AGAY-BEAUJON, William

Neurath's Reading of Diderot. Between Mentions, Revendications and Similar Attitudes

Abstract: Among the protagonists of the Vienna Circle, Otto Neurath appears to be the one who mentions the Enlightenment the most. Together with Hans Hahn and Rudolf Carnap, their 1929 manifesto presented their view as a "scientific world-conception" understood as a « spirit of Enlightenment and anti-metaphysical factual research ». His leading role regarding this topic can be explained with his wide range interest in the history of philosophy, his view on the historical roots of their thought and most importantly by his growing focus on the idea of unity of science and especially his big editorial project of the International Encyclopedia of Unified Science.As the main editor of the 18th century Encyclopédie and a leading figure of the French Enlightenment, Denis Diderot could be the main source of inspiration. But in Neurath's writings, Diderot has fewer mentions than Voltaire, D'Alembert, Condillac or Rousseau. Evidences tends to show that Neurath starts to read more closely Diderot in the late 30's. Especially in 1938 when he's writing articles about his view on encyclopedism, around the publication time of the International Encyclopedia of Unified Science first volume. This paper aims at reconstructing Neurath's reading of Diderot, linked with the elaboration of their new encyclopedia.I would like to demonstrate that before the 30's, Neurath seems to know only vague elements of Diderot's thinking, even during the elaboration of the International Encyclopedia of Unified Science when he takes encyclopedism as an epistemological model and the French one as a renewed inspiration. This analysis will be based on Neurath's mentions of Diderot and his Encyclopédie before 1935.Then I would like to reconstruct Neurath's reading of Diderot's works around 1938 when he became more aware of their proximity and revendicates explicitly his heritage for their new encyclopedia. My analysis will be based on Neurath's notes of his reading of Diderot's philosophical works, (still in his exile library kept at the Wiener Kreis Gesellschaft). And the similar arguments he made inside "Unified Science as Encyclopedic Integration" (1938). But also, on a few letters to R. Carnap and R. Schappire found in archives (kept at the Österreichische Nationalbibliothek).Finally, I would argue that the main parallel that could be made between Neurath and Diderot is their similar attitudes. Both on a theoretical and a practical level emerging from different problems and historical context. As "polymaths" engaged in society, driving force of the groups and projects they initiated.Indicative bibliography:Diderot, D. (2018) Œuvres philosophiques. Classiques Garnier. Edited by P. Vernière. Paris: Garnier (Classiques Jaunes).Nemeth, E. and Roudet, N. (eds) (2005) Paris-Wien: Enzyklopädien im Vergleich. Wien ; New York: Springer (Veröffentlichungen des Instituts Wiener Kreis, Bd. 13).Neurath, O., Morris, C. and Carnap, R. (eds) (1955) International Encyclopedia of Unified Science. Chicago, Illinois: The University of Chicago Press (1–5).Neurath, O. (2021) Gesammelte Schriften. Band 1: Gesammelte philosophische und methodologische Schriften. Neuauflage. Edited by R. Haller and H. Rutte. Wien:

LIT.Neurath, O. (2021) Gesammelte Schriften. Band 2: Gesammelte philosophische und methodologische Schriften. Neuauflage. Edited by R. Haller and H. Rutte. Wien: LIT.

Aiello, Miriam

Leibniz on the Relationship Between Light and Substantiality

Abstract: In recent years, a growing body of literature has emphasized how Leibniz's interest in optics can shed significant light not only on the broader architecture of the principles of his philosophy of physics but also on some of his metaphysical commitments. It is quite indisputable that his strong interest in optics (Unicum opticae, catoptricae, dioptricae Principium, 1682) facilitated his formulation of an argument from design and the vindication of the role of final causes in the study of nature (Tentamen anagogicum, 1696; Discourse on Metaphysics, 1686): the reflexive and refractive behaviors of light, captured in light of the Most Determined Path Principle, serve as a model to demonstrate the inadequacy of a purely geometric consideration of phenomena and their analysis in terms of efficient causes alone. However, in these texts the well-known Leibnizian exhortation to seek the metaphysical foundation of phenomena beyond their purely geometric description is exemplified precisely by a geometric model of light behavior, albeit teleologically adjusted, without Leibniz ever delving into reflection on the nature of light, towards which he shows a surprisingly timid attitude. Based on these considerations, in this paper, I will explore the ambivalent attitude that Leibniz exhibits toward light: on one hand, light is included in the realm of apparent qualities lacking substantiality (De modo distinguendi phaenomena realia ab imaginaria, 1702), and on the other, it is massively used to elucidate fundamental characteristics of substances and in particular their capacity to act.

