist critique of metaphysics they learned from Ernst Mach’s writings. Certainly Hans Hahn and Otto Neurath were sympathetic to Mach’s phenomenalism, or at least the empiricist spirit it embodied. But Hahn was also deeply interested in the logicist philosophy of mathematics of Bertrand Russell and Gottlob Frege, while Neurath had drawn novel consequences from Pierre Duhem’s holism. Schlick’s scientific philosophy, developed in his Allgemeine Erkenntnislehre of 1918, was marked by a quite distinctive position on the positivism-realism debate, a dispute in which his teacher, Max Planck, had engaged Mach in the previous decade. Indeed, rather than simply accept Planck’s realism, Schlick’s scientific philosophy implied that none of the principal philosophical views then current, neither neo- Kantianism nor phenomenalism, was tenable. While Schlick affirmed the reality of the non-observable posits of advanced theoretical science (like Planck), the content of science –its concepts and claims- required a relation to intuitive experience. And these intuitive experiences were closely akin to the phenomenal ‘elements’ that populated Mach’s theory of knowledge. On this key issue and others, the scientific philosophy developed by Schlick departed significantly from the prevailing philosophical views of his day. And this conclusion demands, in turn, a re-consideration of the prevailing orthodoxy concerning the origins of analytic philosophy of science.
Thursday - 23 June (Plenary Session I)

Plenary Session I (16:30-18:00): HHH Auditorium

Karine Chemla (ERC Project SAW & SPHERE, CNRS, and Université Paris Diderot)

Generality as a Value in Various Epistemological Cultures

The talk will put forward theses with respect to generality on the basis of two key examples. One, taken from the mathematical corpus that has come down to us from ancient China, will give us the opportunity to examine different practices of generality with respect to numbers and operations. This example also raises the theoretical problem of analyzing how actors understood and practiced generality, when generality can mainly (but not only) be grasped through its effects on the mathematical knowledge presented in a certain number of writings. The second example will take us to the 19th century in Europe, and will allow us to examine how actors understood and practiced generality in geometry. These examples will illustrate how we can gain a much more detailed understanding of the meanings and values actors attached to generality in scientific practice, if we focus on epistemological cultures, in a sense that will be discussed. The local approach to generality I advocate will also allow us to show how actors’ philosophical work on generality in a specific mathematical context was read as such and adopted in another mathematical context, where it allowed practitioners to achieve major new scientific developments. Finally, in the two examples examined, generality will appear as an element in a set of epistemological values. We will outline how actors appear to have understood the relationships between these various values and also the structure of these sets of values.

Friday - 24 June (Plenary Session IV)

Parallel Session IV (9:00-10:30)

4.1 Symposium: From Powers to Forces: The Difficult Legacy of Aristotelianism in Early Modern Natural Philosophy

Two Aristotelian Responses to Mechanization: Kenelm Digby and John Sergeant on Substantial Forms

• Han Thomas Adriaenssen (University of Groningen)

Aristotelian natural philosophy conceives of the natural world as populated by entities endowed with powers, which are rooted in substantial forms, and which account for objects’ characteristic effects and for the causal connections between them. As is well known, this Aristotelian view came under attack in the first half of the seventeenth century. According to what has been for long time the standard narrative, very different authors such as Descartes, Hobbes, and Boyle attempted in different ways to dismiss the
Aristotelian idea of powers and substantial forms and build a “mechanist” natural philosophy able to account for phenomena in terms of matter in motion. Yet, by the end of the seventeenth century the limits and difficulties of this project were glaring. Several domains of natural philosophy (e.g. biology, chemistry, medicine) resisted a full-blown mechanization, while even in the realm of physics itself the limits of mechanism became clearer and clearer. Newton’s mathematical treatment of the “force of gravity” and Leibniz’s account of “living forces” are symptomatic examples of how early modern science soon realized the need to reintroduce the notion of powers and forces in the clockwork universe. Leibniz explicitly theorized the necessity to save the legacy of Aristotelianism and exploit it to rethink the source of causal activity in nature. This suggests that the relationship between Aristotelianism and the so-called “Scientific Revolution” is much more complex than what the standard narrative usually admits. However, the precise relationship between the Aristotelian legacy and the new science is not yet fully understood. Thus, as yet little is known about the way in which seventeenth-century Aristotelians themselves responded to the mechanical philosophy and offered rival accounts that aimed to update and modernize Aristotelian natural philosophy. Moreover, to the extent that we see in early modern science and philosophy a certain backsliding to Aristotelian concepts to account for the natural effects of and causal relationships between bodies, these concepts often underwent substantial reinterpretation. Specifically, they were reconceptualised in ways that bear further exploration and research, so as to make them amenable to quantification and geometrical analysis. This panel aims to explore these aspects of the puzzling relationship between the legacy of Aristotelian natural philosophy and the new science of the seventeenth and eighteenth centuries. The first paper will focus on seventeenth century Aristotelians themselves and on the novelties they introduce in the attempt to compete with rival mechanist approaches. The second paper will discuss the controversy between Leibniz and Maupertuis on the notion of “living force” and on the best way to understand and elaborate this concept. The third paper will concentrate on Kant’s pre-critical New Elucidation in order underlie how Kant’s view remains indebted to the seventeenth-century debate concerning the nature of causation and how his view squares with what a later scholastic author such as Suarez would have considered a kind of sine qua non causation, rather than a more robust form of causation.

Maupertuis and the Leibnizian Living Force
• Christian Leduc (Université de Montréal)

It is well known that Maupertuis questions Leibniz’s principle of living force in order to favor his own principle of least action. Both principles aim at completing the mechanical explanation of nature by the help of a metaphysical and teleological grounding, although they are at the foundation of distinct conceptions of physics: Maupertuis adopts the Newtonian theory of body and force, while Leibniz integrates the idea of a conservation of quantity of force in his own renewed dynamics, opposed on many points to the Cartesian mechanics. In comparison with the Leibnizian living force, Maupertuis maintains that the least action is universal in its application – to all bodies, including hard ones –, which proves its scientific superiority (Œuvres I, p. 42-43). The controversy over the discovery of this principle, initiated by Samuel König, is also well documented and was the subject of numerous works. On this day, we know that the concept of least action can be found in Leibniz (but maybe not the principle itself as is the case in Maupertuis), but that Maupertuis could most probably not have read the manuscripts, unpublished, which contain the explanations about this concept. Above all, we can say that both approaches were distinct and not meant to solve the same problems (Gueroult, 1967, Bachelard, 1975, Hecht, 1995). In this paper, I would rather like to reexamine Maupertuis’ definition of the principle of conservation of living force. The main problem is that the quantity of living force and the least action do not refer to the same
determinations of nature. However, Maupertuis relates one to another, in particular in the Essai de cosmologie, to give advantage to his principle. Leibniz's principle expresses the conservation of a quantity, while Maupertuis' principle is independent of any quantitative conservation, and is even perhaps contrary to such an idea. I would like to analyze the way Maupertuis gives an interpretation of the idea of a principle of conservation, in Leibniz, but also Descartes, to integrate it within his reading and to redirect the debate to the problem of least action. What is common to both views is that they rely on the hypothesis that mechanical physics requires a teleological principle, showing the perfection of nature on the basis of God's will; but Maupertuis had to reorient the Leibnizian idea of a universal law in order to justify the superiority of his principle. This implies to give up on major elements of Leibniz's metaphysics, in particular that God's perfection can be understood by evaluating the totality of forces in nature.

Sine qua non Causation: Kant and the Legacy of a Scholastic and Early Modern Debate

- Andrea Sangiacomo (University of Groningen)

Recent scholarship on the roots and sources of Kant's account of causation convincingly demonstrated that Kant is seeking for an alternative to major accounts of causation, namely the "physical influx" model, the "pre-established harmony" and "occasionalism" (Watkins 2005, Hogan forthcoming). The first can be traced back to scholastic thought; the second is due to Leibniz, while the third is usually associated with Malebranche. There is an apparently unchallenged consensus on the fact that Kant rejected both Leibnizian and Malebranchian models of causation and developed a new version of the "physical influx theory". Nonetheless, in this paper we argue that Kant's understanding of causation inherited crucial features of the occasionalist model advanced by Malebranche. First, we sketch the later-scholastic account of causation by taking Suarez's view as an example of it. We focus on Suarez's distinction between per se and per accidens causation and draw attention on a particular case of per accidens efficient causation, namely, sine qua non causation. According to Suarez, a per se cause is a cause that has a real influx on the being of the effect in virtue of its own esse or being. On the contrary, sine qua non causes are necessarily linked to the effect, but they do not influence them in virtue of an action. Although Suarez stresses that is difficult to distinguish between proper per se efficient causes and merely sine qua non conditions, he maintains that (whenever possible) natural philosophy should always consider per se efficient causes as the real causes of effects. Second, we show that the seventeenth century revival of occasionalism amounts to the claim that sine qua non causes are the only causes operating in nature (Perler and Rudolph 2000). This point is made explicit both by Malebranche and by his critics. Malebranche also developed a refined account of how God operates in nature, by offering a new account of laws of nature understood as general wills of God. In Malebranche's view (and pace Leibniz), God does not operate by arbitrary and instantaneous interventions in nature, but always through the simplest and most general laws. Third, we show that Kant's pre-critical writings defend an account of causation that relies in fact on a kind of sine qua non causation. We focus on the New Elucidation and on Kant's claim that isolated substances, or substances considered as merely existing, cannot produce any effect. In fact, Kant argues that interactions are possible only if God establishes the whole array of relationship among substances. Since interactions are a necessary condition for causation, but interactions require God's creation of the relationship among substances, it follows that substances considered in itself are necessary but not sufficient conditions for proper causation. Substances are necessarily linked to their reciprocal effects, but they do not provide a sufficient ground for them. In this sense, finite substances, considered in themselves, remain sine qua non efficient causes in Suarez's and Malebranche's sense.
4.2 Symposium: Fourteenth-Century Theories of Logical Consequence

This panel will include three presentations on fourteenth-century theories of logical consequence. Of the many branches of medieval logic, medieval treatments of consequences (consequentia) most closely resemble the subject matter of modern-day logic, and therefore are specially poised to shed light on the origins of logic as it is presently conceived. Consequences were a topic of interest particularly in the fourteenth century, receiving the attention of logicians such as Walter Burley, William of Ockham, Jean Buridan, and Marsilius of Inghen. Ockham and Burley each devoted sections of their respective logic textbooks to consequences, while Buridan and Marsilius each have special treatises devoted to the topic. Despite their potential interest to both historians of logic and historically-inclined contemporary logicians, these texts are not all widely available for consultation by the non-specialist: Burley's and Buridan's texts are the most widely available, existing in both Latin critical editions and well-known English translations; the relevant section of Ockham's Summary has received a critical Latin edition, but no English translation has yet been published; as for Marsilius, only manuscripts are available, though a Latin edition is under preparation. These authors examined and codified a wide variety of logical relationships between propositions, going well-beyond the confines of the Aristotelian Syllogistic they inherited. Among much else, their texts exhibit novel and sophisticated semantic accounts of modal and temporal propositions as well as a curious distinction between formal and material consequences. Although such consequences were classified on logical grounds as good (bona), true (vera), or valid (valet), perhaps not all would be classified as valid inferences by today's standards. This raises the intriguing question as to just what the definition of logical consequence is --- or, rather, whether there is such a unique definition after all.

Ockham and Buridan on the Distinction between Formal and Material Consequences

- Milo Crimi (UCLA)

William of Ockham and John Buridan appear to offer very different accounts of the distinction between formal and material consequences. In his Summary of Logic, Ockham appeals to a doctrine of intrinsic and extrinsic middles (media) to mark the distinction --- a terminology that harkens back to Boethius' treatment of the logic of places (locri), later codified by Peter of Spain in his enduring Summaries of Logic. In the Treatise on Consequences, Buridan, by contrast, makes no mention of intrinsic and extrinsic middles, but rather marks the distinction between formal and material consequences in terms of a hylomorphic account of the matter and form of propositions. The two theories then classify certain consequences under different headings --- some consequences Ockham considers to be formal are taken by Buridan to be material --- and therefore appear to have very different structures. Closer examination, however, shows this not to be the case. Here I argue that the accounts underlying Ockham's and Buridan's distinctions between formal and material consequences share a common feature. In particular, I argue that Ockham's talk of extrinsic middles "having to do with the general conditions of propositions" ("respiciens generales condicione propositionum") is an oblique reference to a notion of propositional form in agreement with Buridan's.
This paper details and situates Walter Burley's account of consequentiae, or consequences. In the first part, I present the basic distinctions governing Burley's division of consequences, followed by a symbolic reconstruction of the rules governing consequences in Burley's later version of De puritate artis logicae. An examination of the rules of Burley's logic reveals that: Burley distinguishes consequences from conditionals, and prioritizes the former. For instance, Burley gives contraposition for consequence as a basic rule, on which he claims contraposition for the conditional depends. For Burley, every consequence is an enthymeme. Hence consequences are not foundational for Burley (as they are for his younger contemporary John Buridan), but rather depend on the theory of the topical syllogism. While Burley discusses consequences involving contradictions, he gives every indication that these consequences are not explosive. Rather, propositions derivable from a contradiction are restricted to those whose terms either themselves appear in the premises or are linked to these terms in a suitable way (e.g. where the beings denoted by one term form a subset of those denoted by the other). In this respect, Burley's account is somewhat close to modern relevant logics. The second part of the paper compares Burley's remarks on formal consequence to the account of John Buridan, and thereby charts a path whereby Burley's account was able to be creatively engaged by his younger contemporary at Paris. For Buridan, i) a consequence holds formally exactly when it holds for every uniform substitution of its categorematic terms; ii) it holds in virtue of its formal part, identified with its syncategorematic terms; and iii) the distinction between formal and material consequences, based on that between formal and material parts of a language, is taken to be disjoint, exhaustive, and absolute. Hence, Buridan inherits a medieval version of Tarski's problem of the demarcation of logical constants. By contrast, Burley sees uniform substitution as a necessary, but not sufficient, condition for formal consequence. Hence, Buridan's theory takes what for Burley is a method for finding formally invalid consequences, and turns it into a definition of formally valid consequence. Burley distinguishes between formal consequences that hold in virtue of terms and those that hold in virtue of the whole structure of an argument. But whenever Burley speaks about formal consequences holding in virtue of terms, he means categorematic terms. Hence, Buridan's understanding of categorematic consequences as dependent on syncategorematic terms reflects an interesting inversion of Burley's approach. Burley's distinction between formal and material consequence is relative, and depends on an explicit determination of which terms are to be held constant in a given consequence. Hence, unlike Buridan's account, Burley's does not suffer from Tarski's demarcation problem. Burley is probably best known for his ‘extreme realist’ metaphysics. This study shows an examination of the logic of this extreme realist provides much of interest even for understanding the nominalists opposed by – and indebted to – him.

4.3 Contributed Papers: Models and Representation

Disciplining Little Models: Epistemology and Scale Models from the 17th Century to the Present

• Susan Sterrett (Wichita State University)

There are already several published case studies in the intellectual history of scale models. In this talk, I briefly describe them, and discuss the development of the methodology used from the seventeenth century up to the present day. In "Fish and Ships: Models in the Age of Reason", Simon Schaffer describes an era in the eighteenth century during which the outcome of trials of specially constructed models was often presented as evidence for scientific claims and policy recommendations. Besides the use of Leyden
jars (and later, electric piles) to model electric fish and the use of tabletop models of house and thundercloud configurations to investigate effects of lightning, there were more mathematically guided uses in mechanics. When investigations were commissioned for publicly funded mechanical structures (e.g., ships and bridges), they often involved building and experimenting on a model, and providing the results as evidence. By the eighteenth century it was known that larger versions of an object often behaved differently than scaled down ones did. Schaffer notes that the use of models bore on a larger debate about philosophy of science: "[In] these enterprises of 1790s engineers and analysts, the rival claims of custom and principle were fought out with experimental models. Some showed that artisan custom was a legitimate authority; others that rational principles could govern tradition." [Schaffer 2004; p. 74] I supplement Schaffer's account with some prehistory and an account of contemporaneous developments in physics and mechanics. In "From Scaling to Simulation: Changing Meanings and Ambitions of Models in Geology," Naomi Oreskes describes an era during which experiments on physical models were used in geology. [Oreskes 2007] The same issue arose: how the model was constructed and employed in making scientific claims were matters of debate. Oreskes claims a change occurred in the mid-twentieth century in which computer simulations replaced the use of physical models, and that it was due to a change in the demands placed on geological research by its patrons: whereas geologists had previously focused on understanding the causes that had operated in the past to bring about geological formations such as mountain ranges, during the Cold War they were asked to predict future changes in geological formations near nuclear waste repositories. I provide a modified account of this history that makes sense of the fact that geologists actually still use experiments on physical models today. Though neither of these historical studies mentions it, physical model experiments are used in the present day in a wide variety of disciplines, including theoretical physics. I present examples, and characterize the different methodologies.

On the Analogy between Scientific Models and Maps: Insights from Medieval Cartography and Post-Representational Cartographic Epistemology

• Guilherme S. Oliveira (University of Cincinnati)

A common view in the recent philosophy of science literature is that the representational relationship between a model and the target phenomenon it simulates is what secures the epistemic value of model-based scientific research. This representationalist assumption is often supplemented by implicit or explicit analogies between scientific models and maps. In this paper I draw from the history and philosophy of geography to argue that a better understanding of maps and map-making motivates either rejecting the analogy between models and maps or rejecting representationalism about models. Theories of scientific representation abound: there are views based on isomorphism (van Fraassen 2008) and similarity (Giere 2004, 2006, 2010), accounts dubbed “inferential” (Suarez 2003, 2004), “interpretational” (Contessa 2007), and “semiotic” (Knuttila 2010), as well as deflationary approaches (Suarez 2015; Morrison 2015). Despite the variety, most in the literature frame the epistemic value of model-based science in terms of how models represent their target: models enable indirect investigation of some phenomenon insofar as they more or less accurately represent the phenomenon they simulate. This representationalist view has also included analogies with maps and map-making. Because of the known distortions and inaccuracies contained in models, it is necessary to determine what the “representational mapping” is, i.e. what aspects of a model represent what aspects of the world and in what ways (Weisberg 2013; Van Fraassen 2008; Giere 2010). Further, some authors have explicitly compared scientific models to maps, suggesting that the quality of both models and maps depends on how well they represent some existing particular (see e.g. Bailer-Jones 2009 after Giere 1999). Despite the popularity of representationalism and the
seeming plausibility of the model-map analogy, serious consideration of the history and philosophy of geography motivates a different perspective. Traditional views in cartographic epistemology have tended to frame the history of cartography as “progressive, cumulative, objective,” with maps “continually evolving in their accuracy” (Martin 2011, p. 148). In contrast, post-representational cartographic epistemology emphasizes how “mappings” are used for addressing “relational problems” (Kitchin et al 2013, p. 482) and therefore should be treated as “collaborative artefacts” (p. 483). Rather than the ontology of maps (i.e., how maps attempt to describe what exists), in this view the essential feature of maps is their ontogenic role (i.e., how maps play a dynamic role in problem solving). I discuss the main ideas of cartographic post-representationalism, elaborating on this view by examining the “artifactual turn” in medieval cartography that led to the rise of modern maps. Moving away from richly adorned scholastic maps which had clear “didactic and moralizing” objectives, the late 1200s sees the invention of portolan charts “made by seamen for seamen” (Livingstone 1992, p. 50). These were geometrical versions of written records and instructions (Hofmann et al 2013), designed as tools to be used alongside other nautical instruments (Crone 1978; Unger 2010). I argue that the historical evidence and the post-representationalist perspective in cartographic epistemology motivates either rejecting the model-map analogy or taking seriously the idea that scientific models are tools or artifacts, not truth-bearing representations.

4.4 Symposium: Nature and Life in Classical German Philosophy

We most often associate German idealism with Fichte's now famous declaration of “the primacy of the practical.” Picking up from Kant's insight that his articulation of transcendental idealism into distinct theoretical and practical realms in the first two Critiques, German idealism from at least 1790 (Kant himself, as well as Fichte, Schelling, and Hegel in his wake) was preoccupied, on the received view, with reconciling nature and human freedom. Kant's own attempt to bridge the gulf was cautious and qualified, and was met with dissatisfaction in nearly all quarters. In what seems to be a race to cast of the epistemological limits imposed by Kant's critical philosophy, the idealists constructed increasingly speculative philosophical systems that aimed to reconcile human morality and freedom with the causal determinations of the natural world in which they are always found. In the creative confusion of all this system building, the unquestioned principle of the whole endeavor was an unwavering commitment to the primacy of human freedom and morality. As part of a growing scholarly effort to qualify and nuance the received view sketched above, this panel will consider the ways in which the German idealism developed not only through debates about the metaphysics of freedom, but crucially through engagement with the natural sciences, and the emerging life sciences and chemistry in particular. The first paper, “A Material Correlate of the Transcendental Unity of Apperception: Reconciling Nature and Consciousness in the Opus postumum,” turns to Kant's last writings. In these notes Kant argues that an all-pervading material—ether or caloric—is the highest material condition of the possibility of experience in general, and so functions as the material correlate of the transcendental unity of apperception. This paper argues that the Opus postumum invites but ultimately frustrates the claim that Kant's late thinking provided new and more robust means for uniting nature and consciousness on the basis of a new interest in chemistry. The second paper, “Schelling's Freiheitsschrift and the inevitability of teleological reason,” returns to Kant's analysis of natural teleology and its relation to human freedom, now in order to explain how Schelling's Philosophical Investigations of the Essence of Human Freedom takes up and contests Kant's merely heuristic account, supplementing Kant's biological analysis of self-organization with a speculative metaphysical explanation of “self-actualization.” The paper concludes that Schelling's metaphysical
speculation supports a more robust engagement with the philosophy of nature than does Kant’s epistemologically-restrained natural theology. The final paper, “The Logic of Life and the Life of Logic: Hegel on the Continuity between the Organic and the Spiritual,” argues that the truly problematic division in Hegelian philosophy is not that between nature and freedom, but between the living and the dead. Drawing on Bichat’s conception of life, Hegel develops a dynamic understanding of conceptuality that allows him to overcome this Kantian problem but ultimately highlights the similarly vexing distinction between the living and the dead.

A Material Correlate of the Transcendental Unity of Apperception: Reconciling Nature and Consciousness in the Opus postumum

- Michael Olson (Macquarie University)

After focusing for more than a generation on epistemological questions that emerge from the Critique of Pure Reason (1781/1787) and interpretive puzzles centered on the Transcendental Analytic, Anglophone Kant scholarship has, in recent years pursued a new interest in the ways Kant’s engagement with the natural sciences inform his conception of the conditions of human cognition and morality. The enigmatic working notes that have come to be known as the Opus postumum (written 1796-1803) offer ample and still underappreciated resources for those examining the reciprocal influences of Kant’s natural scientific and more properly philosophical interests. Whereas much recent research has focused on Kant’s view of the life sciences, the Opus Postumum invites us to consider how Kant’s understanding of the emerging science of chemistry contributed to the development of the critical philosophy through the 1790s. Although Kant famously dismissed chemistry as incapable of becoming a true science in the Metaphysical Foundations of Natural Science (1786), his correspondence, notes, and publications in the last decade of his life indicate a sustained interest in developments in the field. He paid particular attention to theories of ether that promised to provide a unified ground for the dynamic theory of moving forces Kant himself defended. By the late 1790s, Kant came to think of ether as a central concept not only for chemistry but for transcendental philosophy as well. This paper argues that the analysis of ether in the Opus postumum suggests that Kant came to think it necessary to identify material conditions of cognition to complement the famous formal and subjective conditions elaborated in the Critique of Pure Reason. These material conditions of cognition are, moreover, real in contrast to the merely regulative or heuristic principles through which Kant sought to reconcile humanity and nature in the Critique of Judgment (1790). It appears, in other words, that Kant’s late engagement with chemistry and the role of ether in grounding the moving forces of the physical world provide a real bridge between the spontaneity of transcendental apperception and the determinate causality of the natural world. This appearance is, however, misleading. In the end, Kant’s notes do not argue that the ether functions as a kind of tertium quid that unifies the activity of the mind and the inertia of the material world (though there are indeed passages that invite such an interpretation). Rather, the position that emerges from the Opus postumum is that what serves as a newly-recognized material condition of experience is not ether as a real, empirical substance, but a certain idea of ether that is both necessary to ground the unity of the diverse array of laws of nature and compatible with the a priori principles of natural science in general as laid out in the first Critique and the Metaphysical Foundations.
Schelling’s Freiheitschrift and the Inevitability of Teleological Reason

• Ashley Vaught (Independent Scholar)

In the Philosophical Investigations into the Essence of Human Freedom (1809), Schelling describes the revelation of God through creation. This presentation provides Schelling an opportunity to narrate the process through which human being evolves from primitive nature. The process is largely teleological, although Schelling recognizes that teleology alone cannot account for it. The brilliant discovery of the Freiheitschrift is the dark ground, which is the co-constitutive basis of creation along with the will of the understanding. The will of the understanding is the force that directs a teleological development of nature, from the primitive to the increasingly involuted, diversified forms, of which the apex is human being. In the human being, I describe the teleological development as “self-organization,” by which I mean what we today might call the genetic coding of an organism that directs its physiological changes through its existence. But human being is unique, for Schelling, in that it possesses a certain will that emerges in the moment (for lack of a better word) of “self-actualization.” I use this term to describe the event(s) in which a human soul is formed. The event might equally be called germinal, insofar as it produces a character according to which an individual acts. To add one further dimension, Schelling allows that self-organization occurs in time, whereas self-actualization is an eternal act. In this essay I shall discuss the consequences of the dark ground for a philosophy of nature and in particular for human freedom, especially in view of the critique of teleological judgment. The critique of teleological judgment in Kant’s third Critique poses serious limitations for a philosophy of nature, and especially for a philosophy of nature that is inscribed in an account of divine revelation. Yet one might well argue that Schelling has done justice to those limits in his unique and potentially heretical revelation of God, in which God does not stand outside the development of nature, but emerges through it. That said, just as for Kant there is a certain inevitability of teleological reason—and especially so for German Idealism writ large—teleology shows up even in the climactic accounts of self-organization and self-actualization. Self-organization describes an intrinsic purposiveness that repeats purposiveness in nature at large. The individual human being emerges from a phylogenetic process that has been repeated through non-human species. How does self-actualization secure Schelling’s account from submitting to the tyranny and blindness of teleological reason? Self-actualization is bound by the limits of Kant’s own account of transcendental freedom (it must lie outside of time), so as to protect it from inscription within efficient causality and concomitant successive temporality. Yet in order for it to be coherent, it cannot determine the character of the human being in advance—say, in the moment of (genetic) conception. Self-actualization is therefore eternal primarily in the sense that it lies outside of time. In other words, no single temporal moment can explain this character. I conclude the essay considering the price of this account for the revelation of God.