Amichay, Suf

Medieval Jewish Radical Aristotelianism: a Study case in Scientific Isolation

Abstract: I argue for a reconsideration of the scientific orientation of the Medieval Jewish tradition, in anticipation of a larger project to follow. The presence of Jewish thinkers in the history of medieval science often remains overlooked. In an era defined by two dominant scientific traditions-the Islamicate and the Latin-Christian-Jewish individuals existed within both societies, yet were mostly prevented from participating in the main scientific institutions. I claim that this exclusion fostered an environment of intellectual isolation which gave rise to a radical approach to science. Amongst medieval Jewish thinkers emerged a group which championed Aristotelian natural philosophy over religious dogma; this at a time when both Islamicate and Christian societies veered away from Aristotelian science due to its perceived incompatibility with the teachings of Islam and Christianity. This group, including famous names such as Gersonides and Crescas, but also lesser-known thinkers such as Samuel Ibn Tibbon and Isaac Albalag are currently described as 'Jewish Averroists'; I contend that this classification is misleading and offer a new classification, the 'Radical Jewish Aristotelians'. The defining tenets of this group were commitments to the Aristotelian theories of perpetual celestial motion, the perseverance of species and the Principle of Plenitude. These commitments were nothing less than radical at the time. I suggest that this Jewish school used its extreme scientific approach to validate itself within an adversarial intellectual environment and to assert their right to participate in the larger discussion. Nevertheless, I show that Judaism, by being older than its Abrahamic counterparts, was more harmonizable with ancient theories of science. In choosing these radical commitments the Jewish authors were also innovating in philosophy while remaining orthodox in their religious interpretations.

Amijee, Fatema

Du Châtelet's Causal Idealism

Abstract: I show that unlike her rationalist predecessors Leibniz and Wolff, Du Châtelet is committed to epistemic causal idealism about natural causes, where a 'natural' cause is just one that is not divine. According to this view, it is constitutive of what a cause is that it is in principle knowable by us (i.e., finite beings). Epistemic causal idealism is akin to Kantian idealism, according to which the objects of experience (i.e., Kantian appearances) do not depend on the mind for their existence, but nevertheless fail to be "experience transcendent": if an object exists, then it is possible for us to experience it. Likewise, my claim is that for Du Châtelet, if a cause exists in the natural world, then it is possible for us to know it-natural causes just are possible objects of knowledge. I claim that Du Châtelet thus rejects what we might call "knowledge transcendence" for natural causes. I argue that Du Châtelet's commitment to epistemic causal idealism stems from three distinct views. First-like her predecessor Leibniz-Du Châtelet holds that sufficient reasons are reasons that are intelligible. But instead of endorsing a general intelligibility constraint, she goes further: for her the class of relevant agents for whom sufficient reasons must be intelligible includes us, finite human beings. Second, Du Châtelet holds that natural causes are good insofar as they 'satisfy' the principle of sufficient reason, and so any intelligibility constraint on sufficient reason applies equally for her to natural cause. Third, Du Châtelet's particular epistemic constraint on sufficient reason-and thus on natural cause-stems from the theoretical role the PSR plays in her system, as well as the argument that she puts forward for the PSR. This feature, along with the absence of any other explanation for the modal connection between natural causes and their intelligibility for us, makes the intelligibility constraint constitutive of what a natural cause is for Du Châtelet. I thus show that far from merely explicating Leibniz's metaphysics, Du Châtelet develops a radical and novel rationalism that is in keeping with her core commitment to science.