The Logic of Life and the Life of Logic: Hegel on the Continuity between the Organic and the Spiritual

• Raoni Padui (St John’s College, Santa Fe)

It has been widely acknowledged that one of the central motivations for both German Idealism and German Romanticism was the attempt to work through and overcome what some saw as the dichotomy between the natural world and the spiritual world in Kant’s critical project. For Kant, the distinction that is most difficult to overcome or mediate between is to be located between the spheres of necessity and freedom. This places him within the tradition, begun by Descartes and still operative today in “hard problems of consciousness,” of thinking that there is a difficulty in incorporating the rational, minded, and moral subject within a natural world constituted by deterministic causal laws. Hegel, however, saw himself as having overcome such a dichotomy, and in this paper I will argue that the central notion for this putative
overcoming is his understanding of life. The concept of life appears at surprising places within Hegel's system, making a central appearance at the end of the Science of Logic, a work that largely traces the necessary connection between what Kant called the categories of thought. In this paper I will attempt to explain why the “Idea of Life” is so significant to Hegel's logical project, and why he calls life the “immediate Idea.” But in order to shed light on how he thinks about life and its application to the logic, I will turn to his descriptions of the organic and the living in his Philosophy of Nature. In that work, Hegel engages with several natural scientists of his day, in order to develop a notion of life as self-organization and self-making that opposes absorption into its environment. Of paramount importance to his understanding is Xavier Bichat, who defines life as “the totality of those functions which resist death.” I will argue that what emerges through Hegel's engagement with the theories of the organism of his time is a picture of thinking as continuous with the living, in so far as resistance to stimulus, sensation, self-organization and self-determination are interpreted as more highly differentiated but continuous forms of the same process, a process he calls “life.” The paper argues that Hegel is a thinker who defends the continuity between the organic realm and the conceptual realm, which in turn explains why there is, for him, no major difficulty in mediating between the natural world and the world of minded, spiritual beings. Rather, in the conclusion I hope to show that Hegel, like Bichat who he relies on and later vitalists, has difficulty in explaining the transition between the spheres of dead, non-living, matter and that which is conceptual and therefore living.

Friday - 24 June (Parallel Session V)

Parallel Session V (11:00-12:30)

5.1 Symposium: Dedekind's Mathematical Structuralism: 100 Years Later

Richard Dedekind was one of the greatest mathematicians of the nineteenth century, as well as one of the most important contributors to algebra and number theory of all time. Among historians of mathematics, he is mostly known for his theory of ideals, now a central part of modern algebraic number theory, as well as for his introduction of the notions of algebraic number, field, module, and lattice. Among philosophers of mathematics, his contributions to the foundations of mathematics are widely known, including his introduction of the real numbers via Dedekind cuts, his approach to arithmetic in terms of the Dedekind–Peano axioms, his proof of the categorizity of those axioms, and his contributions to the early development of set theory. In addition, during the last few decades he has been acknowledged as an early proponent of a structuralist conception of mathematics. Dedekind lived from 1831 until 1916, so that this year is the 100th anniversary of his death. This centenary is an occasion for reconsidering his contributions to both the history and the philosophy of mathematics. The present symposium proposes to contribute to this reconsideration by focusing on several related aspects of Dedekind's structuralist perspective. This includes: (i) structuralism seen as a kind of methodology for mathematics, or as a way of organizing mathematical knowledge, in terms of basic axioms, deductions from those axioms, relational systems satisfying them, and morphisms between the systems; (ii) structuralism taken to be a philosophical view about the subject matter of mathematics, namely the view that mathematics is the general study of abstract structures, with a systematic theory of sets and functions providing the background, as opposed to the traditional, much narrower view of mathematics as the science of number and quantity; (iii) structuralism investigated in terms of its underlying logical or quasi–logical principles, including set– and function–theoretic construction principles, as well as principles for the kind of
abstraction often associated with structuralism about mathematics. It is these three aspects of mathematical structuralism that will be explored in the talks by Ferreirós, Morris, and Reck, respectively (cf. the abstracts below). By looking at Dedekind’s structuralism from all the mentioned perspectives, the goal is to address a number of issues relevant for HOPOS. First, reconsidering his structuralist methodology is meant as a contribution to what is now often called “the philosophy of mathematical practice”; it also presents Dedekind’s mathematical innovations in relation to, e.g., the works of Emmy Noether, Nicolas Bourbaki, and current category theory, thus adding to our understanding of the history of mathematics. Second, rethinking his foundational ideas contributes to debates in the philosophy of mathematics, e.g., about the nature of mathematical concepts, objects, and functions. Third, reconstructing his basic assumptions or principles logically deepens both of those debates further, by providing finer-grained details of the views involved; and it connects Dedekind’s views also to approaches by, among others, Georg Cantor, Gottlob Frege, Bertrand Russell, and David Hilbert, thus contributing to the history of the philosophy of mathematics as well.

Steht es alles schon bei Dedekind? Dedekind's place in the history of structuralism

• José Ferreirós (Universidad de Sevilla)

It is a well-established view that Dedekind’s work played an important role in the emergence of structural thinking in modern mathematics; important issues of different kinds – logical, metaphysical, epistemological and methodological – arise around his approach (see Reck 2011). Dedekind’s structuralism is easy to discern, e.g., when comparing his foundational work to Frege’s, yet a lot more can be said about it by bringing to bear his contributions to algebra and algebraic number theory as well. On the other hand, it has been argued that Dedekind’s mathematical innovations fall short of modern mathematical structuralism; Corry (2004) has thus taken exception to Emmy Noether’s famous remark that “it’s all in Dedekind already” [es steht alles schon bei Dedekind]. Generally speaking there is thus a great need for further clarifying Dedekind’s place in the history of structuralism, his connections with other key figures, and his influence, also contrasted with other trends. Due to the impact of model theory and of Nicolas Bourbaki’s works, there is a tendency in the literature to focus on the basic set-theoretic definition of a structure (as a “relational system”, a set endowed with operations and relations); this is often taken to be the goal and focus, so that inquiries about the emergence of structuralism focus on the configuration and exploration of that notion. And indeed, much can be said about Dedekind and his contributions along such lines. Yet my proposal in this talk is that we should follow a different course. I will discuss Dedekind’s structuralism in connection with the basic idea of a morphism. This word cannot be found in Dedekind, but he pioneered the study of isomorphisms, homomorphism, automorphisms (and this aspect of his work turned out to be highly influential among the generation of modern algebraists who focused on morphisms during the 1920s and 1930s, e.g. Noether, Emil Artin, and Øystein Ore). While Dedekind does not speak of “morphism”, he elaborated a notion of Abbildung (mapping or representation) that increasingly focalized his attention from the 1870s onward (Ferreirós forthcoming). An Abbildung is not just a function in the basic sense; rather, in his works it tends to play the role of a form-preserving mapping, that is, a relation between an original and its image [Bild] that specifies a common, shared structure. We will thus consider the evolution of Dedekind’s views about Abbildungen, the role they played in his work, and some aspects of their reception.
Dedekind’s structuralism: Creating concepts and deriving theorems

- Rebecca Morris (Carnegie Mellon University) and Wilfried Sieg (Carnegie Mellon University)

In his well known 1888 essay, Was sind und was sollen die Zahlen?, Dedekind set out to offer a rigorous, purely logical account of the natural numbers. However, a comparison of the final version of his essay with earlier manuscripts reveals that his work underwent a number of changes. In particular, his earlier work focused on the creation of new mathematical objects, while the final version centered around the creation of concepts. In a forthcoming article, Wilfried Sieg and I argue that the shift in Dedekind’s position is mathematically and philosophically significant (Sieg and Morris forthcoming). On the mathematical side, the final version of his essay introduced, via the concept of a simply infinite system, the Dedekind–Peano axioms, as well as new and important meta-mathematical results. For example, Dedekind established, in modern terminology, the categoricity of the concept of simply infinite system. On the philosophical side, we suggest that Dedekind’s final version and his earlier views represent different kinds of structuralism. We claim that the structuralism to be found in the published version of his work is not the same kind of structuralism that is often attributed to him in the literature (e.g., in Reck 2003 and Yap 2009). In this talk, I will discuss the transformation of Dedekind’s work on the foundations of arithmetic. I will also describe the new interpretation of his account that Sieg and I have developed. In addition, I will address the differences between our interpretation and others given in the literature, as well as tackle the question of why Dedekind changed his position.

On Reconstructing Dedekind Abstraction Logically

- Erich Reck (University of California, Riverside)

At a few points in Dedekind’s writings, he appeals to a kind of “abstraction”, and to a corresponding form of “free creation”, that is central to modern mathematics for him, and in particular, to the nature of the natural and the real numbers. Unlike many of his other contributions, this appeal has been the target of strong criticisms, or even ridicule, for a while (from Bertrand Russell to George Boolos and beyond). In particular, the “creation” involved tends to be seen as representing a crude form of psychologism decisively refuted by Gottlob Frege and Edmund Husserl soon thereafter. Dedekind’s philosophical position has correspondingly been dismissed as a form of “mystical structuralism” (Dummett 1995). In defense of his views, the suggestion has been made to interpret the appeal to “abstraction” and “free creation” not in a psychologistic or more generally psychological manner, but along logical lines (Tait 1997, Reck 2003). Yet how exactly to interpret, or how to reconstruct, the underlying “abstraction principles” remains in need of clarification. The primary goal of the present talk is to distinguish four options in this connection, i.e., to identity four kinds of possible Dedekindian “abstraction principles”. Depending on which of these options one takes to be implicit in Dedekind’s writings, his position is assimilated to views often associated with, respectively, Bertrand Russell, David Hilbert, Gottlob Frege, and Georg Cantor. That is to say, a Russellean, a Hilbertian, a Fregean, and a Cantorian reading of Dedekind are possible on the sur face, as will be shown. Moreover, the former two lead to what is known in the literature as “eliminative structuralism”, while the latter two lead to “non-eliminative structuralism”. In the final part of the talk, it will be argued that the Fregean and Cantorian readings are supported most by the available evidence, although the other two are compatible with large parts of the texts too. Whether it is the Fregean or the Cantorian option that is to be preferred in the end is harder to decide; but some tentative suggestions will be made in that connection as well.
5.2 Contributed Papers: Aristotle, Harvey, and Cavendish

The Oriel Noetics, Aristotle, and Newton
• Joseph Milburn (University of Pittsburgh)

The Oriel Noetics were a group of scholars active in Oxford during the 1820’s and 1830’s. The most famous members of this group were Richard Whately and John Henry Newman. In this paper I explore the attempts of Richard Whately and John Henry Newman to provide a synthesis of Aristotelian philosophy and Newtonian mechanics. In this paper I argue that Whately and Newman have a realist, but relativizing view of Newtonian mechanics. On their view, Newton’s mechanics are true as far as it goes, but they are true in virtue of abstracting away all aspects of reality except those that pertain to the position, mass, and forces acting on an object. Thus, Newton’s mechanics provide a true view of reality, but a partial one. I argue that Whatley’s and Newman’s main move is to generalize Aristotle’s account of mathematical knowledge so that an analogous account is made to apply to all of the natural sciences. Each of the natural sciences understands nature only partially, by focusing on certain aspects while ignoring others. A general Aristotelian outlook is preserved, then, not by adopting an anti-realist approach to the natural sciences, but by making the objects of these sciences something less than the whole of nature, even regarding those sciences that are taken to be fundamental. I argue that Whatley’s and Newman’s approach to Newton provides the template for an appealing philosophy of science comparing it to the work of contemporary philosophers of science such as Nancy Cartwright and John Dupre.

William Harvey and Margaret Cavendish on the Generation of Animals: Materialist and Aristotelian Models of Explanation
• Benjamin Goldberg (University of South Florida)

In his Exercitationes de Generatione Animalium (1651), William Harvey famously established that generation occurred by epigenesis: the fetus forms part by part, over time, a process that was described and understood as fundamentally teleological for centuries, despite changing philosophical fashion. Even marshaling all the resources of an Aristotelian philosophy founded upon final causality, Harvey had great difficulty explaining generation. And, as has been pointed out since at least Jacques Roger’s work from late 1960s, later corpuscularian and materialist philosophers had an even more difficult time explaining and understanding epigenesis. In this talk, I want to compare and contrast Harvey’s Aristotelian approach to explaining generation with that of vitalist-materialist Margaret Cavendish in her Observations upon Experimental Philosophy (1666), Philosophical Letters (1664), and Philosophical and Physical Opinions (1655). Though these thinkers are quite different, they both arrive at an understanding of generation that depends on the rationality of nature. More specifically, they both end up articulating an idea of something like natural knowledge, wherein human ratiocination and ideas serve as models for the way that epigenesis occurs, and thus how they attempt explanation. Specifically, I shall explore how Cavendish does this by extending to matter powers of perception and thought, while Harvey accomplishes this by amalgamating the vegetive soul and the rational soul via the power of God’s Design. I end with some thoughts on the idea of reason in nature and science in early modern thought.

Cavendish on Accidents: Identification and Reduction Strategies
• Jonathan Shaheen (Ghent University)

The basic contours of Margaret Cavendish’s trialist materialist naturalism are increasingly well-known: she holds that nature is composed of three types of matter, the two animate types of which are, from 1664 on,
repeatedly identified with motion. In Philosophical Letters, motion is held to be identical with these kinds of matter on the grounds that body necessarily accompanies motion. Cavendish is also committed to an identity thesis for other Aristotelian accidents, namely, figure and place. However, she identifies the latter accidents not with a particular type or types of matter, but instead with composed natural bodies, which are individuated as the effects of the motion of self-moving matter. The difference between how Cavendish identifies motion with matter—by making motion a type of matter—and how she identifies figure and place with matter—by identifying figures and places with bodies—cries out for explanation. I propose that her theorizing here can be explained in terms of the explanatory power of her identification of motion with a type of matter. Galileo’s Assayer presents an interesting contrast with Cavendish. First, he also considers necessary accompaniment as a relation linking body and motion. But his comments about necessary accompaniment apply to all so-called primary qualities, whereas Cavendish, as noted above, treats motion differently from figure and place. Second, Galileo distinguishes qualities like heat, which can be given corpuscularian explanations, from ‘other conditions’ (i.e., secondary qualities) like colors, tastes, and odors. But Cavendish accounts for color and heat in precisely the same way, explaining both in terms of the activities of more basic (in fact, basic simpliciter) self-moving matter. That is, in addition to Cavendish’s two identification strategies for the incorporation of Aristotelian accidents, she also has a reduction strategy. Now, Cavendish gives a positive argument for the identification of motion with matter, and she is able to take on board the explanations of her contemporaries by making that identification. Relevant evidence comes from her discussions of chemical principles in Observations upon Experimental Philosophy. There, Cavendish is concerned to show how each of the chemical principles proposed by the theorists of her time can be further explained in terms of self-moving matter. She thus claims that self-moving matter is more fundamental than any of these principles. That move allows her not only to adopt any explanation that can be given in terms of these principles, but also to present her account as deepening any such explanation by accounting for the constitution of the principles themselves. While her ontology of self-moving matter has sufficient explanatory power to allow for the reduction of accidents like heat and color, figures and places are effects of the motion of self-moving matter. But these effects are themselves composed natural bodies. So the choice between identification and reduction for all accidents other than motion itself falls out of her account of motion as a type of matter, and of the allied theory of the individuation of natural bodies.

5.3 Contributed Papers: Language and Imagination: Hobbes, Kant, Berkeley

Language, Memory and Universality in Hobbesian Scientia

- Peter Distelzweig (University of St. Thomas)

When thinking about 17th century reformations and transformations of the study and understanding of the natural world, it can be helpful to start by distinguishing questions of method from questions of the product of those methods. The latter, in turn, can be approached in terms of changes in the understanding of what constitutes scientia of nature. In the “received view” of scientia, we can distinguish three facets: scientia is certain, universal and causal. When looking at the work and thought of a 17th century neoteric like Thomas Hobbes, it is valuable then to examine how each of these facets fares. Much attention has been provided to the causal facet, attending to the importance of “causal definitions” and “makers’ knowledge” in Hobbes’s system of philosophy. In this paper, however, I focus instead on universality. The universality in scientia depends for Hobbes on universality in language. Hobbes articulates a nominalistic theory of language in which only names can be called universal and in which, more generally, language manipulation is identified as the principle instrument of reasoning. This nominalism is wedded to and
perhaps motivated by Hobbes’s materialism. In late scholastic context the purported universality of the content of acts of intellect is at times referenced in proofs of the immateriality of the soul. Perhaps, because of this association of universality in thought with the immateriality of its faculty, Hobbes gives an account, relying crucially on the use of memory, in which universality in discourse does not imply universality in thought. In this paper, I examine Hobbes’s account of universality in detail, with some attention to its relation to scholastic approaches and its reception by other neoterics. Leibniz sees in Hobbes’s highly linguistic approach to scientia an unacceptably radical nominalism. Descartes is unimpressed by many of Hobbes’s criticisms, made in terms of this linguistic conception of reasoning and universality, in the Fourth Objections. My account of Hobbes on universality concludes by evaluating the merit of these two contemporary responses to Hobbes. Descartes is dissatisfied both with Hobbes’s restriction of ideas to sensory modalities and with his linguistic treatment of reasoning. Leibniz’s criticism is not unrelated to more recent commentators’ suggestion that Hobbes’s account makes definitions and so scientific truth arbitrary. However, recent attention to Hobbes’s philosophical psychology and an emphasis on the role of causal definitions has done much to diffuse these concerns. Nonetheless, I will argue, two difficulties remain for Hobbes’s account of universality and so of scientia: the first is connected to Hobbes’s account of the universality of names, the second to his account of universality in propositions.

The Category-Theoretic Space of Kant’s Imagination

Susan V. H. Castro (Wichita State University)

Even though the work of neo-Kantian David Hilbert was clearly one of the historical precursors from which category theory in mathematics was developed, Eilenberg and Mac Lane chose the word “category” only as a superficial tribute to Immanuel Kant. Obviously Kant’s categories of the understanding (e.g. plurality, causality) are not substantive precursors of the categories in mathematics today (e.g. Set for set theory), so the relation between Kant’s work and category theory is prima facie tangential and unworthy of attention. I argue that some features of category theory are more deeply rooted in Kant’s critical philosophy than has been recognized, and these more fully articulated descendants of Kant’s work may be used to illuminate some of the more opaque and puzzling problems in Kant scholarship. The problem I address in this paper is Kant’s claim that there can be only one space. The space occupied by real, physical objects that are given through outer intuition must surely be different from the space occupied by the merely imagined objects we give ourselves in intuition, yet pure geometric constructions are “imaginary” syntheses in intuition about real objects (A715/B743ff). Kant thus seems to both require and deny that space is “essentially single” (A25/B39). To reconcile the tension I argue that Kant’s notion of space as a “pure a priori form” is, or is the precursor of, the category of all structures that have the sorts of properties Kant mentions, e.g. adjacency. Given that Top and Vec are equivalent “up to isomorphism,” topological spaces and vector spaces are all “categorically” the same space. By analogy, if pure intuition is whatever is presupposed by or shared by Top and Vec, then Kant can coherently claim that the spaces of sensibility and imagination are literally the same yet different spaces. To make sense of the priority of space over spaces (A24/B39), I then appeal to Elaine Landry’s top-down, algebraic, in re structuralism - a category acts a “schematic type” that frames “what we say about the shared structure of abstract kinds of mathematical systems… without our having to specify what either kinds or types are ‘made of’”. From the top down any “two” spaces of cognitive activity, e.g. auditory or imaginative, are categorically identical because the identity and nature of a pure a priori form is determined only by formal/structural features. Just as transcendental ideas have a necessary use despite being empty and reason is a single faculty with
multiple uses (theoretical and practical), the very same space can have multiple uses which generate distinct spaces.

Berkeley's Theory of Quasi-Entities
- Kenneth Pearce (Valparaiso University)

Central to the project of Berkeley's De Motu (1721) is a distinction between two uses of language, which we may call 'genuine reference' and 'quasi-reference.' Genuine referring expressions, like 'red,' are used to label objects (red things) which exist independently of the sign system. Quasi-referring expressions are syntactically (and hence inferentially) just like genuine referring expressions, but differ semantically in that they do not label objects in the way genuine referring expressions do. The central thesis of De Motu is that the theoretical terms of physics are quasi-referring expressions. Thus Berkeley says quite explicitly that “'Force' . . . is used . . . as if it signified a quality” (De Motu, §5, tr. Clarke). What metaphysical consequences can be derived from this claim about language? Since Berkeley denies that “'Force', 'gravity', 'attraction' and similar terms . . . are useful for designating so many distinct qualities” (De Motu, §17), it might be thought that he holds that 'force' simply fails to refer, and this would seem to be a meta-linguistic way of saying there are no forces. However, I will argue that Berkeley does not hold this. The metaphysical upshot of Berkeley's linguistic distinction is not the denial that forces exist, but rather a particular view about how forces exist, or about what sorts of things they are. Berkeley's view is that forces are quite literally theoretical constructions: in the course of our theorizing activity, we create forces. Thus corresponding to Berkeley's linguistic distinction between reference and quasi-reference is a metaphysical distinction between two kinds of (really existing) things, which we may call 'genuine entities' and 'quasi-entities'. Genuine entities such as sensible qualities (i.e., for Berkeley, ideas) have a “stable essence in the nature of things” whereas quasi-entities such as forces “depend on the notion of the person who defines them” (De Motu, §67). I begin, in §1, by examining Berkeley's account of the error De Motu aims to combat: the view that theoretical terms “are used . . . to signify certain natures. . . which are not the objects of the senses” (De Motu, §6). In §2, I defend the above interpretation of the distinction between reference and quasi-reference and show how Berkeley employs this distinction against his opponents. Then, in §3, I examine the metaphysics of quasi-entities. I will be particularly concerned to show that Berkeley wants to preserve the claim that forces exist while denying that they exist prior to and independent of our theorizing activity. I conclude, in §4, with a brief discussion of some evidence from the Three Dialogues which suggests that Berkeley may also regard bodies (i.e., macrophysical objects) as quasi-entities created by a sort of ‘folk theory' of physics. If this hypothesis (which I defend in more detail elsewhere) is correct, it calls for a radical reorientation of our thinking about Berkeley's view of the relationship between commonsense and Newtonian science.

5.4 Contributed Papers: Reichenbach and the Philosophy of Science

Reichenbach vs. Russell: the Metaphysics of Induction
- Michael Shaffer (St. Cloud State University)

Hans Reichenbach's pragmatic treatment of the problem of induction in his later works on inductive inference was, and still is, of great interest. However, it has been dismissed as a pseudo-solution and it has been regarded as problematically obscure. This is, in large part, due to the difficulty in understanding exactly what Reichenbach's solution is supposed to amount to, especially as it appears to offer no response to the inductive skeptic. For entirely different reasons, the significance of Bertrand Russell's
classic attempt to solve Hume's problem is also both obscure and controversial. Russell accepted that Hume's reasoning about induction was basically correct, but he argued that given the centrality of induction in our cognitive endeavors something must be wrong with Hume's basic assumptions. What Russell effectively identified as Hume's (and Reichenbach's) failure was the commitment to a purely extensional empiricism. So, Russell's solution to the problem of induction was to concede extensional empiricism and to accept that induction is grounded by accepting both a robust essentialism and a form of rationalism that allowed for a priori knowledge of universals. So, neither of those doctrines is without its critics. On the one hand, Reichenbach's solution faces the charges of obscurity and of offering no response to the inductive skeptic. On the other hand, Russell's solution looks to be objectionably ad hoc absent some non-controversial and independent argument that the universals that are necessary to ground the uniformity of nature actually exist and are knowable. This particular charge is especially likely to arise from those inclined towards purely extensional forms of empiricism. In this paper the significance of Reichenbach's solution to the problem of induction will be made clearer via the comparison of these two historically important views about the problem of induction. The modest but important contention that will be made here is that the comparison of Reichenbach's and Russell's solutions calls attention to the opposition between extensional and intensional metaphysical presuppositions in the context of attempts to solve the problem of induction. It will be shown that, in effect, what Reichenbach does is to establish an important epistemic limitation of extensional empiricism. So, it will be argued here that there is nothing really obscure about Reichenbach's thoughts on induction at all. He was simply working out the limits of extensional empiricism with respect to inductive inference in opposition to the sort of metaphysics favored by Russell and likeminded thinkers.