Amin, Ramy

Real or Not, it Matters Not: Newton's Gravity as Weak Emergence

Abstract: Commentators on Newton's conception of gravity grapple with three main questions regarding the status, cause, and nature of gravity. These questions are: Is gravity a genuine cause (a real force) in its own right, or is it only a mathematical representation of an underlying undiscovered real physical force? Is gravity intrinsic or extrinsic to bodies? And is gravity's action (or gravitational effects) carried locally (through a medium) or can it operate at a distance (with no medium)? The purpose of this paper is not to give answers to those questions, it is rather to advance the following two claims: 1) that interpretations of Newtonian gravity can be unified under the concept of weak emergence, and 2) that this epistemological move allows us to see the possibility that, despite Newton consistently describing gravity as a force, he may have thought of it more in line with our modern conception of powers, which focuses on the causal contribution of certain features, rather than the traditional understanding of force, which requires citing the entity causing the observed phenomenon as a necessary part of the explanation. In doing this, I show that so long as Newtonian gravity is thought of in terms of forces, the contention remains, while in shifting to thinking of Newtonian gravity in terms of powers, the contention disappears. In section I, I present a map of different answers to the three questions, in section II, I argue that readings of Newtonian gravity fit the profile of weak emergence, in section III, I taxonomize readings of Newtonian gravity under the schema of weak emergence, and in section IV, I propose thinking of Newtonian gravity in terms of powers instead of forces as a way to bridge the split.

Angleraux , Caroline

Applying cybernetics to the study of living organisms: the case study of Henri Laborit

Abstract: Emerged in the middle of the 1950s, cybernetics is an interdisciplinary field that aims to explore how information is transmitted within complex systems. When applied to physiology and the broader field of biology, cybernetic concepts offer valuable insights into organic regulatory processes. Notably, cybernetics, with its emphasis on retroactive and autoregulated mechanisms, significantly influenced the development of theories concerning living beings, such as the one on autopoiesis by H. Maturana and F. Varela during the 1970s. As a fervent advocate of cybernetics, Henri Laborit was a surgeon in the French Navy and distinguished himself notably for his groundbreaking discovery of the first neuroleptic, chlorpromazine, a pivotal treatment for psychosis. Later, as an independent researcher, Laborit established his own laboratory at the Boucicaut Hospital in Paris. As Laborit claimed it, "life works cybernetically" (Biology and Structure, 1968, p. 44) and many of his books started with a dedicated chapter on cybernetics – see for instance Physiologie humaine, cellulaire et organique (1961), L'agressivité détournée (1970) or La nouvelle grille (1974). His conviction in the cybernetic interpretation of living beings was so profound that he structured his laboratory into three sections, each devoted to research at distinct organic levels: the cellular, the organ-specific, and the whole body levels. Despite financial constraints leading to the temporary closure and subsequent reopening of the Boucicaut laboratory, Laborit remained resolute in organizing research endeavors guided by cybernetic principles. In his second lab, he continued the tripartite division, focusing on the molecular, cellular, and behavioral levels. In both cases, the central objective was to scrutinize the interactions among diverse organic levels, aiming to comprehensively understand the impact of various phenomena (be it a molecule, an enzyme, or an environmental factor) on the entirety of the living organism. Laborit developed a systemic approach of living beings that aims to study the structures at each level of an organic system and their mutual interdependence in order to highlight the dynamics of the whole system. Examining Laborit's laboratories provides a historical case study wherein a distinct philosophical perspective on living beings, shaped by cybernetics, fundamentally dictated the concrete execution of laboratory experiments. In this talk, my aim is first to present how Laborit presented life as a cybernetic process and his theoretical view of living beings that stemmed from it. Then, through an analysis of the laboratory's organizational structure, I endeavor to illustrate how his experimental protocols were crafted to align with his specific theoretical tenets regarding organisms. Laborit's work was quite unconventional for his time. Studying his way of working provides us material for thinking about how hypotheses, theoretical principles, experimental design and research outputs are connected; it is the broader dimension of this talk.

Arcila Valenzuela, Migdalia

Primary Bonds: The Role of Early Ethological Studies (1930-1940) in The Development of Attachment Theory (1958-1961)

Abstract: The purpose of this paper is to delve into the dispute between John Bowlby and the British Psychoanalytic Society (1958-1961) over the origin of "object relations" or "primary bonds", i.e., early attachment relationships between infants and caregivers. Specifically, I aim to explore how Bowlby's comprehension of human behavior is fundamentally shaped by his collaboration with ethologists such as Nikolas Tinbergen, Konrad Lorenz, Karl von Frisch, and Harry Harlow. I will present three main reasons why this collaboration played a pivotal role in the development of attachment theory and in the understanding of developmental psychology as a science. The prevailing explanation for the formation of early human bonds, according to psychoanalysis, suggested that infants became attached to their caretakers primarily due to the urge to satisfy basic physiological needs, such as food and warmth. In contrast to this perspective, the British psychiatrist, and former psychoanalyst, John Bowlby (1958) proposed that behaviors associated with early human attachment were better described as "primary drives." In essence, Bowlby's theory posited that attachment behavior comprises innate, built-in responses, diverging from the psychoanalytic viewpoint according to which those behaviors are derived from primary needs and, therefore, learned.Firstly, in contrast to the psychoanalytic approach, Bowlby conceptualizes attachment behavior in terms of "fixed action patterns." These are observable, species-specific patterns of behavior activated in response to specific environmental cues, rather than being driven solely by "instincts." Secondly, Bowlby rejects the idea that attachment behavior solely arises from learned reactions aimed at fulfilling physiological needs. However, he does acknowledge the vital role of the environment in triggering these fixed action patterns. Lastly, ethology introduced a significant methodological shift in the study of animal behavior, emphasizing the observation of animals in their natural environments. This same methodological approach shapes attachment theory and Bowlby's research on the effects of separation, which contrasts with the clinical interview-based approach by providing a comprehensive analysis of behavior within its context. Furthermore, Bowlby's adoption of ethological concepts and methodologies for the study of human behavior reflects his understanding of developmental psychology as a science that must prioritize observation, rather than biographical and introspective techniques as they were used in psychoanalytical clinical settings.

Ariotto, Andrea

Historicity of Mathematics and Historicization of Mathematical Intuition in Jean Cavaillès' Thought

Abstract: This talk addresses Jean Cavaillès' philosophy of mathematics to show the fruitfulness of his point of view as well as the consequences it implies for the conception of subjectivity within mathematical activity. Several features of what can be characterized as the French philosophy of mathematics can relate to more canonical and contemporary approaches and, in particular, to the philosophy of mathematical practice, as pointed out by Carter (2019). A major point of interest consists in the interactions between history and philosophy of mathematics, which is undoubtedly a distinctive feature of French epistemology. In Cavaillès' philosophy as well, the most significant aspect consists in the historicity that characterizes the development of mathematical objects and theories. Rejecting any cultural anthropological relativization, Cavaillès adopts an internalist approach affirming a form of historicity that is purely intrinsic to the ideal objects of mathematics. In the first part of the talk, we seek to clarify the role of two basic philosophical references underlying Cavaillès' thought. Cavaillès' formation takes place between two fundamental poles, the French neo-Kantianism held by Léon Brunschvicg, and the phenomenology of Husserl in relation to which Cavaillès seeks to define his position by opposing a philosophy of the concept to the phenomenological philosophy of the consciousness (1947b). In the second part of the talk, we focus on Cavaillès' notion of the historicity of mathematics and we examine its epistemological effectiveness. We take the text Remarques sur la formation de la théorie abstraite des ensembles (1938), and the Cantor-Dedekind correspondence as examples to follow Cavaillès' analysis of the genesis of new mathematical notions and theories. A key point of the Remarques is the first chapter, devoted to the «Prehistory of set theory», wherein Cavaillès takes into account the different domains in which basic set-theoretical notions develop. Such threshold moments in which it is possible to understand the genesis and formation of the notions that give rise to a new theory seem particularly crucial for understanding the necessity that runs through mathematical theories, unites different operative fields, and determines the progressiveness of its history. In the final part of the talk, we examine the consequences that this conception of the historicity of mathematics involves for the structure of mathematical subjectivity, and, in particular, for the notion of intuition. It seems that the consequence of the rational development which expresses the authentic conceptual nature of the history of mathematics is a historicization of the intuition. The expression «dialectic of the concept» designates the historical progressivity of mathematical knowledge, which is opposed to any form of philosophy of consciousness, but implies a correlation with what Cavaillès calls the «intuitive superposition». In this way, Cavaillès aims to establish a historical relativization of intuition that progresses in parallel with the dialectical concatenation of concepts (Cavaillès 1947a, p. 471). To account for Cavaillès' these, we take the example of Dedekind's