From Peirce’s Abduction to Lipton’s Inference to the Best Explanation: How Two Historical Developments Fill the Gap

Mousa Mohammadian (University of Notre Dame)

The relationship between Peirce’s abduction and Lipton’s inference to the best explanation (henceforth IBE) has been viewed in opposite ways. Some have argued that they are basically the same and others think they are utterly different. I think that neither of these extremes is true. In this paper I argue that Lipton’s IBE is the natural result of Peirce’s abduction after two historic developments in the philosophy of science, i.e. emergence of the notions of underdetermination and the distinction between the context of discovery and the context of justification. The paper consists of four sections. §1. I briefly discuss three main interpretations of Peirce’s abductive inference: Abduction is a way of discovering new explanatory hypotheses. Abduction is a method of justification of explanatory hypotheses. Abduction leads to judgments about the comparative pursuitworthiness of rival explanations. §2. I propose my own reading of Peirce’s abduction. I argue that abduction is a double-phase inferential process. Phase one is inventing candidate explanations for the observed phenomenon. This is done by what Peirce calls insight and describes as a mysterious inborn mental faculty. Phase two is ranking the invented explanations based on economic considerations for further studies and tests. Then it is time for induction, i.e. empirical testing (and probable rejecting) of the explanation with the highest rank which is continued until we find the unique true explanation. §3. Here I show that: My view, with some qualifications, embraces the first and the third abovementioned interpretations. In particular, I argue that Reichenbach’s view of discovery—mentioned in his famous ‘context of discovery’—is to a great extent similar to the phase one of Peirce’s abductive inference. The goal of phase two—i.e. ranking candidate explanations—is determining the comparative pursuitworthiness of rival explanations. Justification is the function of induction and cannot be done by abduction at all. §4. I argue that by introducing Reichenbach context distinction and Duhem-
Quine underdetermination of theory by empirical data to Peirce's inductive inference, we naturally come to Lipton's IBE. Reichenbach famously argues that the context of discovery is a subject-matter of psychology but not epistemology. Now if we admit his context distinction and its immediate ramification, and if I am true that phase one of Peirce's abduction corresponds with Reichenbach's context of discovery, then it should be eliminated from epistemology of science altogether. This is exactly what happens in Lipton's IBE where no discussion about the invention of explanations can be found. And if we introduce the problem of underdetermination to Peirce's account, empirical test cannot eliminate all rival explanations but one. Therefore, in contrast with what Peirce imagined, having the highest rank and passing the empirical tests are insufficient for an explanation to be adopted as the only empirically adequate explanation. It also needs to be the best explanation—whatever "best" means here—among all its empirical equivalents. Thus, after introducing the context distinction and the underdetermination, and only after that, abductive inference becomes an inference to the best explanation.

The Reichenbach-Einstein Debate on the Geometrization of the Electromagnetic Field

- Marco Giovanelli (Universität Tübingen)

The 1958 English translation of Reichenbach's Philosophie der Raum-Zeit-Lehre (1928) was missing the long, technical Appendix entitled 'Weyl's Extension of Riemann's Concept of Space and the Geometrical Interpretation of Electromagnetism'. The only detailed analysis of the Appendix was provided in the late 1970s, by A. Coffa, who read it exclusively as a critique of Weyl's 1918 attempt to unify gravitation and electromagnetism. Unpublished letters between Reichenbach and Einstein suggest that the Appendix—despite its somewhat misleading title—should be read more broadly. In the spring of 1926, Reichenbach, after making some remarks on Einstein's newly published metric-affine theory, sent him a typewritten note offering what looked like his own attempt at a unified field theory. Reichenbach's unpublished note turns out to have been an early version of §49 of the Appendix, on which he was working at the time. Einstein's objections and Reichenbach's replies reveal that criticism of Weyl's theory was only part of the story. Reichenbach intended to provide a philosophical reflection of the very notion of 'geometrization' in physics. At the time, many believed that, after general relativity has geometrized the gravitational field, the obvious thing to do was to geometrize the electromagnetic field. To challenge this view, Reichenbach constructed a toy-theory establishing a connection between geometry and electricity which, he argued, was just as good as the connection that general relativity established between gravitation and geometry. Differently from general relativity, however, Reichenbach's theory did not lead to new results. Thus, Reichenbach could provide evidence that the geometrization of a physical field cannot be regarded per se as a physical achievement. As soon as Einstein understood the 'ironical' nature of Reichenbach's enterprise, he immediately agreed with him. As D. Lehmkuhl has recently shown, it was in this correspondence with Reichenbach that Einstein pointed out, for the first time, that general relativity had not geometrized the gravitational field. This paper suggests that the geometrization-issue was not just a spin-off of Reichenbach's 1928 monograph, but possibly the core message of the book. To support this claim the paper proceeds as follows. Describes the context in which Reichenbach decided to send Einstein a note on the geometrization of the electromagnetic field. Offers a reconstruction of Reichenbach's note, in which Reichenbach shows how to rewrite the general relativistic equations of motion in a way that also charged particles, under the influence of an electromagnetic field, follow their 'natural path' defined by a non-symmetric affine connection. Describes Einstein's initially skeptical, then approving, reaction to the note. Shows what Reichenbach's Philosophie der Raum-Zeit-Lehre looks like if read from the perspective of the Appendix. Finally, briefly analyzing Reichenbach's attitude towards
Einstein's distant parallelism field theory, emphasizes the differences that existed behind Reichenbach and Einstein's apparent agreement on the issue of 'geometrization'. From this episode, the paper draws two conclusions: Historically, the Einstein-Reichenbach correspondence inaugurated a philosophical reflection about the role played by geometric considerations in physical theories. Systematically, Reichenbach's theory shows (in spite of Reichenbach's intentions) the limits of any attempt to impose geodesic equations of motion to a non-universal force.

Friday - 24 June (Parallel Session VI)

Parallel Session VI (14:00-16:00)

6.1 Symposium: The Causal Powers Debate: Historical and Conceptual Perspectives

One current debate within contemporary scientific realism has a direct historical analogue, the debate over causal powers. Currently, against the backdrop of a Humean orthodoxy, there is a neo-Aristotelian movement aiming to re-introduce genuine causal powers into our metaphysics of science. Leading this neo-Aristotelian charge are Cartwright, Ellis, Mumford and Anjum, Chakravartty, and McKitrick (among others). Historically, however, the tables were reversed. Within the (so-called) Scientific Revolution, genuine causal powers were entrenched within an Aristotelian scientific image, and critics and innovators pursued metaphysically reductive and eliminative strategies that culminated in Hume's vicious broadside against objectively real and causally necessitating powers. Of course the scientific doctrines, details, and methodologies within the scientific image then and the scientific image now differ drastically. But the underlying formal and metaphysical structures pertaining to causal powers are not (or so we say), and many of the same considerations and concerns that animated the seventeenth-century debate are still applicable to today's (or so we say; this symposium will support that claim). A better understanding of the historical trajectory away from causal powers in the seventeenth-century, especially one informed by contemporary concerns and motivations, will illuminate new issues and challenges contemporary practitioners should confront in developing this philosophical debate. We see the issues surrounding the re-introduction of genuine causal powers as centering on an explanatory triad: powers; natural necessity; and causal laws. The papers in this symposium will engage fundamental conceptual issues across these three domains. The symposium will involve the "blending" of historical and conceptual perspectives and concerns. Two historians of science and philosophy will present papers that highlight issues directly applicable to the contemporary discussion. One concerns the ways Aristotelian and neo-Aristotelian metaphysics can accommodate the broadly mechanistic criticisms and concerns driving the seventeenth-century debate and, in many ways, underlying the neo-Humean position. The other analyzes historical presentations of arguments for occasionalism as case studies in the metaphysics of causal powers. It uses the historical examples to highlight issues that contemporary neo-Aristotelians need to engage to complete the re-introduction of a real metaphysics of causal powers. Two contemporary philosophers of science will present papers that look more closely at current understandings of the conceptual issue surrounding powers, albeit informed by what historians of science and philosophy teach us. One will involve comparing the conceptual structures involved in the historical and contemporary conceptions of powers and the historical and contemporary arguments advocating for them. The other engages with the important topic of dispositions and analyses of dispositions, which strikes at the heart of what many saw as the essential problem with Aristotelian powers talk, i.e. that they are explanatorily empty. By engaging with the variety of analyses of dispositional language, it allows us to see the extent to which Molière's
famous quip was accurate and indicative of a fundamental critique of causal powers. It is our hope that
the contemporary discussion regarding powers will be as enriched and enlivened by the historical
perspectives as historical research will be illuminated and informed by the contemporary perspectives.

**Aristotelian Powers and the Rise of Mechanism**
- Henrik Lagerlund (University of Western Ontario)

It is commonly held that part of the rejection of Aristotelianism and the rise of mechanism in philosophers
like Hobbes, Descartes and Boyle involved a complete rejection of causal powers. It has become clear
that the difference between the Aristotelian and mechanistic worldviews is not as clear cut as previously
thought. First of all, the rejection of causal powers was by no means clear and uniform in the seventeenth
century. Secondly, the Aristotelian tradition is much more diverse and nuanced than it was presented by
its seventeenth century opponents. The so-called via moderna contains a view of matter and a criticism of
powers not unlike that of the mechanical philosophers. This paper will present a more nuanced picture of
the Aristotelian tradition, which points to an obvious continuity between parts of the Aristotelian tradition
and the new mechanical philosophy of the seventeenth century. The paper will also point to contrasts
within the Aristotelian tradition itself. It will become clearer that the mechanism of the seventeenth century
was not as new and original as it has been taken to be. The paper will contain discussions of Aristotle,
Aquinas, Ockham, Buridan and Suarez as well as Hobbes and Descartes. Many of the neo-Aristotelian
conceptions of causal powers developed in recent analytical metaphysics are clearly related, if not
identical, to ideas of causal powers found in the later medieval Aristotelian tradition. A particularly
challenging issue facing contemporary philosophy is how to think about the nature of reality such that
things or events have causal powers. It seems in the present situation particularly useful to look at the
Aristotelian tradition in the Middle Ages that moved away from powers and compare its discussion of
these powers with the contemporary discussion to get a better handle on some of the questions that face
the neo-Aristotelian trend. This paper makes a gesture towards such a comparison.

**Occasionalism and the Critique of Causal Powers: A Cautionary Tale**
- Benjamin Hill (University of Western Ontario)

A key component of one important strand of contemporary scientific realism is the revival of causal
powers: Cartwright, Ellis, Mumford and Anjum, Chakravartty, and McKitrick (among many others) have
forcefully advocated a return to a neo-Aristotelian metaphysics of causal powers against reductive and
neo-Humean analyses of causal interaction. This paper will explore the lessons contemporary scientific
realists can learn from the seventeenth-century occasionalist critiques of causal powers as they build and
defend their neo-Aristotelian metaphysics. It will be shown that the same kind of argument used by the
early modern occasionalists against Aristotelian causal powers can be applied to the contemporary neo-
Aristotelians, mutatis mutandis, despite the great differences between early modern and contemporary
scientific practices and scientific knowledge. It has long been recognized by historians of philosophy that
occasionalism importantly influenced Hume's arguments against necessary connection. Of the four main
occasionalistic arguments, two will be examined in this paper, (a) the No Necessary Connection Argument
and (b) the No Transference Argument. It will be argued that both put metaphysical pressure on neo-
Aristotelianism. Hume's NNCA is commonly parsed in terms of being able to imagine any kind of
connection between events and, consequently, of conflating logical and natural necessity. It will be argued
here, however, that NNCA as developed by al-Ghazali and Malebranche is properly formulated in terms of
metaphysical necessity and possibility rather than logical necessity. As such, it puts a fair amount of pressure on realistic accounts of the epistemology of modality, whether understood in terms of a metaphysical intuition, metaphysical or conceptual analysis, or conceivability. But the bigger challenge posed by the NNCA, it will be argued, is metaphysical and concerns the truth-maker for realist propositions about metaphysical possibility. It will be argued that there is a vicious circularity at the heart of the truth-maker for such claims. The NTA poses a different challenge to neo-Aristotelianism. The NTA, which was Louis de la Forge's argument that neither motion nor motive force could be transferred between bodies if they were modes of bodies, requires that we clarify the metaphysics of the property-subject relationship. The problem concerns the formal features required for transference. These features impose constraints on the ontology of the quantity transferred that cannot be satisfied. The worry is somewhat similar to that developed by Phil Dowe (2000) concerning the identity of physical quantities within transference theories (pp. 55-59), yet importantly different because it emphasizes the ontological dependence particularized properties have on their subjects. It will be argued that the occasionalistic version of the NTA is more biting and less easily dismissed than the worries about identity Dowe raised. The lesson to be learned from this historical engagement is that the structure of the occasionalists critiques survives intact the application of them to contemporary science. It is hoped, then, that this can substantially inform contemporary attempts to revival causal powers and help to move the discussion of neo-Aristotelian scientific realism forward.

Resurgent Powers

Jennifer McKitrick (University of Nebraska, Lincoln)

The ontological status of powers has been debated for centuries. On some accounts, powers came to be regarded as illegitimate entities, relegated to the dustbin of history. But in the 21st century, we find a profusion of philosophical literature on powers, raising the question: If powers were in the dustbin of history, how did they get out? The question is, in part, a historical one. What happened in the 20th century to renewed interest in causal powers? Were the lessons of the past forgotten, or were they overcome? But answering the question also involves assessing the prospects of giving a conceptual analysis of powers-talk. Part of the answer is that people (philosophers, laymen, scientists) never stopped making disposition ascriptions: They continued to describe things as malleable, conductive, soluble, volatile, etc. If we regularly find ourselves and others in our linguistic community, making assertions about the dispositions of things, what should that mean for our ontology? Three alternatives come to mind. We could take these assertions at face value, and take them to commit us to the existence of dispositions. Or, we could adopt an error theory, concluding that all such assertions are false. Alternatively, we could try to explain how these assertion can be true consistent with the non-existence of dispositions – in effect to offer a semantic reduction of disposition ascriptions. It is generally acknowledged that philosophers have been unsuccessful at eliminating dispositional predicates from ordinary, philosophical and scientific discourse. This paper retraces the 20th century research program of analyzing disposition ascriptions, first in terms of material conditionals, then in terms of subjunctive or counterfactual conditionals. It explain why many deem this project to have failed. Since it seems that disposition ascriptions cannot be eliminated from our language, many have concluded that dispositional predicates refer to real dispositional properties. But perhaps there has been an over-reaction to the failure of the conditional analysis. On the author's view, there is an important connection between dispositional predicates and counterfactuals: To ascribe a dispositional predicates is to gesture at a range of counterfactuals that are true of that thing. Dispositional predicates, like many predicates, are vague, so it is not clear which counterfactuals are true of an object that said to have a particular disposition, and there may be borderline
cases. So, while disposition ascriptions are not reducible to counterfactual statements, a number of counterfactuals are true of disposed objects. However, this view is consistent with a number of different truth-makers for those counterfactuals. Even if a disposition ascription is true, it does not follow that some entity instantiates an irreducible power. A view called “dispositional pluralism” allows that things can have dispositions in virtue of its non-dispositional properties, its dispositional properties, the properties of other things (both dispositional and non-dispositional), combinations thereof, or not in virtue of anything, but as a brute fact. How do we determine, in particular cases, which of these is the truth-maker for a disposition ascription? Typically, that is a question that is best addressed by empirical science.

6.2 Symposium: Carnap and Quine on the Analytic/Synthetic Distinction

Quine initially sought clarification on Carnap’s account of ‘analyticity’ in his Truth by Convention (1936). In 1940s, his correspondences with Carnap and Tarski in particular made him more skeptical about ‘analyticity’. By Two Dogmas (1951), Quine took the radical step of attacking the intelligibility of the analytic/synthetic distinction. He famously rejected the dichotomy as an unmotivated dogma of empiricism. Although, the received view favours Quine in the debate between him and Carnap, scholarship especially from 1990s challenges this interpretation and instead proposes a more balanced understanding of the issue (Stein 1992, George 2000, Creath 2007, Lavers 2012). The purpose of our symposium is to contribute to the ongoing literature in favour of the more balanced works. The first paper, Carnap, Einstein, and the empirical foundations of space-time geometry, explains how a sharp distinction between the analytic and the synthetic informed Einstein’s view of the relation between abstract geometrical structures and the physical geometry of space, and therefore his view of space-time structure as a matter of empirical investigation; it suggests that Carnap’s understanding of the empirical content of physical theory was inspired by Einstein’s account, and that some central difficulties of Carnap’s view—and some possible approaches to their solution—can be understood through their connections with Einstein’s view. The second paper Carnap, Quine, Analyticity and the foundations of mathematics will concern two issues that Quine considered to be intimately connected for Carnap: the analytic/synthetic distinction and ontology in logic and mathematics. The paper will trace the developments of Carnap’s view on logico-mathematical portions of language from the time of his Logical Syntax of Language (LSL 1934) to his mature view in 1950s. According to the paper, Quine mistakenly read Carnap’s early instrumentalism of LSL into Carnap’s mature view on the foundations of mathematics which instead favoured realism (without endorsing a realist ontology). The third paper The Evolution of Quine's Revisability Thesis vs. Carnap explores the development of Quine’s revisability thesis—according to which, all beliefs are revisable in principle. Quine’s holism will be considered as an argument against Carnap’s distinction. It will be shown that Quine’s holistic epistemology becomes less radical post-Two Dogmas and does not really challenge Carnap’s relativized and revisable distinction. The final paper What Are the Prospects for Analyticity concerns Quine’s empirical challenge to Carnap. The paper provides surprisingly favourable prospects for specifying behavioral criteria (in principle) for an empirical conception of analyticity. Quine’s challenge to Carnap will be developed in a way that shows exactly what the demands are, to what extent the demands are legitimate for Carnap, and how Carnap could meet them. The papers are intended to contribute positively to the recent scholarship and it is hoped that many misunderstandings between Quine and Carnap are clarified in favour of a more balanced appreciation of the debate.
Quine famously described his principal disagreements with Carnap as concerning analyticity and ontology. Quine also thought Carnap's positions on the two questions were intimately connected. Nowhere is this more so than in the philosophy of mathematics. Here Carnap, as Quine saw things, wished to use the concept of analyticity to help himself to a platonistic universe of types without paying the ontological price. Quine, despite his eventual abandonment of platonism, was very reluctant to accept any abstract objects and saw Carnap's position as illegitimate. At the time of The Logical Syntax of Language, Carnap held an instrumentalist view towards the logico-mathematical portions of the language. This paper will show that Quine clearly read such an instrumentalism into Carnap's later position. Carnap's position on the foundations of mathematics will be traced from the early position of LSL to the position he held in the fifties. It will be shown that Carnap's position in the fifties is far more realist than instrumentalist. In fact, in as much as the position of the platonist can be clarified to Carnap's satisfaction, Carnap would happily side with the platonist (of course as a metaphysical position, Carnap would clearly reject platonism). Certain questions concerning the existence of mathematical entities, Carnap dismissed as being insufficiently clearly formulated. But Carnap does not, as Quine interprets him, employ the concept of analyticity to dismiss any ontological questions. Far from trying to defend some version of instrumentalism, as Quine supposed, Carnap really was trying to free empiricism from its nominalistic scruples.

The Evolution of Quine's Revisability Thesis vs. Carnap

Quine famously attacks Carnap's analytic/synthetic distinction in the Two Dogmas (1951). His major challenge to Carnap is to make ‘analyticity’ intelligible. This is because, for Quine, the concept of analyticity is overly vague as an explicandum (or as a pre-theoretic term) and a formal systematic account of the notion is possible only after we have a reasonably clear ordinary idea about analyticity (1951, 1954/1963). Until then, Carnap's proposal for a rational reconstruction of analyticity remains unilluminating and the dichotomy between the factual (or synthetic) and the formal (or analytic) rests on an unmotivated dogma. In order to meet Quine's demands, Carnap has to provide an experimental meaning of ‘analyticity’. In other words, before Carnap can begin to carry out a genuine explication, Quine requires ‘analyticity’ to be expressible empirically or in terms of “linguistic behavior” (1951). Contrary to the Quine-favored received view on the debate, scholarship especially after 1990s stresses the misunderstandings between Quine and Carnap (Carus 2007, Lavers 2012), and the literature suggests a more balanced approach to the debate (Stein 1992, George 2000, Creath 2007). It is noted that by 1970s, Quine himself accepts an extremely narrow sense of analyticity (Creath 2004). Additionally, using Quine's epistemology, a wider sense of analyticity is developed to account for arithmetical truths (Lavers 2012). While Quine's original conception of analyticity does not play the heavy duty epistemic role needed by Carnap, the viability of an empirical conception of analyticity suggests that Quine's challenge is satisfiable even if Carnap thought it was irrelevant. For my talk, I wish to focus on a closely related aspect of the debate. It is sometimes referred to as Neurath's “universal revisability” thesis (Uebel 1992), but it is now mainly associated with Quine (Creath 2007). The thesis says in effect that any truth can be held “true come what may” so long as we are willing to make enough changes to our “web of beliefs” (Quine 1951, 1970/1978). Alternatively, any truth is in principle revisable, which for Quine means the abandonment of the dichotomy and the adoption of a new holistic epistemology. I will argue that Quine's revisability thesis becomes less radical as it
develops post Two Dogmas. Moreover, I show interesting similarities between Quine’s mature treatment of “core beliefs” (1974, 1990/1992) and Carnap’s revisable a priori beliefs. First, I motivate and clarify the changes of Quine’s thesis from Two Dogmas to 1960s and 1970s. Second, I develop Carnap’s revisable conception of his analytic/synthetic distinction and reveal important similarities with Quine’s opposing thesis. Finally, I suggest that Quine’s moderate and mature version of the thesis is less likely to pose a threat to Carnap’s dichotomy. I do this by showing a weakness of Quine’s argument stemming from a misleading characterization of analyticity. I hope that by clarifying the relationship of Quine’s evolving thesis and Carnap’s relativized and revisable distinction, a better and more balanced appreciation of the ongoing Quine/Carnap debate is gained.

**What Are the Prospects for Analyticity?**

- Richard Creath (Arizona State University)

As a preliminary, this paper explores the nature of Quine’s challenge to Carnap’s notion of analyticity and asks how much of this challenge poses a legitimate problem for Carnap’s view. But the primary focus of the paper is on whether the legitimate part of that challenge can be met. If the challenge is to provide behavioral criteria for an empirical notion of analyticity, then to what sort of behavior ought we be looking? And what are the prospects (in principle) for finding such criteria? In two words, the answer to this last question is: surprisingly good. No attempt will be made to provide detailed criteria. Rather it will be argued that analyticity is not worse off than many other notions about language, the legitimacy of which Quine does not challenge.

6.3 Contributed Papers: Neo-Kantianism

**Cassirer, Kaila, and Helsinki Realism**

- Matthias Neuber (University of Tübingen)

In 1910, Ernst Cassirer published his influential monograph Substanzbegriff und Funktionsbegriff. In that book, Cassirer argued for an ‘invariantist’ conception of objectivity. According to this theory, scientific statements and laws are the more objective the more invariant they are. As an example, Cassirer in his 1921 “Zur Einsteinschen Relativitätstheorie” discussed the principle of general covariance as the most objective – since it’s the most invariant – principle of General Relativity. Programmatically, he intended to argue for what he called “logical idealism.” Interestingly enough, Eino Kaila, implicitly relying on Cassirer, argued for an invariantist conception of objectivity as well. However, his aim was not to strengthen idealism, but rather what he called “critical realism.” His case in point was the theory of measurement that, in his opinion, could only be interpreted in realistic terms. This Kailaian conception of the 1930s and 1940s, in turn, was the smoking gun for the representatives of “Helsinki Realism.” Especially Raimo Tuomela (1973) and Ilkka Niiniluoto (1999) attempted in their respective writings at defending a “critical scientific realism” that they initially intended as an answer to C.G. Hempel’s “Theoretician’s Dilemma” (1958). Yet although very close in spirit to Kailaian critical realism, both Tuomela and Niiniluoto eventually left open the question of their ontological commitment. As will be argued in the paper, Kaila’s original – measurement-based – ‘invariantism’ is capable of bridging this gap. In short, it’s invariant structures that are detected and objectively determined by executing measurements. Accordingly, the physically “real” is to be equated with (mind-independent) invariant measurable structures and thus conceptualized within a naturalistic setting. The resulting position may be called ‘metrological structural realism.’ By adopting this position, Helsinki Realism can be defended against both scientific antirealism.
and metaphysical realism. On the whole, the impact of Kaila's philosophical point of view will be accorded greater detail.

**Cassirer on the Teleology of Mathematics and Science**

- Samantha Matherne (University of California, Santa Cruz)

In the Philosophy of Symbolic Forms, Ernst Cassirer presents his account of mathematics and science only after he has analyzed the other so-called 'symbolic forms' of culture, including myth, religion, and language. Whether this organizational choice reflects a deeper philosophical commitment is a matter of some debate: whereas some commentators argue that Cassirer holds a teleological view of culture, such that mathematics and science are what the other cultural forms progress toward (Friedman, Moss), others deny that teleology plays a role in his approach (Verene, Lassengue, Skidelsky). Although I am in agreement with those who attribute a teleological view of culture to Cassirer, in this paper, I argue that in order to account not only for his theory of culture, but also for the place of mathematics and science in it, we need a revised understanding of what this teleology amounts to. More specifically, I claim that we should resist the tendency among commentators who endorse the teleological interpretation to identify the end of culture as the consciousness of culture we achieve through philosophy. I object that this conception of teleology commits Cassirer to privileging philosophy over the other symbolic forms in a way that he would not endorse. Moreover, I demonstrate that there is different end that Cassirer proposes as the end that organizes the progress of culture, viz., the end of uniting human beings. According to Cassirer, this is an end that all the symbolic forms share and it is the one that orients the progress between the forms, as we advance from forms that are more divisive, e.g., myth and religion, toward forms that are more universal in nature, e.g., mathematics and science. In order to develop my interpretation, I divide my paper into two parts. In the first part, I raise objections to the standard interpretation of Cassirer's teleology of culture and I then defend my alternative interpretation, examining what the end of uniting human beings amounts to and how this shapes Cassirer's account of culture more generally. Meanwhile, in the second part, I focus on the implications this has for our understanding of Cassirer's views of mathematics and science. I claim that there are several features of his account of mathematics and science that we cannot fully make sense of unless we take their teleological orientation towards this end into account, viz., his analysis of how mathematics and science are both based upon, but nevertheless break free from the other cultural forms; his response to the foundational crisis in mathematics, specifically, his position regarding the possibility of the unification of intuitionism and formalism; and his explanation of convergence in the progress of science. A revised understanding of his conception of the teleology of culture thus promises to shed light not only on Cassirer's theory of culture, but also on his distinctive approach to mathematics and science as the highest achievements of it.

**The Origins of Hermann Cohen’s Logic of Pure Knowledge**

- Scott Edgar (St. Mary's University)

Hermann Cohen defended his mature theory of knowledge in his 1902 The Logic of Pure Knowledge. One of the work's most striking features is the idea Cohen underscores in the book's title: his insistence that his theory is a logic of pure knowledge. Cohen intends the term “logic” to signify that his theory abandons Kant's distinction between independent faculties of sensible intuition and understanding: having rejected Kant's view that a philosophical account of knowledge requires a theory of sensibility, all that remains is a logic of our rational faculty. However, the origins of this move are much earlier than Cohen's 1902 Logic, in
his 1883 Principle of the Infinitesimal Method and its History. In this paper, I argue that, while in the
Infinitesimal Method Cohen did not yet understand himself to be rejecting the Kantian theory of an
independent faculty of sensible intuition, he nevertheless makes his first, decisive moves towards that
position. Kant had maintained that an independent faculty of sensible intuition was necessary to explain
two distinct epistemic functions: singular representation and the representation of infinities and continua
in mathematics. And at least on the surface, Cohen in the Infinitesimal Method seems still to accept Kant's
distinction between "intuition and thinking." However, I argue that Cohen makes two moves that
effectively collapse Kant's distinction between independent faculties of sensible intuition and
understanding. First, implicit in the view Cohen articulates in the Infinitesimal Method (as well as the
second, 1885 edition of his Kant's Theory of Experience) is a view that explains singular representation by
appeal to the representation of infinites and continua in mathematics. On this view, mathematically-precise
natural science represents individual objects by placing them at unique locations in a four-
dimensional manifold of mathematical magnitudes that are in turn (Cohen thinks) defined by appeal to continuous
magnitudes. But then second, one of Cohen's principal aims in the Infinitesimal Method is to defend the
view that mathematics' representation of continuous magnitudes -- that is, infinitesimals -- must be
explained by appeal to the principle of continuity. I argue further that, for Cohen, the principle of continuity
is a purely rational principle. Consequently, on Cohen's view both singular representation and the
representation of infinites and continua are explained not by appeal to an independent faculty of intuition,
but to a purely rational principle. Thus despite the fact that Cohen retains Kant's language of independent
faculties of "intuition and thinking," his view has the effect of abandoning the idea of an independent
faculty of sensible intuition. Cohen thus takes a decisive step towards his mature conception of logic.

(Neo-)Kantian Frameworks for the Foundations of Quantum Mechanics
• Michael Cuffaro (LMU München)

It is commonly held that Kant's system of metaphysics was definitively refuted by the developments at the
turn of the last century in mathematics and natural science. For example, quantum theory is supposed to
have invalidated Kant's belief in the synthetic a priori status of the principle of causality. Yet a growing
body of recent scholarship has argued that the truth is more subtle—that in fact the founders of quantum
theory, particularly Niels Bohr, were substantively influenced by Kantian ideas. This opinion is not
universal, however, and I will begin my talk by arguing in favour of a connection between Kantian ideas
and Bohr's. Specifically, I will argue that Bohr's view, that quantum concepts do not pick out objects in the
ordinary sense of the word, can be grounded in Kant's claim that phenomenal experience is not objective
experience unless it is fully determinable in accordance with his 'principles of the pure
understanding' (which include causality). While the connections between Kantian ideas and Bohr's are
only implicit, other thinkers of the period were quite explicit about the links between Kantian metaphysics
and their own views on quantum mechanics. Werner Heisenberg viewed the development of quantum
theory as having shown that Kant's system of metaphysics, including his principle of causality, is
indefensible. However for Heisenberg the metaphysics that should replace it represents a transformation
of Kantian philosophy rather than a total rejection of it. For Grete Hermann, Kant's principle of causality is,
contra Heisenberg, not a predictive but a retrodictive principle; i.e. it merely posits that every succession
of appearances is determinable causally according to it. The principle in this form is not violated by
quantum mechanics, and for Hermann this teaches us that the proper interpretation of quantum
phenomena is one which is relative to the concrete particular outcomes of previous experiments. Finally,
the development of quantum mechanics led Ernst Cassirer—perhaps the most famous student of the
Marburg Neo-Kantian school of thought of Hermann Cohen—to argue later on in his career that the
methodology of physics requires radical alteration. Still, Cassirer holds on to his own particular conception of the relativised a priori that remains at least rooted in Kantian metaphysics. For one generally sympathetic to Kantian metaphysics, all of the foregoing ideas represent different alternatives for comprehending the foundations of quantum mechanics from within a broadly Kantian worldview. But I will argue that it is Bohr's extension of the Kantian Framework which is both most consistent with the spirit of Kantianism and most consonant with modern physics. Further, Bohr's philosophy of 'complementarity' offers a way to illuminate the ways in which modern science in general can be said to be disunified—for Bohr, different aspects of one and the same (not necessarily quantum) phenomenon can be described in contradictory ways if one takes such descriptions completely literally—while still remaining coherent, since for Bohr such descriptions can nevertheless be made compatible if we are willing to accept their inherent limitations.

Friday - 24 June (Parallel Session VII)

Parallel Session VII (16:30-18:00)

7.1 Contributed Papers: Philosophy of Mathematics

19th century roots of Suppes’ semantic conception

- Wilfried Sieg (Carnegie Mellon University) and Aeyaz Kayani (Carnegie Mellon University)

The semantic conception of scientific theories has many versions; one of the best known is that of Patrick Suppes. His formulation crucially relies on a notion of axiomatization that is at variance with the standard syntactic conception. Indeed, axioms are viewed as “characteristic or defining conditions” of an abstract concept concerning sets and, thus, (axiomatized) scientific theories are considered to be “set-theoretic predicates”. The parallel to Nicolas Bourbaki’s way of articulating mathematical theories by structural definitions is quite clear, and Suppes acknowledges Bourbaki’s deep influence. But does this influence result in more than adapting the structuralism of modern mathematics to empirical science? Suppes’ own remarks lead one to conclude that, indeed, the mathematicians’ ways of organizing their subject should be taken over in order to obtain manageable and intelligible scientific theories. However, Bourbaki’s perspective was shaped most directly by the structural axiomatic work done in Germany in the 1920s, especially, in Göttingen by David Hilbert, Emmy Noether, Helmut Hasse, van der Waerden and many others. The axiomatic method as understood by them was mainly concerned with the introduction of appropriate concepts for sets of mathematical objects whose mutual relationships were fixed by axioms. The early paradigm of this form of the axiomatic method was Hilbert’s Foundations of Geometry (1899). It is a surprisingly semantic method, as the notion of a realization (i.e., a set falling under the axiomatically given concept) corresponds to Suppes’ central notion of a model; this way of specifying a realization is still used in Gödel’s doctoral dissertation. There is consequently a profound similarity between Suppes’ semantic conception, on the one hand, and certain methodological views spanning mathematics and the sciences, on the other hand; the latter were already present towards the end of the 19th century. They include not only Hilbert’s views on the axiomatization of mathematics and physics, but also those of Richard Dedekind and Heinrich Hertz that strongly impacted Hilbert. The concerns that animate discussions in philosophy of science today and that prompt the adoption of a semantic conception were prominent in the works of these scientists. There is a clear trajectory of transfer of ideas between such 19th century views and the semantic conception advocated by Suppes most comprehensively in [4] for the 21st century. We examine the systematic connections and bring to light the underlying historical
influences. From a more philosophical standpoint we emphasize, as Suppes did, “the continuity between pure and applied mathematics, or between mathematics and science”.

The Ontogeny of Quine’s Ontology: Nominalism, Pythagoreanism, Platonism

- Greg Frost-Arnold (Hobart and William Smith Colleges)

I attempt to extend our understanding of Quine’s development on two fronts: Quine’s brief attempt to vindicate Pythagoreanism, and why Quine renounced nominalism. First, as Mancosu (2008) brought to light, Quine kept a (currently unpublished) personal notebook during the 1930s and 40s, which Quine labeled ‘Logic Notes.’ In one section of it, Quine outlines a planned book called Arithmetic and Nature that would defend a view he calls ‘neo-Pythagoreanism’: all that exists is the natural numbers; a neo-Pythagorean need not assume sets or physical objects. I describe and analyze this position, which reduces sets to the natural numbers via appeal to the downward Löwenheim-Skolem theorem, and reduces physical objects to natural numbers via Carnap’s use of a ‘co-ordinate language.’ I pay particular attention to Quine’s professed motivations for this (strange) project. Second, I investigate hypotheses to explain (at least in part) Quine’s shifting ontological positions. I focus primarily on why Quine abandons nominalism shortly after publishing 1947’s “Steps Towards a Constructive Nominalism” (co-authored with Nelson Goodman). The usual explanation for Quine’s shift from nominalism to Platonism one hears is: parts of classical mathematics cannot be captured within a language that meets nominalist strictures. But this cannot be the whole story (it is necessary but not sufficient to explain Quine’s shift): first, reconstructive projects often take decades of work, and the collaboration of many researchers—think, for example, of the logicist project. Second, and more importantly, Quine was fully aware that a nominalist language (probably) could not capture all of classical mathematics throughout the period 1935-1947—so we need an explanation for why he abandoned nominalism in 1948, instead of one of those 12 earlier years. Here is my hypothesis for why Quine renounced nominalism in 1948: he stopped believing that nominalism exhibited what he had previously considered its chief theoretical virtue, namely ‘clarity’ or ‘intelligibility.’ And Quine stopped believing that because either (i) Quine dropped clarity as a fundamental, irreducible theoretical virtue, or (ii) he still considered clarity a fundamental theoretical virtue, but stopped believing that physical objects were paradigms of clarity (in part because of quantum mechanical considerations).

Frege’s Begriffsschrift notation: Design Principles and Trade-offs

- Dirk Schlimm (McGill University)

Well over a century after its introduction, Frege’s two-dimensional Begriffsschrift notation is still considered mainly a curiosity that stands out for its clumsiness rather than anything else. It stands out by its two-dimensional layout with symbols for logical relations (implication and negation) on the left and the propositional content on the right. In this paper the propositional fragment of the notation is looked at in detail. In the first part, several idiosyncrasies of the notation, which allow an easy conversion of logically equivalent formulas, are discussed and by showing its close connection to syntax trees I argue for the perspicuity and readability of the notation. In the second part, the aims that Frege pursued with his system together with his considerations regarding possible difficulties with the notation because of its unfamiliar look are presented. In addition, Frege’s justifications for the design principles underlying the Begriffsschrift are discussed, about which was very explicit in his replies to early criticisms and unfavorable comparisons with Boole’s notation for propositional logic. Despite the fact that this discussion is mainly about
Begriffsschrift, it highlights some important trade-offs with regard to notations in general. The issue of familiarity is not about the notation per se, but about the historical and psychological context in which it is introduced. The historical development of Begriffsschrift shows that in some cases a notation that looks familiar has considerable advantages, e.g., readers are more likely to engage with it and previous knowledge can be transferred. In contrast, such familiarity might also lead to erroneous or unjustified analogies and ambiguities. Another important theme that emerges from the discussion is the trade-off between brevity and perspicuity. At a very basic level, shorter expressions are easier to grasp, but when it comes to more complex subject matters the situation is not so clear-cut any more. Brevity can also be achieved by adopting additional implicit conventions (e.g., regarding the binding strength of connectives to avoid parentheses), but these in turn require additional effort to learn. Frege’s reflections demonstrate that he was well aware of many of these issues that surround the design of convenient notations. In sum, my discussion reveals that Begriffsschrift is in fact a well thought-out and carefully crafted notation that intentionally exploits the possibilities afforded by the two-dimensional medium of writing like none other.

7.2 Contributed Papers: Measurement and Realism

Rethinking Duhem’s Antirealism

- Meghan Page (Loyola University Maryland)

Pierre Duhem is often interpreted as an antirealist. And with good reason. His most popular work, Aim and Structure of Physical Theory, is brimming with disavowals of scientific explanation. “A physical theory is not an explanation. It is a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely, and as exactly as possible a set of experimental laws” (Duhem, 1962). Additionally, Duhem calls for a distinction in physical theory structurally similar to the contemporary distinction between observables and unobservables. “When we analyze a theory created by a physicist who proposes to explain sensible appearances, we generally do not take long to recognize that this theory is formed of two really distinct parts: one is the simply representative part which proposes to classify laws; the other is the explanatory part which proposes to take hold of the reality underlying the phenomena” (Duhem, 1962). Finally, despite Perrin’s experimental confirmation of atomism in 1908, and acceptance of atomism by the general scientific community thereafter, Duhem resisted atomism until his death in 1916. Philosophers such as Cartwright and Psillos have woven these threads together into a van Fraassen-esque tapestry. Duhem’s renunciation of explanation and spurning of atomism are seen as consequences of his general distrust of unobservable entities. I argue that this interpretation is ahistorical; Duhem had no interest in drawing a distinction between observables and unobservables. More importantly, this standard interpretation obscures Duhem’s actual concerns about scientific theory, which trace back to worries about solvability. “Atomic” theories, such as the kinetic theory of gasses, are notorious for generating unsolvable integrals. Consider, for example, the three-body problem that arises in Newton’s formulation of classical mechanics. Although we can calculate the trajectory of a single body’s motion through time, it is impossible to integrate (without the use of simplifying assumptions) the trajectories of three or more bodies interacting. Duhem’s contemporary, Poincaré, was the first to note that for certain formulations of the three body problem, there are no analytic integrals. Unsolvable integrals are also generated for systems with multiple constraints. LaGrange was able to provide a more user-friendly formulation of classical mechanics in order to handle constraints, at the cost the atomic detail provided by Newton. Duhem espoused great admiration for the work of LaGrange, and took his formulation of classical mechanics as an ideal model of theory. Good theories, according to Duhem, are able to predict and describe the behavior of a large number of systems.
Atomism, due to the difficulty of the integrals it produces, fails to accomplish this task. I argue it is these worries about the nature of scientific theory that motivate Duhem's philosophical views, rather than epistemological worries about “unobservables.” Perhaps Duhem’s most significant contribution to the philosophy of science remains unnoticed: too much attention to microscopic detail may circumvent our ability to effectively describe and predict the behavior of macroscopic systems.

The Structure and Epistemic Import of Empirical Multiple Determination: The Case of Jean Perrin

Klodian Coko (Indiana University)

Experimental Multiple Determination (multiple determination, for short), is the ability to establish the same result by means of independent experimental procedures. It is considered to be an important epistemic strategy by both philosophers of science and practicing scientists (Wimsatt 1981; Hacking 1981, 1983; Cartwright 1983, 1991; Franklin 1986, 1998; Jardine 1986, 1991; Bechtel 1990, 2002, 2006; Culp 1994, 1995; Burian 1997; Chalmers 2003; Nederbragt 2003; Weber 2005, to mention only a few). Despite this heavy appeal to multiple determination, however, not much analysis has been provided regarding the specific grounds upon which its epistemic virtues rest, besides a very blunt rationale; namely, that it would be improbable for multiple procedures to establish the same result and yet for the result to be incorrect. Jean Baptiste Perrin’s convincing argument for atomism in the beginning of the 20th century is often cited as the paradigmatic case of multiple determination (Hacking 1983, Kosso 1988, Cartwright 1991, Culp 1994, 1995, Woodward 2006, Stegenga 2009, and others). The most famous part of Perrin’s argument was his description of thirteen different procedures for determining Avogadro’s Number (the number of molecules in a gramme-mole of a substance). These procedures included Perrin’s own determinations which were based on the study of the height distribution, mean displacement, and mean rotation of Brownian particles (Perrin 1909, 1913). But although Perrin’s argument is presented as the paradigmatic case demonstrating the epistemic force of experimental multiple determination, the rationale underlying this epistemic force is simply a reiteration of the blunt rationale; namely, it would be highly improbable if thirteen procedures pointed at the same value for the number of molecules in a unit of substance and yet there were no such things as molecules. Not much analysis is devoted to the role that experimental multiple determination played in Perrin’s actual experimental work or in convincing the scientific community. Further, independently of the discussion on multiple determination, Perrin’s case has been the subject of, more or less, detailed case-studies from philosophers’ of science which aim exactly at capturing the reasoning underlying Perrin’s argument for molecular reality. The various philosophers, however, have arrived at very different and often contradictory conclusions, not only regarding the structure of Perrin’s argument in general, but also about the role that the experimental multiple determination of N played in it (Glymour 1975, 1980, Salmon 1978, 1984, Cartwright 1983, Mayo 1986, 1996, van Fraassen 2009, Psillos 2011, Hudson 2013). In my presentation I will analyze the role that Experimental Multiple Determination played in Perrin’s experimental work on Brownian movement and in his general argument for molecular reality. The distinctive feature of my approach is that it emphasizes the historical context but also the temporal development of Perrin’s thought. Following this approach, I locate the main elements underlying the success of Perrin’s argument for molecular reality. I argue that Perrin’s argument was indeed an argument from coincidence which was based on the agreement between independent determinations of the numerical values for N (and, consequently, other molecular magnitudes. What made the agreement remarkable and gave rise to an extremely strong argument from coincidence was: the fact that it was a quantitative agreement which concerned numerical values, the large number of determinations which converged at the same numerical result (thirteen were enumerated...
in Perrin's version of argument presented in Les Atomes), the fact that it was an extremely close agreement if one took into account the possible numerical values for the molecular magnitudes that could be calculated by each one of the different determinations, the fact that each two of the determinations were theoretically independent, being based on independent theoretical assumptions, the fact that the determinations were genetically independent and no effort was made to mutually adjust the values calculated, the fact that the determinations were based on the investigation of unrelated phenomena, the high quality and reliability of some of the determinations the fact that, despite there being a large number of calculations, there was not even one discordant result the fact that when objections and results which challenged Perrin’s determinations of molecular magnitudes Perrin was able to convincingly resolve the discordance. Finally, I will argue that the conclusions from Perrin's and other cases of multiple determination can be used in order to develop a general framework for understanding the structure and epistemic force of arguments which rely on multiple determination.

**Poincaré and Renouvier on Conventions in the Sciences: A Comparison**

- Warren Schmaus (Illinois Institute of Technology)

No single person created conventionalism. Nye (1979), Giedymin (1982), and Pulte (2000) regard Poincaré’s conventionalism as part of an intellectual movement that arose in nineteenth-century France in response to Kant’s philosophy and to developments in mathematics and the sciences. Nor was Poincaré the only one who used terms like “convention” to characterize the sciences. Nye cites Perrin’s use of the term in 1903, and as early as 1885 Renouvier characterized the sciences as resting on conventions. However, my aim here is not to establish priority. Rather, it is to compare Poincaré’s and Renouvier’s views on the roles of conventions in science, the concepts of space and time, and synthetic a priori judgments in order to enrich our understanding of this intellectual movement. De Paz (2014) finds seven different senses of “convention” in Poincaré, including implicit conventions in physics as well as explicit ones in mathematics. She also asks how the idea of a social contract could actually apply to science. Renouvier, who was a political philosopher as well as a philosopher of science, suggested an answer. He wrote that in order for philosophy to ever become a science, it would have to adopt “a sort of social contract” to regulate discussion among philosophers, implying that the sciences rest upon such contracts (Renouvier, 1873). He further explained that a science depends not just on a method but on a “convention” of not questioning particular starting points (Renouvier, 1885). Many of these excluded questions overlap Poincaré’s types of conventions, such as the parallel postulate, the definitions of basic concepts in mechanics, and the principle of inertia. But others are more philosophical, such as whether space, time, and continuous quantities exist outside the phenomenal world in things in themselves, and whether there are souls that influence the motions of living bodies. After a science has achieved some successes, some scientists may want to return to some of these excluded questions and take them up again. Consider, for example, the parallel postulate. Like Poincaré, Renouvier favored Euclidean over non-Euclidean geometry and even the “general geometry” that included both. He also rejected Helmholtz’s geometrical empiricism, considering the parallel postulate a revisable synthetic a priori principle. Not wanting to repeat Comte’s error of legislating what are acceptable areas of research, Renouvier granted mathematicians the freedom to pursue non-Euclidean geometry, with the hope that they may find something useful (Renouvier, 1889). Renouvier regarded the social conventions that guide scientists as living, changing agreements, much like the social contracts that govern societies at large. That they rest on mutual agreement in both cases is the source of and explains their normativity.
7.3 Contributed Papers: Descartes and Gassendi

The Scientific Basis of Cartesian Sensory Skepticism

- Anthony Crifasi (Benedictine College)

One of the most hotly contested points in scholarship on Cartesian philosophy over the past century has been the precise role of skeptical arguments in Descartes' thought. According to one interpretation, the purpose of these arguments was not to establish skepticism, but actually to overcome it, by pressing doubts to such an extreme that the mind is driven back to absolutely indubitable foundations. Others have argued, by contrast, that Descartes did indeed intend to establish skepticism itself as a conclusion, since his mechanistic physics undercuts any necessary resemblance between sensation and its objects. I propose a defense of the latter interpretation. First, I will argue that Descartes did provide a scientific argument for sensory skepticism, from is new brain-based sensory physiology. His first claim in this argument is that the occurrence and content of sense perceptions are occasioned by only one organ – the brain. I will argue that the evidence that he provides for this claim consists only of macroscopic observations that were easily testable at the time, not hypotheses imported from his mechanistic physics. In particular, Descartes appeals to observable correlations between impairments to various organs and the loss of the ability to sense. The second claim in Descartes' physiological case for sensory skepticism is that nothing resembling the content of sense perception ever reaches brain. I will argue that although some of his arguments in support of this claim are imported from his mechanistic physics, others are not. In particular, Descartes argues that other philosophers cannot explain how “images” resembling external objects could possibly be transmitted by nerves to the brain. A survey of parallel texts will reveal that by “images,” Descartes primarily had in mind “intentional forms,” so the other “philosophers” to whom he was referring were primarily scholastics. His claim is therefore that scholastics could not explain how intentional forms can be transmitted by nerves to the brain. Some have criticized this claim against the scholastics, arguing that Descartes misrepresents scholastic intentional forms as “images.” I will argue that although Descartes' portrayal of intentional forms as “images” is indeed a misrepresentation of scholastic philosophy, this misrepresentation does not undermine his claim against the scholastics. A detailed analysis of scholastic intentionality reveals that it does indeed preclude neural transmission of intentional forms, quite independently of Descartes' misrepresentation of it. In particular, scholastic intentionality required that sense organs possess certain physical attributes in order for them to be able to receive and transmit intentional forms. These attributes are not possessed by nerves, a problem that did not go unnoticed by scholastics and scientists in the 16th century, including Francisco Suarez, Johannes Kepler, and John Poinso. If so, then the scientific discovery of neural structure was a tipping point in the shift away from scholastic intentionality to Cartesian sensory skepticism.

Descartes's Non-functional, Quasi-substantive Vitalism

- Barnaby Hutchins (Bar-Ilan University)

The literature treats Descartes's position on life as either reductionist or eliminativist. I argue instead that he treats life itself as an irreducible, and that his position can be (broadly) characterised as a kind of vitalism, which (to butcher a distinction from Wolfe (2015)) we might term ‘non-functional, quasi-substantive vitalism’. It is quasi-‘substantive’ in that Descartes's conception of life involves a weak metaphysical commitment. It is non-functional because it plays no role in his physiology or medicine – that is, Descartes's account of the operation of human and animal bodies is entirely indifferent to his conception of life itself. Descartes describes the living body as reducible to the mechanical activity of pieces of matter. It might well seem that he takes life itself to be similarly reducible. MacKenzie (1975),
Ablondi (1998), and Detlefsen (2016) attempt to find reductionist concepts of life in Descartes; Hutchins (forthcoming) argues that Descartes is eliminativist about life. Eliminativism best fits Descartes’s wider commitments, but struggles to make sense of his frequent references to life. He explicitly distinguishes between living and dead bodies (e.g. AT xi: 330–1; AT ii: 66). He classifies animals as living (e.g. AT v: 278), but severed animal heads still active enough to ‘bite the earth’ as non-living (AT vi: 55). He affirms ‘life’ as the ‘category which includes the forms of all living things’ (AT iii: 566): on a reductionist reading, this would seem strangely tautological; on an eliminativist reading, it looks nonsensical. On the reading I give here, Descartes holds an irreducible notion of life: he can assert the existence of the category not despite its irreducibility to the terms of his system, but precisely because of its irreducibility – the tautology makes sense as a consequence of the irreducibility. Attributing an irreducible concept of anything beyond thought, extension and God to Descartes might – and should – sound odd. But there is a precedent: the union of mind and body. When Elisabeth presses Descartes on the unintelligibility of mind–body interaction, he replies that, although it is unintelligible in the terms of his dualism, we nevertheless understand it perfectly well: the union exists, even though it is irreducible to thinking or extended substances (AT iii: 692–4). My position is that life has a similar status for Descartes. This constitutes a weak ontological commitment to life; under Wolfe’s classification, it makes Descartes a (quasi-)substantive vitalist. Whatever commitment Descartes might have to the existence of life, he makes no use of it in his physiology. He has no extramaterial life-force to drive the body, and no disciplinary commitment to biology as a study of specifically living things. Consequently, his ‘vitalism’, such as it is, is entirely nonfunctional. In this way, he is an eliminativist about life within the confines of his dualism, while still getting access to a notion of life itself. Without endangering the strict materialism of his biology, he even gets to be a vitalist (of some sort).