construction of the different sets of numbers in (1872) and (1888), where the structure proper to mathematical subjectivity is defined by the term Treppenverstand.

Asquith, Peter

Institutionalizing the Philosophy of Science Association in the 1970's,1980's and early 1990's: Challenges, Successes, and Unresolved Issues

Abstract: During the 1960's the number of individuals formally trained in philosophy of science in the United States grew substantially and the individuals in this larger group were dispersed to a greater variety of institutions and were more diverse than the earlier smaller number of individuals in the field had been. This resulted in the need for more open, public, and regular means of disseminating research results and opportunities for networking provided by a stable and responsive organization.The organization for philosophers of science in the U.S. was the Philosophy of Science Association formed in 1933. What was it already doing in 1970? The publication of Philosophy of Science had been continuous since the Association founding. In PSA's earliest years there had been joint meetings with Section L of the AAAS and in 1968 the Association had its first standalone conference at Pittsburgh. Thus, on the surface PSA appeared to be in a good stepping off place to meet the needs of the growing discipline, but what was the actual status of PSA in 1970? Although no one in PSA realized it, the journal, Philosophy of Science, was losing money. There was no publication of the proceedings of this initial conference and the conference finances depended on subsidies from the host institution. Although publication of the proceedings started with the second meeting there was no guarantee that that would happen on a regular basis. What was the status of PSA as an organization in 1970? By-Laws were first adopted in 1947, replaced by new more substantial By-Laws in 1958 and replaced yet again in 1970. However, none of these By-Laws were ones that provided PSA with any legal status other than that of a voluntary organization. While there was a need to sponsor or undertake additional activities, how could that be done under these circumstances? What I want to do in this lecture is examine the issues that PSA had to deal with and how they were handled over the next twenty-five years – 1970 to 1995. Among the issues are: (1) To what extent and in what way was financial stability achieved; (2) What was done to achieve a stable and legally recognized organizational status; (3) What new activities were undertaken; (4) How were the achievements accomplished during this period affected when the PSA office moved from Michigan State University?Some relevant documentation is readily accessible, e.g., By-Laws, some is public, but not readily available, e.g., PSA Newsletters and election mailings, and some is not yet publicly available. As someone who entered the profession in 1970, was a regular participant in PSA activities, and was a collector of the materials distributed by PSA, my account not only utilizes documentation generally publicly available, but also items in my possession including correspondence and notes from meetings and other interactions with other individuals active in PSA during that period.