**Gassendi’s Two Geometries**

- Delphine Bellis (Radboud University Nijmegen)

At least since Koyré, many scholars have emphasized that Pierre Gassendi’s relationship to the so-called Scientific Revolution was problematic because, among other things, Gassendi had a poor understanding of the mathematics of his time. On the contrary, Gassendi’s main rival, Descartes, has been considered as one of the most important mathematicians of the first half of the 17th century. Moreover, as Koyré emphasized, Descartes could be seen as the philosopher who carried out the geometrization of nature that is supposed to be so central to the new science of the 17th century. Now, Gassendi and Descartes were fiercely opposed as to the ontological status of mathematical objects. Whereas Descartes considered mathematical objects as real natures innate to the mind, in the Disquisitio metaphysica in which he forcefully attacked Descartes’ Meditations, Gassendi defended the view that mathematical objects were no more than mental abstractions from the sensible. Starting from that opposition, it seems rather logical to draw a line between Descartes, Galileo, and those natural philosophers who, on philosophical grounds, contributed to the mathematization of nature, and those like Gassendi who seemed to have missed the historical turn, not only for a lack of understanding of mathematics, but also for philosophical commitments that prevented them from seeing nature under the light of mathematics. However, the picture is more complicated, not only regarding Descartes and Galileo, but also Gassendi. Indeed, Gassendi was also a supporter of Galilean physics and of Keplerian cosmology. Does it mean that the French philosopher purely and simply rejected Galileo’s and Kepler’s Platonism to accommodate it to his own empiricism? In order to address this question, I will investigate Gassendi’s conception of mathematics and its relations to physical reality, first by briefly examining his Exercitationes paradoxicae (1624) and Disquisitio metaphysica (1642). I will then show what kind of problem his position might have
raised regarding the applicability of mathematics to physics as it emerged in the posthumous Syntagma philosophicum (1655). In the second part of this paper, I will analyze a little-studied text, Gassendi’s inaugural lecture as Professor of mathematics at the Royal College in Paris (1645). Surprisingly enough, in that text, Gassendi appears as the proponent of a kind of Platonism on the ontological status of mathematics and the mathematical dimension of nature. I will attempt to make sense of what appears as a contradiction by distinguishing, in Gassendi, two kinds of geometries: one divine, perfect, geometry that shapes the natural world, and one human, limited, geometry which can be used as an instrument for knowledge. I will show that this twofold view of geometry—indeed a combination of Platonism and Aristotelianism—is representative of a conception of mathematics that stemmed from Melanchton, the influence of which was transmitted to Gassendi through his reading of Kepler. In so doing I hope to shed new light on some alternative views on the status of mathematics and its relation to physics in the first half of the 17th century.

7.4 Contributed Papers: Causation and Laws of Nature

Intimate Connections: Wigner’s Views on Symmetries and Conservation Laws

• Pablo Ruiz de Olano (University of Notre Dame)

Eugene Wigner is often remembered for his contributions to the application of symmetry principles to physics, which earned him one half of the 1963 Nobel Prize in Physics. Apart from his scientific contributions, however, Wigner is also remembered for his philosophical writings on the topic of symmetry. Wigner was, as a matter of fact, one of the first authors to write on this subject, which has gained much prominence among contemporary philosophers of physics. In this paper, I clarify one aspect of Wigner's views that has been so far neglected in the literature, and concerns the nature of the connection between symmetries and conservation laws. My main claim is that Wigner believed the nature of the connection between symmetries and conservation laws to be different in quantum and in classical mechanics. I support this claim by looking at Wigner's many writings on symmetry, including the collection of essays “Symmetries and Reflections” and his talk and multiple interventions in the conference on “Symmetries in Physics” held in Barcelona in 1983. I use these texts to show that Wigner believed that Noether's theorems correctly account for the connection between symmetries and conservation laws in classical but not in quantum mechanics. Wigner further believed that, as a result of this, three specific differences obtain when it comes to the manner in which symmetries and conservation laws relate to each other in the two theories. After clarifying the content of Wigner's views, I situate them in the historical context in which they were developed. As I show, Wigner's interest in the nature of the relation between symmetries and conservation laws relates to his ambition to use it as a tool to develop a consistent theory for the strong nuclear interaction. His belief that the nature of the connection between symmetries and conservation laws was different in quantum and in classical mechanics, as a matter of fact, formed the basis of a proposal for a research program that Wigner made during the early 1950s. I briefly discuss the nature of Wigner's proposal, and its impact in the course that research on the study of the strong force took over the next two decades.

Max Born’s Philosophy of Causation

• Thomas Bunce (Durham University)

It is an oft-made claim that there is no genuine causation in physics (see, for example Russell (1912/1989) and more recently Norton (2003)) and that philosophers are mistaken in claiming that causal principles are
used in the discipline. In the light of this, it is interesting to examine the arguments of the physicist, Max Born, who claimed that causation is of great importance in the sciences. He writes “scientific work will always be the search for causal interdependence of phenomena”. In a 1948 book entitled Natural Philosophy of Cause and Chance (Born 1948) he gives his views on causation in the sciences, which this paper will explore. The book takes the form of a historical overview of the development of the concepts of 'cause' and 'chance' in physics, from Galileo up until the quantum theory. Born argues that the theories of physics should respect certain principles, named by him as 'contiguity' and 'antecedence', regarding causal relations. These are defined by him thus: “Antecedence postulates that the cause must be prior to, or at least simultaneous with, the effect. Contiguity postulates that cause and effect must be in spatial contact or connected by a chain of intermediate things in contact.” (Born 1948 p 9). He claims that these relations can be read off physical theories, and as physical theories get ‘better’ over time their accounts of causation get closer to respecting his principles. Born tries to assess the status of these principles in particular theories by analysing the time-reversibility of the state of a system for antecedence and whether or not the propagation of forces requires contact for contiguity. Although early physics does not fulfill Born’s principles, he argues that over time both contiguity and antecedence are gradually introduced into the subject as its theories become more sophisticated. He argues that the notion of contiguity is first introduced into physics via the development of continuum mechanics. This satisfies contiguity because it provides a physical model for how forces propagate through a medium, something which Born thinks is simply absent from previous formulations of mechanics. Antecedence is introduced by classical and the quantum statistical mechanics, which provide time-irreversible descriptions of phenomena. This paper will look at and critically assess the historical narrative that Born gives - that of tracing the historical development of his principles of contiguity and antecedence by looking at time and space-dependent relations in physics. It will go on to assess the status of these principles by delineating three possibilities – that they are Kantian synthetic a priori principles, that they are psychological principles (e.g. Emile Meyerson's principles of identity) or that they are realist heuristic principles (e.g. as in J.W.N. Watkin's Confirmable and Influential Metaphysics) and examining what role Born thinks that they are meant to have played in the development of physics.

John Dewey on Causation and Laws of Nature

Matthew J. Brown (University of Texas, Dallas)

John Dewey, one of the key figures in the tradition of classical pragmatism, was a significant figure in early twentieth century philosophy of science. He has recently been interpreted as a defender of a particular form of scientific realism (Godfrey-Smith 2002; Godfrey-Smith 2010), of the entanglement of science and values (Putnam 2002), and of interesting views on the relation of science and democracy (M. B. Brown 2009) and on the nature of scientific inquiry (M. J. Brown 2012). In philosophy of science, pragmatists are often thought of as anti-realists (though this need not be the case, as Godfrey-Smith (ibid) and Magnus (2012) show). They are even more commonly thought of as being generally anti-metaphysical. So far neglected are his views on laws of science and on causation, two related topics in philosophy of science that lean metaphysical and bear sidelong on the question of realism. As causation and laws are of significant contemporary interest, enjoying a renewed creativity of views and approaches, the time is right to consider Dewey’s potential contribution to the current debate. As it turns out, Dewey holds novel and potentially powerful views on these topics, making an exegesis of his views a worthwhile pursuit. His approach firmly rejects Humean and necessitarian views as confused and disconnected from the practices of science. His views bear some resemblance to accounts based on models, pragmatic laws, powers, capacities, or mechanisms (see Cartwright et al. 2005), but they are interestingly different from all
of those accounts. Dewey's views on laws and causation can be summed up by three claims made in Logic: The Theory of Inquiry (1938): (1) Scientific laws are hypothet- ical in form and abstract-universal in content, not concrete or “existential.” (2) Causation is “logical, not ontological.” (3) Causation is “incomplete and partial” and plays an intermediate role in inquiry, not a final one, i.e., at the conclusion of a particular inquiry, causal claims should “drop out.” These may sound like straightforwardly anti-metaphysical views. They are certainly incompatible with realistic views of laws and causation, simply and straightforwardly understood. But Dewey's metaphysics of science is far from nominalistic or positivistic. His claims about laws and causation are deeply related to a naturalistic background metaphysics that privileges events, inter- action, continuity, and instability, and Dewey systematically relates the work of science to that background in terms of concepts of “control,” “potentiality,” and “continuous histories” or “continuous events.” Dewey's view of the role of laws and causes in scientific inquiry is key to understanding his view that basic and applied science are interdependent and not wholly distinct.

Saturday - 25 June (Parallel Session VIII)

Parallel Session VIII (9:00-10:30)

8.1 Contributed Papers: Psychology

James on Instincts and Motivation

William James's account of motivation held that all behaviour could be explained with reference to either ideomotor action, habits, or instincts. Of these, James's account of instincts has received the most negative attention. Most commenters have stuck closely to the definition of instincts that James presents in The Principles of Psychology; namely, “the faculty of acting in such a way as to produce certain ends, without foresight of the ends, and without previous education in the performance.” Commenters note that this definition leads James to include complex behaviours such as kleptomania or constructiveness as instincts; this has led them to reject James's account of instincts and the overall account of motivation to which it leads. In this paper, I will argue that traditional commentaries fail to appreciate the significance of his slightly later claim that instincts “are the functional correlates of structure.” I argue that once this claim is understood in the context of James's neurology, James's definition of an instinct looks very different. For James, all behaviour is the result of nervous energy discharging through pathways carved into the plastic matter of our brains. When met with a particular stimulus, nervous energy enters the brain and discharges through the most well-worn channel associated with that stimulus. The behaviour to which that neural pathway leads follows irresistibly from exposure to the stimulus. The same is true for instincts, but with a catch. In cases of ideomotor action or habits, neural pathways are the result of the organism engaging with and adapting to its environment. In cases of instincts, the neural pathway exists without the antecedent engagement with or adaptation to the environment on the part of the organism. Instincts are the playing out of behaviours that are the result of pre-existing neural pathways; in other words, they are the functional correlates of structure. I will examine three consequences that follow from this point. The first consequence is that instincts are likely to be widely shared amongst members of the same species, given that their brain physiology is largely similar. That said, random variation and differences between members ought to be expected, within limits. The second consequence is that different species will share instincts to the extent to which their brain physiology is similar. The first and second points explain James's claim that humans have more instincts than animals, and not less; with a more complex brain
comes more opportunities for variation. The third consequence is that there is no reason why complex behaviour (e.g. constructiveness) could not be instincts. All that is required is that there be a pre-existing neural pathway between the perception of certain objects (e.g. blocks) and certain behaviour (e.g. building with them). When understood in context, James's account of instincts is much more fascinating than is traditionally thought. Although based on outdated neurology, it is not unsalvageable. To that end, I will conclude my paper with a sketch of an alternative, probabilistic interpretation of James's account.

Fichte's Articulated Body as a Tertium Quid between Idealism and Early Psychophysiology: A Study of the Principle of Apperception in Fichte and Wundt

• Liesbet De Kock (Vrije Universiteit Brussel)

While it is not until the late 1800s that psychology was established as an autonomous science, the emergence of the historiography of scientific psychology is an even more recent development. Comprehensive textbooks on the history of psychology fell from the press only in the 20th century. Especially Boring's (1929) and Allport's (1954) work has long been authoritative in this field. However, these classical historical reconstructions were soon criticized for a number of reasons. For one thing, it has been contended that the exclusive focus on the role of natural scientific methodology and empiricist philosophy in the emergence of a science of mind has prevented a proper understanding of the social and intellectual background of early scientific psychology. As a consequence, revisionist histories of psychology have thrived in the past decades. A lot, however, remains to be done. This paper takes one particular gap in the contemporary historiography of psychology as a point of departure, i.e., the almost complete disregard for the relation between early psychophysiology in Germany and post-Kantian idealism. This neglect is surprising, given the ubiquity of idealist metaphysics in German universities during the 19th century, and idealism's preoccupation with themes (e.g., self-awareness, agency and volition) that were central in early psychophysiological theories. One of the main challenges in this area of research pertains to the apparent incommensurability between idealism's abstract theories of subjectivity and early psychology's preoccupation with the way in which mental functions relate to the physiological body. A particularly fascinating example in this respect is the way in which the concept of 'apperception' – a purely formal principle in Kantian philosophy – was transformed into a hypothesized physiological function in Wilhelm Wundt's voluntarist psychology. This paper aims at introducing an intermediary stage in this apparent historical leap from the transcendental to the psychophysiological understanding of apperception, by analysing the way in which the apperceptive function was gradually inscribed upon the body in Johann Gottlieb Fichte's Ego-doctrine. That is to say, based on Fichte's work, I will examine the hypothesis of the idealist body as a systematic and historical tertium quid between transcendentalism and early (voluntarist) psychophysiology, thus putting into question the apparent incommensurability mentioned above. This exploration entails that I will subsequently go into (i) Fichte's transformation of the transcendental principle of apperception into a function of the articulated body [der artikulierte Leib], and (ii) Wundt's subsequent adoption of the concept of apperception to indicate one of the main psychophysiological functions of the volitional subject, and a constitutive element of experience. In doing so, the shift in the concept of apperception from a cognitive to a volitional function – one might say from an I think in Kant to an I will in Fichte – and from an abstract representation to an affect emerging from an embodied subject will be highlighted. Not only will this analysis help to attenuate the view of Fichte Ego-doctrine as a disembodied, abstract view of subjectivity, it is also helpful in restoring the intellectual continuity between philosophical idealism and early psychophysiology in Germany.
Largely forgotten outside histories of psycholinguistics and the social sciences, Grace Andrus de Laguna's Speech: Its Function and Development is an overlooked precursor to contemporary pragmatic treatments of language, behavior, and collective action. De Laguna develops a behavioristic theory of language that characterizes thought as modeled on external speech and offers a developmental linguistics that posits collective action and coordination as a constitutive factors for individual and group development. What makes her treatment specifically pragmatic is her argument that language, and thus thought, must be understood in terms of communication, gestures, and coordinated group action (as opposed to starting from 'inner speech', or the Cartesian perspective of individual agents, and working our way out). More than twenty years after de Laguna's groundbreaking but generally ignored work, Wilfrid Sellars offered similar pragmatic arguments concerning language, linguistic behavior, and collective intentionality. What separates both theorists is Sellars' commitment to a form of transcendental argument – a strategy de Laguna considered to be the “last bastion” of rationalism's defense against the naturalization of linguistic capacities and practices. Juxtaposing de Laguna's and Sellars' pragmatic treatments of language forces at least one question to the forefront: How do we “mesh” (to use Sellars' terminology) psychological and philosophical accounts of linguistic rules and “rule-regulated behavior”? I argue that while both de Laguna's and Sellars' presuppose specific conceptions of behaviorism (both historically and conceptually related), de Laguna's descriptive account of linguistic practices holds distinct explanatory advantages over Sellars' normative pragmatics. Given that de Laguna offers the same kinds of pragmatic explanations as Sellars without the transcendental baggage (i.e., she is able to offer an account of language without worrying about the naturalization of linguistic entities or rules), it stands to reason that the explanatory burden rests on those committed to the necessity of transcendental conditions or arguments. I conclude by arguing that Sellars' conception of behaviorism, combined with the “rationalistic residue” in his conception of language, fails to provide convincing arguments for the necessity of a specifically philosophical account of linguistic rules.

8.2 Contributed Papers: Mathematics and Method from the 16th to the 18th Centuries

Giordano Bruno's Geometrical Atomism and 16th Century Mathematics

• Paolo Rossini (Scuola Normale Superiore di Pisa)

Bruno's denial of incommensurability is accompanied by the adoption, in his later works, of the atomistic thesis. Bruno's atoms (“minima”) have a triple meaning, for they represent the ultimate indivisible unity in physics (atom), mathematics (point) and metaphysics (monad). Geometrically speaking, the existence of the minima implies that all magnitudes are discrete, i.e., they allow only for a finite number of divisions. Consequently, it is possible to express all magnitudes through a numerical integer and their ratios through a rational number. Bruno's geometrical atomism therefore rules out any incommensurability and succeeds in establishing a connection (which had been absent from Euclidean geometry) between numbers and magnitudes. With this contribution, I would like to achieve two goals. The first is related to Bruno's peculiar use of the history of philosophy as a means for promoting his own thought. This paper will present Bruno's negation of incommensurability as a case study to explain the way in which he argues. He frequently quotes other philosophers and thinkers in order to show how they apparently agree with his thesis. The key of this strategy lies, quite obviously, in the particular meaning he attributes to his sources: when the original one does not fit his philosophy, Bruno renders it more suitable to his purpose. In Bruno's refutation of incommensurability, it is specifically the axiom of Archimedes- Eudoxus to receive such a
treatment. In Euclid’s Elements, this axiom serves as a criterion to define the concept of ratio between two magnitudes (whether they are commensurable or not). Bruno, instead, applies it in order to demonstrate that incommensurability does not affect magnitudes at all. Therefore, in the first part of my contribution, I will focus on the “dialectical” value of Bruno’s confutation. In the second part, I will try to determine Bruno’s place within the broader framework of the history of mathematics. As has been shown well by Malet (Malet, 2006), ancient and modern mathematics diverge over to their respective conception of numbers and magnitudes, which for ancient mathematics had been two different beings. From this point of view, Bruno’s mathematics is evidently modern. What diminishes Bruno’s modernity is however the way in which he joins the two kind of quantities together. Unlike Bruno, modern mathematics will not deny the existence of incommensurable magnitudes, but it will associate them with irrational numbers. Hence, compared with the direction taken by modern mathematics, Bruno’s appears to be more like a dead-end. Nonetheless, it might have participated in the process that resulted in modern mathematics. I intend to explore this hypothesis, by means of an historical analysis of Bruno’s relation to sixteenth- and seventeenth-century mathematics. The aim will be to reconstruct the circulation and the reception of his geometrical ideas.

**Explanatory Virtues in the 17th Century: Kepler and Mersenne’s Competing Theories of Musical Consonance**

- Domenica Romagni (Princeton University)

As the reader is most likely aware, the time period in which Johannes Kepler and Marin Mersenne were writing was a time of considerable intellectual flux. The Aristotelian philosophical framework of the earlier centuries was being overhauled, and new developments in the fields of mathematics, mechanics, and biology (among others) would have significant impact on the kind of philosophical framework that would come to replace it. In order to understand the general aims and methods of scientific inquiry in this time period, I propose investigating an oft-overlooked theoretical disagreement between Kepler and Mersenne regarding the nature of musical consonance. This particular debate presents us with a puzzle because the scientific community at the time almost universally preferred Mersenne’s theory, despite its possessing a number of explanatory gaps that were fully acknowledged by Mersenne and his contemporaries. The fact that it ended up being the more correct theory, even though it was not obvious at the time, presents us with an opportunity to identify the reasons why theorists might have correctly preferred this theory. In examining the Kepler-Mersenne debate, and the stakes and terms guiding its resolution, my aim is to shed light on what kinds of ‘explanatory virtues’ theorists valued in this time period. Uncovering these explanatory virtues will help us to understand the puzzling preference for Mersenne’s theory and will enable us to better understand what theorists in this time period saw as desirable in a scientific explanation. This, in turn, will provide valuable insight into the issues that were important in the formation of our modern scientific methodology. The paper proceeds as follows: I begin by providing some information and background on the debate surrounding the nature of musical consonance. Then I go on to give a detailed account of Kepler and Mersenne’s views, respectively, and make clear the ways in which Mersenne’s theory was seemingly more impoverished. Following this, I examine some potential advantages that Mersenne’s theory possesses over Kepler’s that one might think would explain the general community’s convergence on it, such as simplicity, empirical falsifiability, and potential for unification. While all of these seem to be desirable features of a theory, I show why none of them are entirely adequate on their own to resolve the puzzle of the general preference for Mersenne’s theory. In order to fully resolve our puzzle, I introduce the conception of ‘potential for explanatory depth’, which enables us to understand how the surface shortcomings of Mersenne’s theory actually end up being
indicators of its preferability and, more generally, how explanatory gaps in a theory can sometimes point to its desirability, provided the gaps are in the right place. Finally, I illustrate how this virtue is able to resolve the puzzle at hand and, moreover, help us adjudicate between theories when each candidate seems to possess the same level of other explanatory virtues, all things considered.

's Gravesande's Empirical Concept of Laws of Nature and its Origins in Natural Law Theories
  - Jip Van Besouw (Vrije Universiteit Brussel)

As is well-known, Willem Jacob 's Gravesande was one of the most influential experimental physicists of the early eighteenth century. Equally known to specialist in eighteenth-century philosophy of science is the fact that 's Gravesande repeatedly elaborated on the philosophical foundations of his empiricism. Yet, as he is still regarded primarily as a 'Newtonian' in historiography, these foundations have almost exclusively been studied in relation to Newton's work, or, at best, to British empiricism more generally. Here, I will show that these relations are insufficient to provide a rich understanding of 's Gravesande's philosophy of science – a philosophy that had an importance influence on later Dutch, German, and French thinkers. In this talk, I will focus on one particular element of 's Gravesande's philosophy, namely his concept of laws of nature. According to 's Gravesande, experimental physics could lead us to certain knowledge of these God-given laws. As Ducheyne (2014) has already shown, 's Gravesande's particular understanding of the laws of nature was incongruent with Newton's view on induction. However, it is also clearly incompatible with the probabilistic epistemology of for instance Boyle and Locke. I will show that instead of to the British connection, we must look at German, Dutch and French influences in order to understand 's Gravesande's interpretation of the laws of nature and his epistemology in general. As I will argue here, of particular significance to 's Gravesande were debates on natural law between Pufendorf, Leibniz and Barbeyrac. Trained as a jurist rather than as a philosopher, 's Gravesande was well versed in these issues. Moreover, as his largely neglected works on moral philosophy show, the mature 's Gravesande continued to study new interpretations of the subject. I will pay special attention to the resemblances between 's Gravesande's concept of the laws of nature and contemporary conceptions of natural law. We will see that these were both moulded by theological debates, especially by the Scylla of Spinozist necessitarianism and the Charybdis of an arbitrary God. Particularly noteworthy is the fact that 's Gravesande, in both his moral philosophy as his philosophy of science, sided on many points with Leibniz and against his supposedly 'fellow Newtonian' Clarke. The link between moral philosophy and epistemology will furthermore enable me to elaborate on the critique of the 'voluntarism and science' thesis of Harrison (2007, 2009).

8.3 Contributed Papers: Victorian HOPOS
  
  'Numerical Syllogism,' Probabilities, and Actuarial Science: Augustus De Morgan’s Contribution to Victorian Logic
  - Anna-Sophie Heinemann (Universität Paderborn)

Around the middle of the 19th Century, Augustus De Morgan devises a “numerically definite” version of classical syllogistic which gives rise to shifts in the conception of logical quantity and quantification. It is based on inferences from premises of the form e.g. of ‘45 As are among 70 Bs’ or ‘45 As are not among 70 Bs,’ where the total numbers of As and Bs, e.g. 100 and 200, are given, and hence it allows for a (quasi-)arithmetical treatment which in turn is (quasi-)algebraically generalized. Against this background, 'quantification' means not so much the assign- ment of universality or particularity of scope to a
propagation, but the operation of exhausting a term's extension by conjunction of instances. The subject of the proposed contribution to HOPOS 2016 is the question whether De Morgan's approach to logical theory is rooted in his earlier works on statistics and actuarial science. If this hypothesis can be corroborated, De Morgan's steps towards quantification theory may be regarded as an example of extracting abstract structures and algorithms from problems in applied sciences. De Morgan's writings contain diverse references to extensions of logic by probable inferences. For example, his first of a series of five articles on the theory of the syllogism (1847) contains a concluding section which refers the preceding outline of a numerically specified syllogistic to the idea of a system based on probabilistic measures. The second paper in the series (1850) is supplemented by a separate section on probabilities and the measurement of credence of arguments, which expounds an implication of a guiding idea to De Morgan's logic: If extensions of all terms are numerically specified, as in ‘45 of 100 As are of 70 of 200 Bs,’ presuppositions to the propositions' validity may be gradually corroborated or weakened as the number of instances contained in the complement of a fraction (e.g., 45 As) of a total extension (e.g., 100 As) decreases or increases. Thus, in the case of propositions which on the classical scheme are uniformly classified as particular, quantitative specifications of term extensions afforded by the numerical scheme may increase or decrease the probability that propositions to their contrary may be the case. The ideas outlined above might be thought of as resulting from De Morgan's scheme of a numerically definite logic. However, it is far earlier than his effective revision of classical syllogistic that De Morgan discusses questions of probability. In 1838, De Morgan publishes and Essay on Probabilities, and Their Application to Life Contingencies and Insurance Offices, which is his first book on topics beyond 'pure' mathematics. This makes it plausible to think of De Morgan's idea of numerically definite inference as rooted in methods of handling probable inference. This hypothesis will be pursued by an assessment of De Morgan's writings and correspondence, but also by diachronic and synchronic comparisons of his ideas on the role of probabilities in logic with other approaches.

Herschel and Whewell on Residual Phenomena

Teru Miyake (Nanyang Technological University)

John Herschel's Preliminary Discourse on the Study of Natural Philosophy, published in 1830, had a profound influence on the development of British philosophy of science in the nineteenth century, particularly on the work of William Whewell and John Stuart Mill. An important idea in Herschel's philosophy of science, first introduced in the Preliminary Discourse, is the notion of residual phenomena. This notion was widely known among natural philosophers in the nineteenth century, as evidenced by its being mentioned in the latter half of the century by Jevons in his The Principles of Science and by Thomson and Tait in their landmark Treatise on Natural Philosophy. Residual phenomena are what are now called systematic discrepancies—the discrepancies between the observations that are predicted by theory, and actual observations. Herschel takes residual phenomena to be of great importance, because an examination of the history of science shows that the investigation of residual phenomena has often led to the discovery of previously unknown causes. Herschel thus takes residual phenomena to play a significant role in promoting the progress of science. Although Herschel gives examples of residual phenomena from various sciences, there is no doubt that Herschel’s emphasis on this notion is the result of his familiarity with physical astronomy, in which systematic discrepancies were the main drivers of research, especially in the post-Laplacian period of the early nineteenth century. Herschel's friend Whewell, who was also very knowledgeable about the methods of physical astronomy, mentions residual phenomena as well in his Philosophy of the Inductive Sciences, but under a different label: the Method of Residues. Whewell places less emphasis on the notion, but it plays a role in his history of astronomy, and
he discusses it as one of the methods through which induction is carried out. Although the notion gets mentioned later in the nineteenth century by Jevons and Thomson and Tait, it seems to have died out by the beginning of the twentieth century, at least in the philosophical literature. This paper has two aims. The first is to trace the notion of residual phenomena in the work of Herschel and Whewell, with particular emphasis on the connection between residual phenomena and research in physical astronomy in the nineteenth century. The second is to consider the reasons for the demise of the notion in the latter half of the nineteenth century. In particular, I will be considering the generalizability—or lack of it—of the notion to the sciences that were most on the minds of natural philosophers in the late nineteenth century, especially microphysics.

**The ‘Two Aspects’ Theory: Some Controversies over Neutral Monism and Pragmatism in Late Victorian & Edwardian Mental Philosophy**

• Thomas Staley (Virginia Tech)

This paper will explore some controversies stemming from the once–prominent work of Shadworth H. Hodgson, who articulated a philosophical position that attempted to give equal standing to mental and physical aspects of the human mind (a species of neutral monism he termed a “Conspectus of Reality”). Two of Hodgson’s most prominent interlocutors were G.F. Stout and William James. Stout emerged as a severe critic of Hodgson’s conception in papers published in Mind and the Proceedings of the Aristotelian Society, where he identified the two–aspects theory with absolute idealism and argued that it amounted to speculative nonsense. James, on the other hand, appreciatively endorsed Hodgson’s ideas and incorporated them into his own pragmatism. Notwithstanding the latter’s enthusiasm, Hodgson maintained a critical public and private correspondence with Stout and James, attempting to show both that their positions were both philosophically insufficient – in Stout’s case as excessively scientific positivism and in James’ as a psychologistic “partial” philosophy. These conversations reveal significant fault lines among the generation of English–language philosophers who saw the emergence of experimental psychology as a competitive enterprise with mental philosophy.

**Saturday - 25 June (Parallel Session IX)**

Parallel Session IX (11:00-12:30)

**9.1 Symposium: Naturalistic Accounts of the Mind, 1850-1900**

In contemporary philosophy, the concept naturalism is both ubiquitous and (with respect to the concept's long history) idiosyncratic. Much of today’s naturalism takes off from Quine’s philosophy of language, according to which there can be no clean distinction between synthetic statements responsible to empirical, scientific evidence, and analytic statements responsible to conceptual, philosophical analysis. But by the time “Two Dogmas of Empiricism” was published in 1951, there had already been a rich tradition of naturalist philosophy. That tradition was particularly lively in the late nineteenth century. At that time the term ‘naturalistic’ was used more broadly, to mean something like natural-scientific. For example, William James offered what he called “a purely naturalistic view” of consciousness, one that did not “bring in the supernatural or miraculous.” Such naturalistic accounts of the mind led to a reevaluation of traditional approaches to human cognition, with experimental and evolutionary evidence brought to bear on classic philosophical questions. This panel, a contribution to the history of philosophy of psychology,
analyzes a series of cases in which scientific explanations of mental phenomena led directly to philosophical reflection on topics such as consciousness and human uniqueness. Rather than centering on arguments about language, most of these older discussions were connected first and foremost to evolutionary ideas: Herbert Spencer, in 1870, wrote that his Principles of Psychology was “primarily concerned with psychological phenomena as phenomena of evolution”; Darwin, the next year, declared that “the difference in mind between man and the higher animals, great as it is, is certainly one of degree and not of kind”; Thomas Henry Huxley, infamous as Darwin’s bulldog, defended epiphenomenalism in 1874. Evolutionary approaches had become standard among naturalists by the 1870s, and thus form the background of all three papers. The first paper, “The Curious Case of the Decapitated Frog,” discusses philosophical reactions to a famous set of experiments in which the brains of frogs were removed. These frogs still exhibited purposive behavior, leading to questions about the seat of consciousness and ultimately about the relationship between mind and body. The next paper, “‘The Ordinary Cock-sure Evolutionist’: Pragmatism, Naturalism, Adaptationism,” argues that pragmatist philosophers, although critical of Herbert Spencer’s naturalistic account of the mind, were in the end deeply influenced by his general evolutionary approach. The third paper, “Vera Causa Reasoning in the Debate Over Human Uniqueness,” investigates different approaches to comparative psychology in this period: Is there truly only a difference in degree between humans and other animals? When can we interpret an action as the outcome of a higher psychical faculty? All three papers are also in conversation with philosophy today: the first discusses the recent trend toward experimental philosophy; the second speaks to debates in philosophy of biology over adaptationism; and the third addresses Morgan’s canon, still often used to defend human uniqueness. One aim of our panel is to explore older conceptions of naturalism that might provide instructive alternatives to varieties more commonly on offer today.

The Curious Case of the Decapitated Frog

• Alexander Klein (Cal State Long Beach)

In the nineteenth century, a series of experiments on headless frogs became embroiled in a debate over the relationship between mind and body. The story provides a case study of the role experiments might reasonably play in philosophical deliberation. Edward Pflüger published the most influential account of such experiments in 1853. Healthy frogs will wipe away an acid irritant dripped on the knee. When the foot habitually used for this purpose is impeded or amputated, the frogs often choose a different foot to wipe away the acid. Pflüger showed that frogs exhibit the same choosing behavior even when decapitated, and thus claimed to have shown the spinal cord (not merely the brain) to be an organ of consciousness. His experiments were subsequently repeated, modified, and championed by G. H. Lewes in particular. The results became controversial in part because of the way Pflüger and Lewes’s work made contact with a contemporaneous debate over what is now called “epiphenomenalism.” This is the view that (as Shadworth Hodgson put it in 1865) “states of consciousness are not produced by previous states of consciousness, but both are produced by the action of the brain.” T. H. Huxley later gave the view its most famous metaphor—consciousness does not act on the body any more than the steam whistle acts on the locomotive engine. He would cite Pflüger’s work as offering an elegant illustration of behavior that seems purposive without really being controlled by any conscious experience. Huxley’s argument rested on a claimed right to assume that “a frog’s spinal cord is not likely to be conscious” since “a man’s is not.” Set against the background of this debate over the relationship between mind and body, the decapitation results produced a kind of aporia. Here is William James’s take on the situation as things stood in 1890: “If we start from the frog’s spinal cord …, saying, as that acts so intelligently, though unconscious, so the higher centres, though conscious, may have the intelligence they show quite as mechanically based; we
are immediately met by the exact counter-argument from continuity, an argument actually urged by such writers as Pflüger and Lewes .... [Y]ou can either level up or level down by their means; and it is clear that such arguments as these can eat each other up to all eternity.” Epiphenomenalists must deny that purposive behavior is a mark of consciousness in healthy animals, and so they will certainly deny that such behavior shows that headless frogs can be conscious. But their opponents may accept purposive behavior as a mark of consciousness, and can therefore interpret these same experiments as demonstrating (spinal) consciousness even in decapitated animals. It is tempting to think of this argument over epiphenomenalism as philosophical precisely because one must make a choice about this position before interpreting relevant experimental data. Thus I will argue that Pflüger's decapitated frogs present a challenge to naturalists: viz., to explain what makes this (or any) question both philosophical and, at the same time, empirically tractable.

‘The Ordinary Cock-sure Evolutionist': Pragmatism, Naturalism, Adaptationism

Trevor Pearce (University of North Carolina, Charlotte)

In the late nineteenth century, Herbert Spencer challenged traditional philosophy by offering a scientific or naturalistic account of the mind. His views were criticized by Chauncey Wright and William James, both of whom were involved with the “Metaphysical Club” of the early 1870s, famed as the birthplace of pragmatism. I argue in this paper that although Wright and James were very critical of Spencer, they did not oppose his naturalism or his adaptationism. In fact, the pragmatists ultimately adopted Spencer's evolutionary approach and his organism-environment framework. Spencer argued that the intelligent mind is the product of adjustment to one's present environment and also to that of one's ancestors. He also gave evolutionary accounts of a variety of mental traits: for example the capacity for feeling pleasure and pain. Several scholars have claimed that James and Wright's critiques of Spencer involved a kind of anti-adaptationism avant la lettre, and it is true that both of them insisted that not all traits are adaptations (in our modern sense). But although critical of Spencer, both James and Wright gave straightforwardly adaptive explanations of many traits, and did not question this general explanatory strategy. James, for instance, writing of “cadaveric, reptilian, and underground horrors,” suggested that “the ordinary cock-sure evolutionist ought to have no difficulty in explaining these terrors . . . as relapses into the consciousness of the cave-men.” Both Wright and James also gave evolutionary accounts of consciousness that were explicitly naturalistic, meeting Spencer on his own ground. Thus they were closer in spirit to the English philosopher than his many rationalist enemies.

Vera Causa Reasoning in the Debate Over Human Uniqueness

Hayley Clatterbuck (University of Rochester)

In the late nineteenth and early twentieth centuries, Charles Darwin and C. Lloyd Morgan forwarded two influential principles of cognitive ethological inference from which they drew conflicting conclusions about the extent of mental continuity between humans and other animals. The first, offered by Charles Darwin (with a precursor in David Hume), states: “I can see only one way of testing our conclusions. This is to observe whether the same principle by which one expression can, as it appears, be explained, is applicable in other allied cases; and especially, whether the same general principles can be applied with satisfactory results, both to man and the lower animals.” Using this and other similar principles, Darwin arrives at the conclusion that “there is no fundamental difference between man and the higher animals in their mental faculties.” The second, which has commonly been viewed as a necessary corrective to the
anthropomorphic conclusions to which Darwin's principle led, is stated in C. Lloyd Morgan's famous Canon: "In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale." From this principle, Morgan infers a significant discontinuity in the mental faculties of man and animals, at least with respect to the higher faculties of reason and abstraction. Interestingly, both Darwin and Morgan believed that their principles were consistent with, and indeed motivated by, natural selection and the thesis of recent common ancestry. Why, then, were they committed to such different principles? A common tendency among commentators is to interpret them both as applications of Ockham's razor, focusing on two different senses of parsimony. However, I argue that looking for differing commitments to parsimony in the justification of these principles is a red herring. In fact, both authors followed very similar vera causa inductive strategies in arriving at their principles, in which parsimony plays but a weak role. This common ground sheds light on both the philosophy of science that motivated pioneering work at the birth of comparative psychology and on the true source of the conflict. Darwin and Morgan disagreed about the “true causes” of human psychology, and more specifically, whether the causes specified by a Humean empiricist theory of mind suffice to explain all of human behavior. Darwin's work in comparative psychology was informed by a thorough Humeanism; although this is largely implicit in his published work on the topic, it is much more explicit in his Notebooks M and N from which those works originated. In contrast, Morgan argued that Humean cognitive mechanisms could not account for a class of uniquely human behaviors. Once we lay bare their disagreements on that point, we see that Darwin's principle and Morgan's Canon are quite inferentially inert on their own; their varying principles “fall out” of their similar vera causa approaches and their particular views of the true causes of cognition.

9.2 Symposium: Scientific Explanation in Early Modern Europe

A hallmark of contemporary philosophy of science is an exploration of the connection between epistemology and explanation, especially how they constrain and enable each other at the same time. In this panel, we make explicit different ways that explanation can be illuminating for and illuminated by other sorts of philosophical issues. We do so through three papers that look at how explanations are explicitly and implicitly explored in philosophy of science in the Early Modern period. In the first paper in the panel, “Naturalistic Explanation as Epistemological Firebreak for Hume and Boyle,” we see how some thinkers in the period worked out the connection between explanation and epistemology. The paper looks at how the consequences of naturalism differ for the scientific contexts of explanation and of epistemological grounding for Boyle and Hume. Doing so shows that naturalism puts some constraints on how explanations function and—perhaps more crucially—what explanations can be expected to do. Insofar as the implications of naturalism are still unclear in the period, this is crucial work for philosophers of the time to do: without a way to stop the regress of epistemological grounding, skepticism looms. The paper argues that canny naturalists of the period saw that explanations can be local in a way that makes them autonomous from the regress-inducing grounding requirement of naturalistic epistemology. The second paper, “Cartesian Cryptography and Norms of Scientific Explanation,” directly addresses the connection between explanation and more practical issues. In this case, the paper works from the ground up to uncover what the implicit global norms of explanation are for Descartes and Leibniz by focusing on their under-appreciated discussions of code-making and code-breaking. By focusing on a particular case study, the paper is able to illustrate what the global features of explanation must be, and how what we take now to be diverse models of explanation are actually integrated for these Early Moderns. In contrast
to this more-global look, “Metaphysics and Explanation in Richard Bentley’s Boyle Lectures” shows how a particular case of explanation can illuminate local norms of explanation and illustrates explanation’s connection to metaphysics. By considering how Bentley’s particular commitments constrain the sorts of explanation he can offer for gravitational phenomena, we can adjudicate between different possible interpretations of his position. By doing so we both gain a better idea of how the disputes about gravitation (and superaddition) progressed through the period, we also get an idea of how metaphysical theses constrain which explanations are available in this paradigmatically scientific case. Each of the papers deals with how explanation figures into or is constrained by other natural philosophical explorations in the period. These papers illustrate how deeply explanation as a scientific activity is connected to other philosophical issues in the period, in ways that are analogous to but strikingly different from contemporary discussion of the connection between explanation and epistemology.

Naturalistic Explanation as Epistemological Firebreak for Hume and Boyle

• Richard Fry (Southern Illinois University, Edwardsville) and James Mattingly (Georgetown University)

In the Early Modern period, the de facto ‘official’ philosophy of science came to be a species of naturalism. But by most accounts, a rigorous application of naturalist principles leads directly to skepticism. Thus the nascent project of science seems to be undermined by the stringent demands of its own method. On most Early Modern readings, naturalism’s demands make seemingly trivial empirical assertions collapse into a skeptical quagmire. The depth of this problem makes it necessary to explain how it is possible to solve the problem in the context of Early Modern science. We propose that Hume has a Boylean solution to naturalism’s demandingness. We explicate it by drawing attention to a distinction between explanatory and epistemic scientific contexts. Hume’s ambitions are both explanatory and epistemological, making him an ideal case study. Hume’s work illustrates clearly how skepticism looms when naturalism is conceived as a grounding requirement, that is, as a metaphysical condition on scientific epistemology. The drive to epistemological grounding is eventually corrosive of any attempt to establish a secure scientific methodology because Early Modern epistemological naturalism embeds a regress: to ground any bit of knowledge in more fundamental knowledge (or experience) calls for the knowledge (or experience) doing the grounding to itself be grounded. Whereas Cartesians (and other believers in first-principles) are able to eventually ground out these appeals in knowledge of God (and other first principles), the committed naturalist is not able to do so. As such, skepticism looms. But naturalism—as constraint on explanation—enables various localized explanatory practices to be cordoned off from further justificatory demands. Though some of this is paralleled elsewhere—in Newton for instance—Boyle shows directly how naturalistic explanation is local. For instance, in Grounds for the Excellence of the Corpuscular or Mechanical Philosophy, Boyle illustrates that certain features of the explanans do not matter in determining whether it is successful in explaining the explanandum by appealing to corn-grinding. Millstones grind corn, he says, but whether the source of their power is wind or water or horse is irrelevant to their goodness for the task. For Boyle, what matters is that they turn, and that they are shaped a certain way. He (and we) take this to show that the naturalist can fully explain some phenomena even when the explanans itself is not fully specified or understood. Naturalistic explanation, then, operates by appeal to proximal causes; it is fundamentally local in a way that can provide a firebreak against the damaging global influence of naturalistic epistemology. Hume’s worries about the epistemological stakes of his project in T 1.4 and EHU 12 are well known, but how his explanatory project in T 1.3.6 and EHU 5 is decidedly local (in the manner of Boyle) is less fully regarded. We show how Hume’s naturalistic explanations for beliefs in causes and effects are genuinely successful in leaving
skeptical worries about naturalistic epistemology to the side. This provides an example of how Modern philosophers of science solve the problem of naturalism's demandingness.

**Cartesian Cryptography and Norms of Scientific Explanation**
- Dana Matthiessen (University of Pittsburgh)

In both Descartes’ and Leibniz's early writing, we find accounts of scientific explanation that combine elements of what we now call causal-mechanical, hypothetico-deductive, and unificationist models. In this talk, I will focus on the norms of hypothesis selection and explanation that accompany these accounts, with an emphasis on their repeated references to cryptography made in efforts to exemplify such norms. These references can be situated within the interweaving history of algebra, cryptography, and the idea of an “alphabet of nature” in the 16th and 17th centuries. The development of modern algebra in 16th century Europe was roughly simultaneous with a renewed interest in cryptography. Formal similarities between algebraic and cryptographic practices—the substitution and permutations of symbols—made the emerging class of symbolic algebraists apt code-breakers. This kinship was reflected in the employment of such figures as Viète in the role of court cryptanalyst and in the revived use among intellectuals of a “word calculus” in which sayings and messages are hidden by replacing the letters of words with numbers. One 17th century employer of the word calculus, the Rosicrucian Johannes Faulhaber, saw a close connection between algebra and the interpretation of biblical numbers like 666. Another figure from this period, the Jesuit Athanasius Kircher, used cryptographic methods in an attempt to construct a universal mathematical language. Later in the 17th century, John Wallis, who built on Descartes’s analytical geometry, made advances in the understanding of ciphering that earned him a position as Parliamentary cryptographer. Descartes and Leibniz were well aware of these works, which formed part of the intellectual context in which they formulated their scientific programs. This history of algebra, cryptography, and the “alphabet of nature” is readily conjoined with an analysis of Descartes's intellectual development (specifically the influences of his Jesuit education, correspondence with Beeckman, and meetings with Faulhaber, which inspired the use of algebra as the model for scientific knowledge in his Regulae) and subsequent impact on Leibniz (an avid reader of the Regulae, among other works of Descartes, who draws directly from the latter's cryptographic metaphors). A definite picture is thereby formed that helps us understand the significance of algebraic thinking in these authors’ proposals for scientific methodology. In particular, we can understand why Leibniz would compare an algebraic root to a cryptographic key, and a key to a scientific hypothesis. By examining these connections, we see why their conception of the relationship between phenomena and the causal-mechanical hypotheses meant to explain them can be modeled on the activities of an early modern code-breaker. Taken together, these historical and conceptual strands provide one inroad into seeing how elements from what we regard as diverse models of explanation were combined to form a coherent early modern philosophy of science.

**Metaphysics and Explanation in Richard Bentley’s Boyle Lectures**
- Patrick Connolly (Iowa State University)

In 1692 Richard Bentley was chosen to give the Boyle Lectures. The lectures were designed to bolster Christianity against the perceived spread of atheism. In the course of this task, Bentley relied on the new Newtonian philosophy. In doing so he became one of the first public defenders of Newton and engaged with many of the philosophical issues that were to characterize the reception of Newton in subsequent decades. One puzzle the Newtonian system faced was the explanation of gravitational attraction. Bentley
attempted to show the complementary nature of Christianity and Newtonianism by suggesting that the Christian God could be invoked to explain the existence of gravitational forces. Two commentators have recently suggested that Bentley employs a “superaddition” model to explain how this could be so (Kochiras 2009 and Henry 2011). On their view, Bentley's God was able to endow material bodies with powers that did not flow from their essences. So, for Bentley, the power of bodies to gravitate can be explained by God's decision to endow them with this superadded power. I argue against this interpretation. Bentley could not have endorsed a superaddition view of gravitation. The view Bentley endorsed can be better categorized as occasionalist. Bentley's view was not that God added a power to bodies which allowed them to gravitationally attract one another. Instead, Bentley believed that God was immediately and directly responsible for the effects of gravitational attraction. Understanding why the occasionalist interpretation is superior to the superaddition interpretation requires an examination of the metaphysical system that Bentley develops in the Boyle lectures. Thus the paper examines Bentley's matter theory, his views on causal powers, and his views on modality. Regarding matter theory, Bentley is committed to a form of mechanism according to which matter has three essential features: location, impenetrability, and motivity. Regarding causal powers Bentley argues that all actions of matter can be fully understood through appeal to its essential features. He uses this view to fund an attack on “occult” powers in matter. And regarding modality Bentley argues that God's omnipotence allows him to do anything that does not entail a contradiction. Importantly, Bentley claims it would be a contradiction for an object to act in a way contrary to its nature. These views show that Bentley could not have adopted the superaddition account of gravitational attraction. For Bentley, gravitational forces were active powers and the possession of any active power was contrary to the nature of matter. And even God cannot give something a power contrary to its nature. Superadded gravitation would just be another of the “occult” powers Bentley was so quick to mock. Careful examination of passages in which Bentley discusses gravitational attraction shows that he was aware of these implications of his views. He carefully draws a distinction between (a) God's initial causal contribution at the outset of creation and (b) his continual and immediate causal contribution to the world. Gravitational attraction is consistently characterized as falling into the latter category.

9.3 Contributed Papers: History of Philosophy of Biology

Joseph Needham’s Contributions to the Development of the Levels Concept in Biology (1929-1945)

• Daniel Brooks (Konrad Lorenz Institute for Evolution and Cognition)

The origins of the contemporary concept of ‘levels of organization’ in biology and philosophy can be traced to the organicist biologists of the 1920-30's. Though the basic term “levels” had been introduced into the scientific lexicon earlier as a significant technical concept, it was quickly observed to be a largely unanalyzed term in need of active development. Organicist proponents engaged soon thereafter articulating the meaning and significance of ‘levels of organization’ (hereafter ‘levels’) for biology. Consequently, the term served to both preserve a materialist scientific ontology while elaborating the unique explanatory problems that distinguish biology from the physical sciences. This paper will analyze Joseph Needham’s development of the concept of ‘levels of organization’ in biology during the period of 1929-1945. Needham's efforts imbued ‘levels’ with a programmatic character that was instrumental in its uptake as the pervasive, central theoretical concept it is known as today. This development proceeded in three stages. The first stage, spanning approximately 1929 to the early 1930's, comprised reconciling his experimentalist-oriented epistemology of biology with Joseph Woodger’s philosophical work. The
introduction of ‘levels’ into a biological context by Woodger’s Biological Principles had a significant impact on Needham’s scientific epistemology. Secondly, Needham proceeded in the early- to mid- 1930’s to elaborate an application-oriented understanding of ‘levels’ that successfully combined these lines of thought. Here Needham’s development of ‘levels’ departed from Woodger’s theory-first approach, emphasizing the practical significance of the term for more adequately articulating the problems working biologists sought to solve in their research practices. These efforts, thirdly, culminated in his 1937 Herbert Spencer Lecture “Integrative Levels”, which would also mark his last major contribution to developing the levels concept. The mark this work would leave on the term, however, was more baffling than beneficial. Instead of the piecemeal experimentalist epistemology of the prior years, Needham’s lecture ventured deep into, i.a., a sweeping biologistic view of human society and its ultimate future. Nonetheless, the paper remains noteworthy as (1) a resource directly linking the continuity of usage of ‘levels’ from its organicist roots to subsequent users and developers in biology at large, (2) a programmatic expression of the term now linked to an established system of usage, and as (3) introducing the provisional namesake label under which ‘levels’ was initially absorbed into the scientific community during the critical period of 1942-45. This continuity, however, was threatened by the initially split reception of ‘levels’ following Needham’s (1937) lecture: One point of uptake, an anthology dedicated to “integrative levels” published in 1942 from a conference organized by Robert Redfield, emphasized the all-encompassing, biologistic ability of ‘levels’ to extend biological explanation to social phenomena. A second point of uptake, and arguably a return to Needham’s earlier, pre-1937 development of the levels concept, was Alexander Novikoff’s widely received 1945 article on levels in Science. Novikoff, criticizing the interpretations of Redfield’s anthology, argued for a return to the earlier significance of the concept, i.e. as a useful (and now programmatically endowed) tool for working biologists to articulate the central problems of biological research.

The Operon Model and Scientific Explanation

• Melinda Fagan (University of Utah)

There are multiple accounts of scientific explanation on offer in philosophy of science, which variously locate explanatory power in fundamental processes, general laws, counterfactual dependencies, detailed causal mechanisms, unifying principles, simplified minimal models – and more besides. Jacob and Monod’s operon model of gene regulation (1961) has, at one time or another, been used as an example in support of many of these accounts. All philosophical interpreters agree that the operon model explains (in some sense of the term) regulation of gene expression, by schematically representing relations between genetic and biochemical entities comprising a regulatory switch. But further analysis of the operon case runs the full gamut of philosophical views on biological explanation. The model has been interpreted as an instance of deductive-nomological explanation (Schaffner 1969), as a step toward reducing biological phenomena to physical and chemical laws (Schaffner 1974, Kimbrough 1979), as an “interfield theory” linking genetics and biochemistry (Darden and Maull 1977), as demonstrating anti-reductionism for Mendelian and molecular genetics (Kitcher 1984), as an illustration of highly abstract models of gene regulatory networks (Richardson 1996), as one of a heterogeneous collection of theories or mechanisms required to “cover the domain” in the absence of biological laws (Beatty 1997), as an example of mechanistic explanation supporting interventionism over causal process theories (Woodward 2002, Craver 2007), as an abstract model contradicting the basic mechanistic view (Levy and Bechtel 2013), and as satisfying general principles of biological organization (Bich et al 2015). How can one scientific case justifiably support such diverse views of explanation? What does this ‘one-many’ relationship mean for the status of any one philosophical account of explanation? This paper aims to answer these questions.
Taking an historical approach to the diverse philosophical accounts of the operon model, I show that traditional theory reduction (and only this account) has been justifiably rejected, on the basis of sustained and careful criticism. Otherwise, the operon model supports diverse accounts of scientific explanation: causal mechanisms, interventionism, multilevel systems models, and more. Each focuses on a single aspect of the operon model, identifying it as the source of explanatory power. Each can appeal to norms and standards of explanation in particular fields of life science: molecular biology, biomedicine, or systems biology. Taken together, these diverse philosophical uses of the operon model support pluralism about scientific explanation. In our current context, this interpretive inclusivity is itself an explanatory virtue. It is a commonplace that science is becoming increasingly interdisciplinary. In future, more and more significant scientific explanations will require contributions from multiple fields and disciplines. In such a context, a model supporting different styles of explanation is a valuable resource - a versatile point of contact between fields with diverse explanatory aims and commitments. The operon case suggests that monism about scientific explanation is an artifact of a limited, ahistorical approach.