Babeș, Ovidiu

Early eighteenth-century anti-mathematicism: the case of Robert Greene

Abstract: This contribution foregrounds a hitherto neglected anti-mathematical natural philosophy developed in Cambridge at the beginning of the eighteenth century. Robert Greene (1678?-1730) opposes 'Corpuscular Philosophy' and the 'Homogeneity of Matter,' which he takes to be the two faulty principles grounding the mathematical physics of his day. His philosophy rejects the independent existence of absolute space, along with the most common views on the essence of material objects. In his 1712 and 1727 treatises, Greene denies both the existence of vacuum in nature as well as the Cartesian plenum, as they both rest on improper abstract concepts of matter and its qualities. I argue that Greene has a distinct strategy of undermining mathematics as a ground for natural philosophy, which should be contrasted to other anti-mathematical trends in eighteenth-century philosophy.The label 'anti-mathematicism' (cf. Domski 2012; Schliesser 2017, 2019; Ducheyne 2019; Petereman 2019; Wolfe 2019 and others) stands for the early modern philosophical belief that mathematics cannot provide an adequate basis for natural philosophy. The belief stems from various epistemological and metaphysical worries about the status and role of mathematics within natural science. Some concern the applicability of mathematics to nature: Many natural phenomena cannot be adequately represented in a quantitative fashion. Others pertain to the abstract character of mathematical objects, as well as to several epistemic vices associated with abusing mathematical techniques in disciplines such as medicine. The upshot of these worries is that mathematics should be limited in scope-for instance, to only account for the motions of the planets; and that the precision of mathematical results in natural science should, at best, be taken with a grain of salt.This talk highlights the fact that Greene's metaphysical anti-mathematicism goes further. He objects to the abstract character of the fundamental objects of natural philosophy: matter, motion, and their inherent properties. Greene argues that our concept of space is abstracted from our tactile and visual perceptions of distance. In turn, space as an absolute, immovable, and independent entity is just an abstract idea to which our minds have become accustomed. This has two consequences. First, vacuum cannot exist in nature, because it presupposes the existence of absolute space. Second, there is no Cartesian plenum, as this would wrongly imply that matter is homogenous and uniform in all its parts. Instead, Greene proposes 'expansive' and 'contractive' forces as the principles of material reality. He erects a philosophy which aims to explain all natural phenomena based on these two principles. By zooming into Greene's sophisticated rejection of absolute space and corpuscular philosophy, the label of 'anti-mathematicism' will be further articulated and refined. Aside from Greene's philosophical stance, his anti-mathematicism is insightful precisely because it was born in 1710s Cambridge, where Newtonian mathematical physics

was establishing itself as the norm by the help of people like William Whiston, Samuel Clarke, Richard Bentley, or Richard Laughton.

Baccarat Silva Negrao De Campos, Lucas

Different kinds of Naturalism: Continuities and discontinuities between Otto Neurath and Richard Avenarius

Abstract: Richard Avenarius was a very important figure in the late 19th century debates about the method and object of scientific psychology, and how such discipline should relate to philosophy. In particular, his mature works, Der Kritik der reinen Erfahrung and Der menschliche Weltbegriff, were very impactful at the time, promoting reactions from the likes of Husserl and Rickert and getting appreciation in both philosophical and political circles. The Vienna Circle did not pass unscattered to his work. Schlick, Carnap and Neurath, all quote him as a relevant source of ideas and as a model for scientific philosophy. Neurath, in particular, not only hints at Avenarius´ relevance as a philosophical predecessor, but also makes use of avenarusian notions in several of his writings as part of his argumentation. In my talk, I will claim that Avenarius ńaturalistic approach to knowledge, as well as his insights on the mind-body relation and conception of experience, provide an extremely relevant philosophical background for Neurath´s thinking, something that, to this day, is not yet recognized. However, where both authors come together is just as interesting as where they part ways with one another. Therefore, I argue that Neurath´s had a mitigated reception of Avenarius´ work, in that, whilst there are many continuities among their naturalistic attitude, critique of metaphysics and meta-theory of science, there are important epistemological discontinuities between them both. As Neurath himself stated, in his Sociology in the Framework of Physicalism (1931), there were remarkable residues of out-of-date ideas in some of Avenarius´ core epistemological arguments. In describing his biologically and brain centered theory of knowledge, Avenarius still held on to a kind of teleological view of evolution that was later rejected by Neurath. The former Vienna Circle member would then extend the scope of naturalist epistemology, including the social sciences in it, so that he could account for some of the shortcomings in his predecessors.