**The Use of Karl Popper in Ecology**

- William Bausman (University of Minnesota)

Karl Popper's philosophy of science was used by a group of ecologists in the 1970s and 80s to help challenge the then-dominant research programme - competition between differently-adapted species is responsible for the observed patterns of abundance and diversity in ecological communities. In this paper I investigate the scientific controversy that has come to be known as "the null model wars", which took place most visibly between Jared Diamond and Daniel Simberloff. Simberloff accused Diamond of merely confirming pet hypotheses, when "science progresses by a different route: by posing testable hypotheses and then attempting to falsify them." Simberloff adapted the methods of classical statistical hypothesis testing to his goal and argued that the only way to support the claim that competition was the dominant cause in ecology was to first falsify the "null hypothesis" or "null model" in which competition was absent. My question is this, Why was Popper used in this way, in this science, at this time? Was Popper's falsification a driving influence on Simberloff and company, or was it merely rhetorically useful at the time? And for either answer, why? Philosophers of biology know that ecologists love to say that they are doing what Popper showed was the way and the truth and the life. But almost nothing has been written on the use of Popper in ecology. So this case is ripe for historical and philosophical analysis. The obvious answer to my question is that Popper's use by Simberloff and others was merely rhetorical – Simberloff was not guided by Popper, but enlisted Popper post-hoc, and without much sophistication. This answer is compelling, but over-simplified. I will argue that many of the ecologists of the day had been taught philosophy of science and they took onboard Popper's criticisms of confirmatory science. But at the same time, there were independent reasons, from both ecology and statistics, for critiquing the methods and accepted hypotheses of the day. The null model wars happen at the confluence of three historical movements: (1) Critiques of the modern synthesis of evolutionary theory and Adaptationism across evolution, paleontology, and ecology; (B) Statistical methodology, including Fisher and Neyman-Pearson statistical hypothesis testing; and (C) Karl Popper's Falsificationism. It is a ripe case for philosophical analysis because we see here normative accounts of scientific methodology are put to work by scientists attempting to change the practice of their discipline.
Reconsidering Friedman’s Neo-Kantian Reading of the Early Carnap

Nathan Sasser (University of South Carolina)

In this paper I argue that Michael Friedman's neo-Kantian reading of Rudolf Carnap's The Logical Structure of the World (1928) obscures its emphasis on the intrinsic intelligibility of raw sense data. Friedman's reading of the early Carnap fits into his larger project, his conception of “dynamical or relativized a priori principles” in the exact sciences. One of Friedman’s main inspirations is Carnap’s notion of linguistic frameworks, articulated in The Logical Syntax of Language (1934; trans. 1937) and “Empiricism, Semantics, and Ontology” (1950). Friedman's idea is that Carnap’s linguistic frameworks can play the role of what Thomas Kuhn calls scientific paradigms, providing a priori principles within which normal science can proceed. These a priori principles are however dynamic and revisable. In scientific revolutions, the linguistic frameworks change, constituting a new paradigm for normal science. Friedman not only reads the later Carnap in a neo-Kantian fashion. He also argues that LSW is more neo-Kantian than phenomenalistic insofar as Carnap here agrees with neo-Kantians such as Cassirer that relations alone must serve as principles of individuation. I challenge this claim by pointing out an important way in which Carnap differs from the likes of Cassirer. While Carnap makes purely structural definite descriptions the condition for the possibility of intersubjective scientific discourse, he does not assert any necessary preconditions for the cognition of sense data and relations of similarity between them. Carnap acknowledges that direct empirical experience of basic relations between sense data provides the foundation for the constructional system. For Cassirer, by contrast, empirical experience always presupposes a system of logical relations. Friedman downplays the fact that Carnap never attempts to reduce all objects of cognition whatsoever to purely structural definite descriptions. He therefore overemphasizes the significance of sections 153-5 of LSW for Carnap’s project. Looking back on LSW in 1961, Carnap announced that he still agreed with the book's main goal, “the rational reconstruction of the concepts of all fields of knowledge on the basis of concepts that refer to the immediately given” (Preface to the Second Edition, vi). Friedman can legitimately claim that Carnap is neo-Kantian in the sense that his logical rules and definitions function as conditions for the possibility of intersubjective scientific discourse. But it is just as legitimate to claim that early Carnap is neo-Humean, since he holds that experience itself, the immediately given, is intrinsically intelligible to individual subjects and grounds the most basic concepts.

Moving Beyond Antiquarianism: Explication and the Future of Philosophy of Science

Christopher French (University of British Columbia)

There is an often discussed disciplinary tension between the history of science and the philosophy of science: Rather than engage in historical questions about why and how historical actors held particular scientific views, some philosophers of science instead ask whether one should hold those views. In this paper I illuminate a similar tension between historians of philosophy of science and philosophy of science proper using Rudolf Carnap's method of explication as an example. Seeking an alternative to philosophical methodology like conceptual analysis, several philosophers of science have recently provided constructive critiques of Carnap's method of explication with the aim of articulating an empirical, or experimental, method of explication (Dutilh Novaes and Reck 2015, Schupbach 2015, Shepard and Justus 2015). Once informed by recent empirical work in social psychology and experimental philosophy, this empirical version of explication transforms philosophy into a well articulated engineering project:
Philosophers can design and construct concepts they find useful while taking on board the lessons from human psychology and cognitive science. But what role does the historian of scientific philosophy have to play in such reconstruction projects? One answer is that they should embrace an antiquarian approach that emphasizes the historical and social contexts of Carnap's thought. Aside from examining the differences between Carnap and his peers, like Hempel, Feigl and Reichenbach, this approach adopts a critical tone; e.g., it may explain (i) how such reconstruction projects domesticate Carnap's more radical views about language and mathematics by failing to properly highlight how Carnapian explication is undergirded by his liberalization of empiricism and logical tolerance, (ii) why Carnap would maintain a sharp distinction between logic and psychology, or (iii) that Carnap himself never held a steadfast distinction between descriptive and normative philosophy of science. Nevertheless, there are reasons to resist antiquarianism. First, such reconstruction projects are clearly in line with Carnap's own engineering sensibilities. Second, of all people, Carnap would not have us forestall philosophical progress with historical digressions (even if they are his own). I then draw on a historical episode using Carnap's correspondence with his former student and peer, Richard C. Jeffrey, about latter's probability kinematics to help us better understand how Carnap thought about the application of his inductive logic to decision theory and, by extension, the prospects for a contemporary empirical method of explication. Finally, I argue that a historically informed understanding of Carnap's unique methodological views toward logic and the empirical sciences can provide contemporary philosophers of science with an insightful resource for articulating an alternative way of doing philosophy.

Hilbert's Axiomatic Method and Carnap's General Axiomastics

Michael Stoeltzner (University of South Carolina)

I compare the axiomatic method of David Hilbert and his school with Rudolf Carnap's general axiomatics that was developed in the late 1920s and that influenced his understanding of logic of science throughout the 1930s, when his logical pluralism developed through various stages. While recent scholarship has analyzed Carnap's general axiomatics primarily as an instance of early model theory, my paper starts from Carnap's primary motivation, to wit, the axiomatization of science and its paradigmatic example Hilbert's Foundations of Geometry. The distinct perspectives of the axiomatic method and general axiomatics become visible most clearly in how Richard Baldus, along the lines of Hilbert, and Carnap and Friedrich Bachmann, in 1936, analyzed Hilbert's specific axiomatization of geometry. While Baldus discussed a large number of different reorganizations, Carnap and Bachmann exclusively focused on the logical status and the proper formulation of the completeness axiom that stated that the system could not be extended by further axioms without running into inconsistencies. The main problem was that – after Gödel – there existed different and logically inequivalent options to formalize “completeness”. This created severe difficulties for Carnap's general axiomatics and its pivotal role within the epistemology of science. Whereas Hilbert's axiomatic method started from a local analysis of individual axiom systems in which the foundations of mathematics as a whole entered only when establishing – in a second step – the system's consistency, Carnap and his Vienna Circle colleague Hans Hahn instead advocated a global analysis of axiom systems in general, in the form of an attenuated logicism or with reference to a formal language chosen on pragmatic grounds. A primary goal was to evade, or formalize ex post, mathematicians' 'material' talk about axiom systems. For such talk about the motivation and structural quality of different axiom systems was error-prone and susceptible to metaphysics, at least to the extent that it went beyond pragmatic considerations of simplicity or fertility. Most problematic from the Viennese perspective was Hilbert's repeated talk about ‘deepening the foundations’ of an axiom system, which Carnap and Hahn tried to counter by emphasizing that all axioms stood essentially on a par irrespective of whether they
were close to or remote from concepts actually used by scientists. If used properly however, or so I will argue, Hilbert’s ‘deepenings’ could be reformulated without raising ‘metaphysical’ concerns.

Saturday - 25 June (Parallel Session X)

Parallel Session X (14:00-16:00)

10.1 Symposium: The Natural and the Normative at 25: Psychology, Perception, and Measurement in Kant and Helmholtz

In the century that passed between the appearance of Kant’s Critique of Pure Reason and Hermann von Helmholtz’s mature writing, psychology -- its methods, the proper object of its investigation, and the possibility that it could produce genuine knowledge -- were matters of constant controversy. At issue for Kant and the post-Kantian tradition was the possibility of a priori transcendental psychology and its relation, if any, to empirical psychology. Could there be an a priori transcendental psychology, and if so, how? For that matter, could there be a rigorous, experimental psychology, and what would its methods be? But at least within the Kantian tradition, those questions were necessarily connected to others. For Kant, no empirical psychology could produce knowledge of a priori, transcendental cognitive processes. Thus prior to questions about the relation between transcendental and empirical psychology were questions about the a priori and empirical constituents of knowledge: what is the role of experience in constituting knowledge, and what is the role of a priori constituents in knowledge? Kant, Helmholtz, and others gave radically different answers to these questions. Gary Hatfield’s 1990 The Natural and the Normative: Theories of Spatial Perception from Kant to Helmholtz established these topics as central to the history of philosophy of science in the late modern period. In the 25 years since the book’s appearance, it has served as a starting point for and elicited responses about a range of issues: the place of the transcendental mind in Kant’s critical philosophy and the possibility of transcendental psychology; Helmholtz’s interpretation of and relation to Kant; the role of both empirical and a priori elements in Helmholtz’s theory of knowledge; Helmholtz’s “sign theory” of perception; and Helmholtz’s accounts of measurement and space. This panel will consist of three papers responding to different themes in Hatfield’s The Natural and the Normative, and a fourth paper by Hatfield responding to those papers. The first paper will argue that, for Kant, there is a complex relation between transcendental and empirical psychology, and that the former can in certain ways inform the latter. The second paper will take up the question of Helmholtz’s relation to Kant, arguing in particular that while Helmholtz rejects Kant’s theory of a priori forms of intuition, he is nevertheless committed to the idea that certain additive principles are necessary for judgments about quantity to be meaningful. The third paper will defend an interpretation of Helmholtz’s theory of perceptual complexity, with an eye to highlighting the enduring significance of his views for contemporary philosophy of psychology. Finally, Hatfield will respond to these papers. But he will also offer a revised interpretation of Helmholtz’s mature epistemology, focussing on the question of whether Helmholtz’s law of cause is an empirical or an a priori principle, and the consequences that question has for his theory of knowledge.
Empirical and Transcendental Psychology
• Corey Dyck (Western Ontario)

In his The Natural and the Normative, Hatfield rightly stressed the innovative character of Kant's distinction in the Critique of Pure Reason between empirical psychology as “the investigation of inner appearances” and a properly transcendental “investigation of thought.” Without questioning the importance of this distinction, in this paper I will argue that Kant nonetheless conceives of these two disciplines as standing in a complex relationship. On the one hand, empirical psychology depends upon the legitimating results of the transcendental investigation, a point which Hatfield likewise recognized, though I will argue that this is most evident in Kant's account of the transcendental basis of the causal laws governing empirical psychological investigation (presented in the Appendix to the Dialectic but also elsewhere). On the other hand, and I would argue of equal significance (especially concerning later appropriations of Kant's discussion), is Kant's use of the results of his transcendental investigation to drive his empirical investigation into the mind. I will contend that this is most evident in the Subjective Deduction in the A edition, where Kant makes use of his conclusions regarding the status of space, time, and the cognitive subject to draw new empirical conclusions regarding the constitution of the mind. In this way, I hope to underline the importance of Kant's distinction between empirical and transcendental accounts of the mind (and indeed of Hatfield's elaboration of it), but also to emphasize that this did not (for Kant) and should not (for us) amount to a rejection of the much-derided empirical investigation. Instead, Kant clearly saw it worth pursuing after the advent of transcendental psychology, and even held that the latter opened new avenues for empirical psychology.

The Natural and the Normative Reconciled: Helmholtz’s Theory of Measurement
• Francesca Biagioli (Universität Konstanz)

Hatfield's work on the theories of spatial perception from Kant to Helmholtz offered an illuminating account of Helmholtz's twofold relation to Kant: on the one hand, Helmholtz considered himself in agreement with Kant's commitment to a priori elements of knowledge (i.e., the law of causality and the subjective forms of experience); on the other hand, in a series of writings and public lectures on the foundations of geometry from 1868 to 1878, Helmholtz argued for the empirical, rather than a priori, origin of geometrical axioms. Finally, in his later works, he considered causality to be a guiding principle for the comprehensibility of nature, rather than an a priori principle in Kant's sense. Hatfield shed light on the latter development by showing that, even apart from the question of geometry, there was a tension between Helmholtz's view that empirical evidence provide ultimate criteria of knowledge and his assumption of a priori elements, which justify the possibility of knowledge from a Kantian perspective. This tension has its origin in one of the core ideas of Helmholtz's psychology of the senses, namely, the view of sensations as “signs” for unknown causes. Since we do not have immediate access to external reality, the meaning of such signs must be learned through experience, the learning process being mediated by the structures of the mind. The aim of this paper is to reconsider Helmholtz's relation to Kant in his later paper, “Counting and Measuring from an Epistemological Viewpoint” (1887), which contains one of the clearest expressions of Helmholtz's naturalization of the Kantian theory of the forms of intuition. He considered the concepts of number and of sum as taken only from the inner intuition of time in order to address the question: Under what conditions are mathematical symbols justifiably used to express metrical relations in the world? Helmholtz's answer was that such conditions can be formulated regardless of the entities to be measured as laws of addition; nevertheless, the same laws are necessary for judgments about quantities to have a meaning. In other words, additive principles play some role in the
constitution of the objects of experience, in that their domain of applicability defines what is measurable. At the same time, Helmholtz denied that a possible experience in general can be delimited a priori. Therefore, he replaced Kant's distinction between extensive and intensive magnitudes with a relative distinction between additive and nonadditive magnitudes. Helmholtz's demand was that the laws of addition be extended to all known physical processes, although composition according to the method of addition was yet unknown for such attributes as sense qualities. I argue that the advantage of Helmholtz's empirical approach lies in his dynamical perspective on the problems concerning measurement. My suggestion is that this approach – despite being in contradiction with Kant's views – does not necessarily contradict a dynamical perspective on a priori knowledge, as proposed by the Marburg School of neo-Kantianism.

Helmholtz on Sensory Complexity
- Lydia Patton (Virginia Tech)

In the nineteenth century, the science of the responses of sensory nerves to stimuli exploded. Johannes Müller, Ewald Hering, Franciscus Donders, Ernst Weber, Gustav Fechner, and many others investigated the reactions of sensory nerves to stimuli. The increasing sophistication of accounts of sensory perception only made it more evident that our visual, auditory, and haptic perceptions appear more complex than can be explained by appeal to such responses alone. Simple, linear combinations of responses of nerves to single stimuli do not add up to a satisfying explanation of perceptual complexity – of the perception of compound notes in chords, of the shades of light on leaves, or of depth of field. Hermann von Helmholtz's first scientific investigations into perception investigated compound tones and color mixing. The problem of complexity is among Helmholtz's motivations for the accounts in the Handbook of Physiological Optics and On the Sensations of Tone. Here, Helmholtz deals with the phenomenon of color contrast, and with our ability to construct a map of spatial relationships in memory, which contribute to the complexity of visual and acoustic images. Helmholtz's account has affinities with contemporary accounts of perceptual complexity: Computational accounts (Marr) appeal to the brain's ability to make inferences to explain complexity. Sensorimotor accounts (O'Regan and Noë) argue that complexity is explained by a continuing physical interaction between subject and environment. Cognitive penetrability (Siegel) has it that we can represent the same stimuli in distinct ways depending on our cognitive states and dispositions. Increasingly, some psychologists and neuroscientists (Biederman et al., Vladusich, Todd et al., and others) provide accounts of perceptual complexity that combine computational, sensorimotor, and penetrative explanations. These accounts are pluralist: a feature of our perceptual experience might be explained by appeal to the properties of sensory nerves, to the brain's computational power, or to the increasingly popular thesis that there is a "grammar" of perception. This pluralist trend is the true descendant of Helmholtz's method. Helmholtz develops an account according to which sensations, shifting attention, unconscious inference, and even stable illusions contribute to perceptual experience. The account treats the complexity of perception as an epistemic puzzle. The a priori law of causality is invariant: external objects are taken to be the causes of subjective sensations. Otherwise, the observed character of perceptual experience determines the solution to any particular puzzle. The themes of the talk follow the development of Gary Hatfield's contributions to the history, philosophy, and practice of science, from The Natural and the Normative to Perception and Cognition. Hatfield's emphasis on the epistemic character of Helmholtz's account, and on the significance of the law of causality to Helmholtz's explanations, are central to the account developed here. My talk will delineate Helmholtz's vivid response to the problem of perceptual complexity, and will explain how Helmholtz's view could provide a more flexible and powerful theory of perceptual complexity than its more singleminded rivals.
The Natural and the Normative and The Facts in Perception: Two Works Revisited
• Gary Hatfield (University of Pennsylvania)

My role as presenter in the proposed session would be to serve as commentator on the other three papers and to offer reflections on the book, The Natural and the Normative: Theories of Spatial Perception from Kant to Helmholtz. These roles will be combined. The three papers offer extensions of themes from the book: Kant on empirical and transcendental psychology; Helmholtz and the constructive complexity of perception in relation to sensory nerve responses; and Helmholtz's theory of measurement as relating the empirical to the a priori. In my role as commentator, I will respond to these papers (full drafts of which I will have beforehand) as extending, amending, or correcting the book and as arguments in their own right. In my reflections on the book, I will consider and revise its reading of Helmholtz's most ambitious epistemological writing, The Facts in Perception (1878). The focus will be on the place of the law of cause in this work and Helmholtz's distinction between the actual and the real. I will investigate whether Helmholtz adopted a purely empirical stance toward the causal law, or treated that law as a presupposition of empirical investigation (a kind of relative a priori), or held it to be a fully a priori (in Kant's sense) constitutive element of perception and knowledge. This will include an assessment of whether, in the end, Helmholtz posits things in themselves as underlying the relations exhibited among sensations in experience. In accordance with Helmholtz's view of sensations as signs, these things would be unknown except for the relations they exhibit. With a fresh reading of Helmholtz's Facts in hand, we can then consider the place of this work among his other writings on perception and the law of cause.

10.2 Symposium: Logical Empiricism and the German Youth Movement

The aim of this symposium is to study important aspects of the philosophical concepts of key figures of Logical Empiricism, such as Rudolf Carnap and Hans Reichenbach. More precisely, the goal is to trace Carnap's and Reichenbach's philosophical concepts back to their early involvement with the German Youth Movement. Our overall working hypothesis is that the sense of interrelatedness of philosophy, science, culture, and politics that was quite characteristic for Carnap and Reichenbach on the one hand but was more or less absent in most of the later representatives of Logical Empiricism has its roots in the German Youth Movement. The German Youth Movement consisted of a widespread number of organizations and clubs, most prominently the Wandervogel movement, founded by German high school students in 1896, and the Free Student Movement, founded by university students in Leipzig in the late 1890s. Both groups were intended as alternatives to the domineering traditional corporations: rejecting the exclusive drinking rituals of the corporations they promoted a new kind of sociability which included hiking, dancing, singing, and campfire discussions, preferably in natural settings. Starting in the teens of the 20th century, most clubs of the German Youth Movement were open for women, whereas the corporations stayed all male associations. Additionally, they were active in worker's education and strongly advocated pedagogical reforms. The German Youth Movement combined a rather backward-looking, melancholic sense for romantic poetry and medieval culture – they put on colorful medieval costumes and organized processions with horse carriages, medieval dances, and songs at the campfire – with an almost revolutionary sense of life reform – they intended to transform the whole of culture, including the rituals of everyday life, gender relations, art, science, and politics. Clubs such as the Jena Sera Circle or the Free Students were not just corporations in sheep's skin, insofar as they did not just intend to transform university sociability. They were also intended as alternatives to the university culture as a whole. The members of the Sera Circle, for example, with Carnap, the sociologist Hans Freyer, the
pedagogue Wilhelm Flitner, and the art historian Franz Roh among them formed a discussion group that tried to develop a fairly universal program of cultural reform, in which at least at the beginning the university context did not play a central role. Informed and deeply shaped by these experiences some of them – Freyer, Flitner, and, most prominently, Carnap – became representatives of a new university culture. As the examples of Carnap and Reichenbach show, these former members of the German Youth Movement preserved their beliefs in the urgency for reform of overall culture and society, implementing such a reform inside of the scientific community, using science as a tool.

‘Pacifist, Anti-militarist, Anti-monarchist, perhaps also Socialist': Carnap (and other Prominent Members of the German Youth-Movement) facing World War I
- Hans-Joachim Dahms (Institute Vienna Circle)

In 20th century Germany there took place at least three major youth movements (meaning not just a generation in the biological sense, but also as a cohort with a common set of experiences, world-views, principles about everyday life, organisations). The first one - before and also to some extent during and after - the First World War was formed by the experience of the (upcoming of the) war. The third one is known as the 1968-Generation. Between these two - it has to be admitted - came the National-Socialist student-movement, which contributed significantly to the "seizure of power" in 1933. My talk will briefly contextualize the first of these movements, against the background of the second and third. Then I will proceed to show how a number of important philosophers lived their formative years in the context of the youth movement and became deeply shaped by their experience of World War I. Most importantly, Rudolf Carnap and Hans Reichenbach were leading figures already in the Freistudentenschaft since around 1909 and after the war became co-founders of socialist student groups. But also some philosophers of a quite different outlook were members of the German Youth Movement and interacted with the aforementioned key-figures of logical empiricism, in this context. On the one hand, the left-leaning Frankfurt school philosopher Walter Benjamin interacted in the Free Students Movement in Freiburg and Berlin with Hans Reichenbach and Kurt Grelling, during the teens of the 20th century. On the other hand, Hans Freyer, who became a right-wing propagandist in the early 30th, was an important discussion partner for Carnap around 1920 and somewhat influenced his first major book, the Aufbau. My talk will set up that whole spectrum and then try to show how key concepts of logical empiricism such as the principle of tolerance (Carnap) and moral non-cognitivism (Carnap, Reichenbach) can be directly traced back to the German Youth Movement and can also be found in non-logical empiricist philosophers such as Benjamin and Freyer. I will use the available archival sources (in particular, Carnap’s diaries and the correspondences of Carnap and Reichenbach, as well as Wilhelm Flitner and Franz Roh; the available sources at the Archiv der deutschen Jugendbewegung, Burg Ludwigstein) in order to illustrate that these key narratives of logical empiricism were important elements of the Youth Movement already before (and during) World War One.

Carnap, Reichenbach, Freyer. The Social Adaptiveness of Values, in the Context of Logical Empiricism and the German Youth Movement
- Christian Damböck (Institute Vienna Circle)

Two pioneers of logical empiricism, Rudolf Carnap and Hans Reichenbach, placed discussions of values and value statements in the context of some of their major publications. Reichenbach’s The Rise of Scientific Philosophy contains two chapters on ethics (= ch. 4, 17). Carnap devoted the final chapter of his replies to his critics in the Schilpp volume to the topic of a philosophy of values, and in a late interview he also highlighted ethics, among inductive logic, as one of the most important philosophical tasks being left
for him. Both Carnap and Reichenbach defended quite similar non-cognitivist conceptions of value statements and they both somewhat based their overall understanding of our scientific world conception on the distinction between theoretical or cognitive questions as being a task for the sciences and practical or non-cognitive questions as being a task of culture and “emotion”. This paper argues that the latter dichotomic picture, in the cases of both Carnap and Reichenbach, can be traced back to their early involvements with the German Youth Movement. My starting point is an idiosyncratic essay Antäus. Grundlegung einer Ethik des bewussten Lebens, published by Hans Freyer in 1919, at a time where Freyer and Carnap intensively discussed cultural, philosophical, and political issues. My working hypothesis is that Freyer’s essay, though being devoted to a rather conservative and right wing conception, shows a certain sense of adaptiveness of values which also can be found in philosophers of the so-called left wing of Logical Empiricism such as Carnap, Reichenbach, and probably even Otto Neurath. Moreover, this adaptive conception of values and culture is something quite typical for the German Youth Movement, in which Freyer, Carnap, and Reichenbach have been involved during the teens of the 20th century. I will further illustrate this connection between the non-cognitivists idea of adaptiveness of value statements and the German Youth Movement, by means of a number of sources that became opened for research just recently: Carnap’s diaries and his correspondence with his family and other members of the German Youth Movement, such as Franz Roh and Wilhelm Flitner.

Rudolf Carnap and the Erlangen Conference (1923)

Christoph Limbeck-Lilienau (Institute Vienna Circle)

In 1923 Carnap organized a conference in Erlangen on the application of symbolic logic to epistemology (“Erkenntnistheorie”) and to philosophy of science. The conference gathered a group of young philosophers (Carnap, Reichenbach and Bernhard Merten) psychologists (Kurt Lewin and Fritz Heider), mathematicians (Heinrich Behmann) and physicists (Paul Hertz). The conference was not only the first occasion of a meeting between Carnap and Reichenbach, but also the reason for first contacts between Carnap and Schlick. Retrospectively, Carnap considered that the conference was quite an important historic event, presenting it as a “small but significant initial step in the movement of a scientific philosophy in Germany” (Carnap 1963). I will look into the motivation for that conference and consider the subsisting material of the talks as well as the relevant archival sources in order to evaluate the importance of that event for the history of Logical Empiricism. Besides the scientific intentions for the conference, I will also consider the broader social context which motivated the conference, especially the spirit of social and intellectual reform, with origins in the German Youth Movement. Carnap wanted the conference to be an encounter of young philosophers and scientists whose intellectual positions are still forming. And he wanted that the conference dedicates large parts to discussions in order to develop a common position. The informal discussions in the German Youth Movement can be seen as a model for the conference’s attempt for a reform of philosophy in a spirit of open discussions. Indeed, several participants of the conference took part in the German Youth Movement (Carnap, Reichenbach, Merten). The Erlangen conference can be seen as a more academic version of discussion groups Carnap had previously tried to form with friends of the Youth Movement, as well as an expression of the “will to reform” (“Wille zur Neuordnung”) which he advocated in his first sketch for the Aufbau (“From the Chaos to Reality”). Carnap’s main philosophical intention for the conference was to present his structuralist conception of knowledge, based on Russell’s logic of relations. I will sketch the reception of Carnap’s program at the Erlangen meeting. With the exception of survey by Christian Thiel (1993), Carus (2007: 154-9) and a recent paper on Karl Gerhards’s influence on the Aufbau by Alan Richardson (Carnap developed the plan for the 1923 meeting together with Gerhards) there is little research on the seminal Erlangen meeting. The aim of

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this contribution is to fill this gap and to illustrate the aims, content, development and reception of this meeting, by means of the available archival sources, in particular, Carnap’s diaries and the scientific correspondences of Carnap, Reichenbach, Schlick, and others.

Carnap’s Turn to Philosophy of Science from the Perspective of his Diaries, Reading Lists and Personal Letters

• Lois Marie Rendl (Institute Vienna Circle)

According to his autobiography Carnap began to work on a dissertation in experimental physics in 1913, but after the first world war he realized that his inclinations and abilities where of a rather theoretical nature. In 1920 he finally decided that his field of research would be philosophy of science. The first result of this decision was his dissertation Der Raum. Ein Beitrag zur Wissenschaftslehre, written in 1920/21. Whereas the philosophical influences on Carnap’s dissertation are rather well known and studied, his initial move toward philosophy of science was only recently considered in the context of his engagement in the German Youth Movement and his experiences in the First World War by scholars such as André Carus and Hans-Joachim Dahms. The aim of this paper is to reconstruct the links between Carnap’s earliest notion of philosophy of science as developed in 1920 and his involvement with the German Youth Movement. This will be studied on the basis of Carnap’s diaries and reading lists and his correspondence with his family and colleagues such as Wilhelm Flitner and Franz Roh. These documents, for its most parts, were not yet considered in the secondary literature, either because there did not exist any transcriptions of Carnap’s shorthand (as it is true for the diaries and reading lists) or because the material has been closed for research until recent times (as it is true for the private correspondence and a number of other important sources). These sources, in particular, allow us to reconstruct Carnap’s move away from religion in spring 1920 and provide additional documentation on the Buchenbach meeting in August 1920. The importance of the latter meeting as the first step toward the Aufbau was pointed out in a recent paper by Hans-Joachim Dahms. The aforementioned sources allow us to enrich our picture of this seminal meeting in 1920 and therefore to clarify Carnap’s initial philosophical program.

10.3 Contributed Papers: Ancient HOPOS

Galen on Analytical Method in On the Diagnosis and Cure of the Soul’s Errors

• Donald Morrison (Rice University)

The main texts in which Galen presents methods of analysis are found in three works: On Therapeutic Method, The Diagnosis and Cure of the Soul’s Errors, and the Art of Medicine. A great deal of confusion has arisen because interpreters have tended to assume that the analytical methods discussed in these works are one and the same. In fact, Galen’s most original discussion of analytical method is found in chapters IV and V of The Diagnosis and Cure of the Soul’s Errors. The method he describes there is an outlier in the tradition. It is quite different from the analytical methods described by rough contemporaries such as Alexander of Aphrodesias and Alcinous. I shall argue that The Diagnosis and Cure of the Soul’s Errors Galen uses “analysis” for three different kinds of method: (1) practical analysis that arrives at actions to be performed; (2) theoretical analysis (often called “geometrical analysis”) that arrives at propositions rather than things done; and (3) criterial analysis. What is original and important is the idea of criterial analysis. The idea of practical analysis goes back to Aristotle’s ethics (perhaps via Aspasius). But Galen’s approach is distinctively Hellenistic. The central concern of Hellenistic epistemology was the problem of the criterion: what can we (ultimately) look to as guide in making judgments. Different schools
of philosophy not only have different conceptions of the good life; they also have different views of the criteria by which their views must be judged. To justify these criteria, a philosopher must eventually stop with an appeal to the most fundamental or primary criterion. In order to know which school's conception of happiness is correct, one must first settle the problem of the criterion. Galen's account of criterial analysis is something new in the history of thought, not precisely the same as either problematic or theoretical analysis. Criterial analysis ends not in a proposition or an action to be taken, but a criterion by which statements about propositions or actions may be judged and evaluated. Problematic and theoretical analysis are employed by geometry. But criterial analysis is not a method employed by geometry. Criterial analysis is, in fact, a method distinctive of epistemology, though as Galen explains it has a role in what we can call “everyday epistemology” as well as in philosophy. The example Galen uses to illustrate his analytical method is an engineering problem: a practical problem that makes use of some geometry. The problem is the construction of an accurate timepiece for the city. I shall go through Galen's example of the construction of a sun-dial to show how the process of analysis to a primary criterion works. Finally, Galen uses the possibility of employing the analytic method as his basis for judging between science and pseudo-science. What matters for science is whether criterial analysis can be completed; i.e., whether it is possible to eventually identify a starting-point that is clear and evident to everyone.

Aristotle on the Domain Specificity of Scientific Inquiry

James Lennox (University of Pittsburgh)

In a well-known passage in the Prior Analytics, Aristotle draws a surprising implication from his view that each science has principles that are distinctive to it: The majority [of principles] for each science are distinctive (ἴδιαι) to it. Consequently, it is for experience concerning each subject to provide the principles. I mean, for instance, that it is for astronomical experience [to provide the principles] for the science of astronomy (for when the appearances had been sufficiently grasped, in this way astronomical demonstrations were discovered; and it is also similar concerning any other art or science whatsoever). (APr. I.30, 46a17-27) Not only do different sciences have distinctive principles, but arriving at these principles depends on experience that is equally distinctive—astronomical principles are provided by astronomical experience. Moreover, this passage concludes by generalizing from this example to every τέχνη and ἐπιστήμη. In this talk I discuss some important, and perhaps startling, consequences of this view of experience. I argue that Aristotle cannot have a universal ‘logic’ of induction, if induction is understood, as Aristotle understands it, as the path by which one moves from perception through experience to the universals that constitute the principles of a science. The methods that are required for successful scientific inquiry are as domain specific as are the principles that successful inquiry uncovers. This explains why Aristotle's brief characterizations of induction are so uniformly uninformative: at the domain neutral level, there is very little to say beyond—‘experience provides the principles’ (above) or ‘it is by induction that perception implants the universal’ (APo. II.19, 100b5)! Aristotle does, however, have a great deal to say about the methods for acquiring appropriate, domain specific experience in a manner that prepares you for the discovery of the natures and causes of the phenomena of interest in each domain. Most of this paper is an exploration of Aristotle's rich and complex views about inductive, causal inquiry, views that are presented and defended in the context of his actual scientific inquiries, not in the thin air of his Analytics or Physics.
A Sliding Scale of Experiment-Kinds in Ancient Greek Science  
- Paul Keyser (Google, Inc. - Chicago)

Scholars contemplating the nature of “modern” scientific endeavor have often argued that “experiment” is a distinctive or even definitive feature of “science,” and correlatively that experiment was absent in prior endeavors, which are thus excluded from science. However, the claim that science emerges in a sudden breakthrough assumes either the emergence of new cognitive capacities, or the new deployment of existing capacities, and neither hypothesis admits a sensible account of that occurrence (Lloyd 2009, 159-160). The same point is valid for experiment – it could not have emerged suddenly. Bacon, Novum Organum Scientiarum (1620), clarified how experience is crucial to developing scientific models: encountered experience shows a path to organized experiences (i.e., experiments), that then lead to theories, which in turn motivate further experiments of a more developed kind (§1.82, 1.117); he also attempted to categorize various kinds of organized experiences (§2.2152). That is a sliding scale of increasingly organized experiences, which when sufficiently digested and ordered are “experiments” (cf. Frey 2015). Although Bacon criticizes Greek scientists for lacking experiments (§1.63, 1.73), he also cites the Hippocratic Epidemics as embodying experiment: De Augmentis Scientiarum (1623) §4.2. In ancient cultures that manifested any scientific activities, experience was always an element of their scientific thought. I will focus on ancient Greek sciences, and argue that they did not wholly lack experiments. I build my argument using examples drawn from Greek texts of the fifth century BCE up to the third century CE, treating a variety of sciences. The deployment of experiment (or even experience) was not uniform or systematic. My approach thus differs from Lloyd (1964) and von Staden (1975), who did argue that ancient Greek science deployed experiments, but assumed any candidate procedure either is – or is not – an experiment. My approach allows for, even insists upon, gradual development. Not every feature of a careful modern experiment is present in all my examples, but the texts that I will cite describe what we may fairly call experiments. The procedures described fall along a sliding scale, between raw observations and the elaborate controlled repeated experiments sought in modern science. Some are little more than demonstrations, but some involve measurement, some involve constructed apparatus or situation, some involve repeatability, and some involve accepting an unexpected result (Berryman 2015, 252). Modern experiments themselves often lack some aspects of a fully developed experiment (Hacking 1983, 149185; Open Science Collaboration 2015).

Aristotle, Pluralism, and the Essentialism Story  
- Justin Bzovy (Western University)

Species pluralism is the view that there can be multiple, non-unified ways of grouping organisms into species. Given the overwhelming number of competing species concept (cf. Mayden, 1997; Wilkins, 2011), pluralism seems obvious. But species pluralism hasn’t always been obvious, so why has it only recently been considered defensible? (Mishler and Donoghue, 1982; Kitcher, 1984; Mishler and Brandon, 1987; Dupr´e, 1993; Ereshefsky, 2001). At first glance, two stories explain the rise of pluralism. According to one, pluralism became fashionable only after general problems with monism, unification, and reductionism became apparent (e.g., Oppenheim and Putnam, 1958; Suppes, 1978; Dupre, 1993). According to another, pluralism became fashionable only after a certain threshold in the amount of differing species concepts was reached. I argue that these accounts fail to tell us the complete story. Before pluralism became fashionable, debates about species concepts were framed monistically (Dobzhansky, 1935; Mayr, 1942; Simpson, 1951; Sokal, 1962). Everyone agreed that the true concept was not Aristotelian, essentialist, or typological (e.g., Hull, 1965). For an Aristotelian, beavers (C. canadensis) do not evolve, because
canadensis is fixed and eternal type. An underlying essence explains why members of \textit{C. canadensis} are similar. For an evolutionist, species are evolutionary units, and we need a species concept that reflects this role that species play. I show how an Aristotelian species concept was part of a larger rhetorical strategy developed by Ernst Mayr (1940; 1942; 1957; 1963). Mayr provided a framework which linked both an essentialist conception of species, and an post-Darwinian species concept with monism. With the prevalence of his strategy in disputes about species, we were all too distracted by the essentialist witch-hunt that ensued to notice whether or not anything was wrong with the assumption of monism. Recent work has shown that the essentialism story that this rhetorical strategy was built upon is incorrect (McOuat, 2001; Winsor, 2003, 2006a; Levit and Meister, 2006), both with respect to Aristotle’s own theory of species (Henry, 2011, 2014; Leunissen, 2014), and with respect to other pre-Darwinian taxonomists (Winsor, 2006b). Despite these problems with the story, I show how Mayr’s strategy can be developed as a foil for current accounts of species pluralism (e.g., Dupr’e, 1993; Ereshefsky, 2001) given a pluralist reading of Aristotle. Many argue that Aristotelian science allows for an approach to classification that is, in practice, highly pluralistic (Henry, 2011, 2014; Leunissen, 2014). From this perspective we can understand the assumptions at stake between modern species pluralist. I will argue that Mayr’s essentialist story left us asking the wrong questions about evolutionary theory’s effect on species, but that having a deeper understanding of Aristotle’s approach allows us to ask the right questions. First, pluralists need to consider the explanatory power of all taxonomic ranks. For Aristotle species are not necessarily the basal taxonomic units. Modern pluralists, generally do hold that they are, even if there are different ways of conceiving of the basal level (e.g., Mishler and Donoghue, 1982). Second, pluralists must reconsider the significance of the species category problem (Ereshefsky, 1992, 2001; Pigliucci, 2003, 2005; Queiroz and Donoghue, 1988; de Queiroz, 1998, 1999, 2005, 2007; Dupre, 1999; Ereshefsky, 2014a). Aristotle’s pluralism shows that solving this problem is a wasted effort. Third, pluralists ought to consider whether intrinsic, rather than relational or historical properties, are required to group and rank species. Modern pluralists remain divided on this (e.g., Kitcher, 1984; Ereshefsky, 2014b), and Aristotle gives us a valued means of comparison. Lastly, Aristotle can help us interpret the “cross-cutting” metaphor endemic to the pluralist literature (e.g., Dupre, 1999; Ereshefsky, 2001). For Aristotle, cross-cutting happens at various levels, though not at the population level. Further, modern pluralists have much to learn from his criticisms of Plato’s bifurcating division, and his own use of multifurcating division to get at the right explanatory level of biological organization. In sum, for pluralism to truly supplant monism, the essentialist story needs to be rewritten with these questions in mind.

\section{Contributed Papers: Mathematics and Method in the 18th Century}

\textit{Émilie du Châtelet on the Fundamentality of Change}

- Aaron Wells (University of Notre Dame)

Émilie du Châtelet’s Institutions de Physique offer a metaphysical foundation for physics. In this paper, I explain du Châtelet’s response to the problem of persistence or change, which is a central part of this project. Unlike Leibniz and Christian Wolff, du Châtelet insists that real change occurs at the fundamental level of reality, and that the capacity for change is essential to substances, rather than superadded by God’s will. Recent debate over the problem of change centers on time or tense – how to understand properties as somehow temporally relative. Du Châtelet, notably, does not appeal to features of time at all, but only to causal interaction, in approaching the problem of change. Very briefly, the problem of change is as follows: if objects persist through genuine change, they appear to have incompatible properties; this in turn seems to violate the principle of the indiscernibility of identicals. It’ll be helpful to highlight two of the
many possible responses to the problem of change. Medieval and early modern approaches, by contrast, often appeal to the ontology of substances and their accidents. On such an account the substance as such, respect to its nature or essential properties, does not change. Rather, its accidents, determinations, or modes do. Leibniz does not adopt either of these strategies. His fundamental substances (monads) are atemporal and non-interactive. He also rejects any relevant distinction between substance and accident, insofar as he takes all the properties of a monad to be essential to it. It's debatable whether Leibniz even takes monads to undergo, as opposed to ground, meaningful change. Wolff is agnostic about the nature of fundamental reality, yet takes causal interaction and change to be explained by what he calls active force. Yet he maintains that nothing grounded in a substance's essence can change without annihilating the substance; any property so grounded would also be an essential property. So active force, and therefore the capacity for change, is not essential to Wolff's substances. Seemingly, it is superadded to essences if and when God chooses to create them. Like Leibniz, Du Châtelet takes time to be non-fundamental. Nonetheless, by her lights fundamental substances, though not spatiotemporal, causally interact and undergo change. For they have different properties depending on which causal relations they stand in. Properties are relativized to interaction partners, just as properties can be relativized to times. This account rests on du Châtelet's distinction between essential properties and modes. The essence specifies only that a substance must have certain modes, but does not determine how the modes are, qualitatively. It is essential to a substance, not merely a matter of superaddition, that it have some changing determinations (which determinations it has will be determined by its causal interactions). Corresponding to this metaphysical distinction is a division in orders of explanation. Du Châtelet distinguishes a priori metaphysical reasoning about essential properties from a physics that tracks the changing modes of substances.

Émilie Du Châtelet on Scientific Methodology

- Katherine Brading (University of Notre Dame)

In 1740, Émilie Du Châtelet published her Institutions de Physique, in which she presented a systematic natural philosophy drawing primarily on Descartes, Leibniz, Wolff and Newton. The book is important within history of philosophy of science for multiple reasons: it was the first to attempt such a synthesis (so far as is known); it was widely read, appearing in two editions, the second of which was translated into both German and Italian; and crucial passages were reproduced (often without attribution) in the highly influential Encyclopédie of Diderot and d'Alembert (publication of which began in 1751). In my paper, I argue that Du Châtelet's attempted synthesis stems not from the metaphysics or the physics of the Institutions (as has been presumed in the literature until now), but from the scientific methodology that she develops in that text. I argue that this scientific methodology is highly sensitive to the methodological challenges faced by early eighteenth century natural philosophers. These challenges centered around the appropriate roles in scientific theorizing for a priori principles and for empirical input, including (famously, following Newton's "hypotheses non fingo") what role, if any, hypotheses should have. The methodological challenge was made acute by the epistemological crises of the seventeenth century, and in particular the problem of the "system of the world". By the time that Du Châtelet was writing, the heliocentric view had achieved ascendency, but the challenge posed by observationally equivalent theories remained vivid, not just in the memory but also as a pressing live problem in evaluating vortex versus action-at-a-distance theories of gravitation. I show that Du Châtelet offered a sophisticated, normative account of the roles of a priori and empirical elements in scientific reasoning, and the interplay between them, including the crucial role of falsifying instances. Given the afterlife of her proposed methodology, especially in the Encyclopédie, a careful examination of her scientific methodology is important for history of philosophy of
science. The structure of my paper is as follows. Since Du Châtelet's work remains largely unfamiliar, I begin with an exposition of the key elements of her methodology, as explicitly set out in the early chapters of the Institutions. I show that this methodology synthesizes key elements drawn from Descartes, Leibniz, Wolff and Newton. I then argue that this methodological synthesis is at work in her discussion of specific examples from physics (such as gravitation) later in the text, and make my case that it is her scientific methodology that unifies her text. Finally, in order to make the case for the importance of her contribution to scientific methodology, I situate my analysis of her text in the broader context of the methodological and epistemological challenges of the time at which she was writing, and highlight relevant responses to her work. Du Châtelet's Institutions de Physique is emerging as an important text in the history of philosophy, and my paper is intended as a contribution to our understanding of the significance of this text for history of philosophy of science.

Wolff and Kant on the Mathematical Method

Elise Frketich (KU Leuven)

Kant's discussion of the mathematical method has been of interest to scholars both as a kind of appendix to the Transcendental Aesthetic (Guyer 2006, Parsons 1992), and for informing interpretations of Kant's mathematics (Carson 1999, Friedman 1985, Hintikka 1967, Schabel 1998, Young 1982). However, it has not yet been interpreted in view of its critical aim: to exclude the mathematical method from philosophy. In order to understand Kant's view, a clear grasp of the mathematical method is required. To this end, I will first expound Wolff's particular method, and then discuss the argument Kant raises against it. The mathematical method, generally speaking, is the axiomatic-deductive method (of Aristotle and Euclid), which consists of chains of Aristotle-approved syllogisms. However, Wolff's version is unique. On his own description, it employs chains of syllogisms which are specifically "geometrical" (German Logic, Ch.4, §22). I will argue that the "geometrical" aspect of Wolff's method consists in producing a necessary premise from the experience of an individual thing (as opposed to, e.g., forming a universal premise by abstraction). According to Wolff's examples, such premises are about things in nature, which, nonetheless, form a part of apodictic syllogisms. To clarify the metaphysical underpinning of Wolff's method, I will sketch his modal logic, which I will argue is best characterised as an S5 modal logic. For, not only is Wolff committed to the weaker principle $p \rightarrow \Diamond p$ (German Metaphysics (GM) §15), but also to the stronger S5-formula: $\Diamond p \rightarrow \Box \Diamond p$ (GM §38). However, on my view, Wolff exceptionally accepts the following, which is even stronger than the S5-formula: $\Box [\text{for some object } o \text{ and some essential attribute } \alpha, p \text{ expresses that } o \text{ has } \alpha' \rightarrow (\Diamond p \rightarrow \Box p)]$. On Wolff's view, these modal principles express the rigidity of essences and causal connections between things in nature. Accordingly, for Wolff, his modal logic justifies his treatment of concepts of actual things as concepts of geometrical figures; that is, he treats them as what Kant calls "mathematical cognitions" which can produce a necessary premise from a single experience. For Kant, by contrast, a mathematical cognition is essentially distinct from a cognition of an actual thing. While the former is a "cognition from the construction of concepts" (A713/B741), the latter is a "rational cognition from concepts" (A723/B751). The former is gained by representing a perfect exemplar of the concept in a priori intuition, e.g., a geometric figure, and inferring properties which necessarily belong to it. By contrast, the latter is discursive; it subsumes appearances under concepts according to their real, empirical content (in accordance with the rules of empirical synthesis) (A723/B751). While cognition of an actual thing can only consider the particular in the universal, according to Kant, mathematical cognition considers the universal in the particular (A714/B742). Thus, Kant concludes that a single experience of an actual thing cannot produce a necessary premise, while one representation of a geometrical figure can; accordingly, he concludes that Wolff's mathematical method cannot be employed within philosophy.
It is often assumed that the transition of Newtonian to Lagrangian mechanics was a straightforward translation of Newton's geometrical demonstrations into the language of the calculus. Ernst Mach even claimed that nothing fundamental occurred between Newton and Lagrange. In my paper I want to reconsider this historical development from a philosophical perspective. Whereas it was one of Newton's lifelong endeavours to render natural philosophy demonstrative and certain by transferring the certainty of mathematics to natural philosophy, eighteenth-century scientists became increasingly disinterested in such epistemological questions. Consequently, despite the “unreasonable effectiveness” of the formalism of the calculus, it became also less and less clear how the mathematics applied in physics can produce certain knowledge, and in which sense mathematical formulas represent physical reality. Even though most practicing physicists and mathematicians were rather pragmatic and often naive in their formal manipulations of symbols, they were confronted occasionally with an epistemological impasse caused by the incongruence of mathematical models and reality. I will briefly discuss two examples: ‘the problem of determining the shape of the earth’ (Maupertuis, Clairaut) and ‘the vibrating string controversy’ (Bernoulli, Euler, d'Alembert). In both cases the debate was about the meaning of a mathematical solution to a physical problem – a solution which itself seemed to have no bearing anymore to the physical world. Mathematical physics became an exercise of purely rational thought (i.e. the algebraic manipulation of equations and symbols) divorced of empirical content. Once a problem was expressed mathematically, it was lost to the realm of experiments and experience at large. Accordingly, the realm of mathematical intelligibility and modelling was no longer connected in a straightforward manner with the empirical world. The distortion of the relation between mathematics and nature ran even deeper on a methodological level. Newton started his Principia by introducing his axiomata sive leges motus, which formed an empirical starting point for his mathematical modelling. Many eighteenth-century physicists found this foundation too weak and wanted to base rational mechanics on self-evident, clear and necessary mathematical principles. They were convinced that they could proceed in physics more rigorously by following the axiomatic-deductive method of Euclid and by using the analytical approach of the calculus. But how can purely mathematical principles ground and organize a system of knowledge about physical reality? How can rational mechanics still incorporate experimental practices when it has become a project that only aims to construct a logically coherent system of axioms, principles, and concepts from which certain mathematical deductions can be made? Even though the Euclidean ideal in mechanics (cf. Pulte 2001) can be found in the works of d'Alembert, Maupertuis and Euler, I will limit myself in my paper to the most radical case, Joseph-Louis Lagrange, who in his Méchanique analitique posited the mathematical principle of virtual velocities as “a kind of axiom for mechanics” and wanted to derive the whole of mechanics from this a priori principle.
Philosophy of history and history of philosophy of science make for an interesting case of “mutual containment”: the former is an object of inquiry for the latter and the latter is subject to the demands of the former. This talk will discuss a past seminal turn in philosophy of history with an eye to the practice of historians of philosophy of science. The narrative turn by Danto and Mink represents both a liberation for historians and a new challenge to the objectivity of their findings. I will claim that good sense can be made of “working historical veins of possibility” (contrary to how the phrase was originally intended) and that already Danto and Mink provided materials (though they did not quite deploy them this way) to assuage fears of a reconstructionalist free-for-all.