Baedke, Jan

Towards philosophy of biology through plants: Emil Ungerer's research on plant physiology and teleology in early 20th century

Abstract: This paper discusses debates about teleology and agency in plants in early 20th-century German-speaking theoretical biology and philosophy of biology. This especially concerns neo-Kantian and holistic positions in early 'Theoretische Biologie' on whether or not plants' metabolism and behavior show purposiveness. By using digital text analysis tools, I will first trace the legacy of Kantian reasoning about organismal purposiveness in influential monographies and book series in theoretical biology in the time. Second, I investigate the 'plant holism' of botanist and philosopher of biology Emil Ungerer (1888-1976). Ungerer, a pupil of Hans Driesch, widely published on teleology in plants. He tried to replace (neo-)Kantian understandings of purposiveness by a view of teleology as the maintenance of a dynamic equilibrium and organization ('Ganzheit'/wholeness) of living systems. He focused on how, in cases of disturbance, plants can regenerate themselves and thus reestablish their internal organization in a goal-directed manner. For this holistic view of plant agency, Ungerer argued, we do not need to introduce Aristotelian 'final causes', nor vitalism (against, e.g., Driesch). Finally, this paper contextualizes Ungerer's holism ('Ganzheitsbiologie') within Nazi biology and discusses reasons for the failure of this philosophy of biology after WWII.

Baldassarri, Fabrizio

Plantanatomia Democritea: 17th-Century Democritean Study of Plants in Context

Abstract: Late seventeenth century "anatomies" of plants revolutionized botanical studies. They propose new ways of understanding vegetable life. And yet, interestingly, some of them claimed to revive a Democritean interpretation of nature. In this talk, I would like to shed new light on the works of Marco Aurelio Severino (1580-1656), Henri Power (1623-1688ù9 and Pierre Borel (1620-1671). While these authors belong to different backgrounds, a common methodology surfaces. Severino was a Calabrian physician and lawyer, well-acquainted with Tommaso Campanella, William Harvey and Thomas Bartholin, he worked in Naples and authored the Zootomia Democritea (1645), where he proposed comparative anatomy. Power was an English physician and experimenter (and fellow of the Royal Society) who performed several microscopic and corpuscular observations, combining Cartesian philosophy with a Democritean interpretation. Borel was a French chemist, physician and botanist, and was also a biographer of Descartes; in his work, Borel performed several observations with microscopes, especially detailing about plant life. All these authors propose interpretations of vegetation in line with forms of atomism ascribed to Democritus, a common link between them. In my talk, I present these cases and contextualize these authors, and then I discuss their particular interpretations of nature in general and plants in particular, focusing on some of their observations of plants and their efforts to understand vegetable life by means of microscopic observations.

Barés Gómez, Cristina
Matthieu, Fontaine

An analysis of Akkadian Diagnosis

Abstract: When we talk about Akkadian Medicine we refer to a corpus based upon the medical practice of the Ancient Near East [2,3,5,6]. This practice has a clear structure, which corresponds mostly with the modern medical diagnosis based on causal reasoning. Diagnosis is the art/science of recognizing possible diseases from their signs/symptoms. This possible illness allows to make a prognosis and a treatment. The texts we have at our disposal are usually divided into Diagnosis texts and Therapeutic texts. Nevertheless, we do not have a clearly defined structure in all the texts. Moreover, these texts sometimes use transversal structures. In this work, we will analyze the inference schema at stake in the medical reasoning by using examples mainly from the Diagnostic Handbook [2,3,5,6]. The inferential structure is an abduction [1,4,7] which standard definition was given by Peirce [4] [CP 5.189]:The surprising fact, C, is observed;But if A were true, C would be a matter of course,Hence, there is reason to suspect that A is true.Its main point is the hypothetical aspect of the possible illness and the uncertainty of the conclusion, while the prognosis leads us to a reaction with the treatment. Our point is to try to understand Akkadian medical reasoning from the texts we have at our disposal and their conditional structure. In order to understand medical reasoning, we have to analyze it as a whole and books as the Diagnostic Handbook are important sources which not only give us some texts, but also their organizational structure. For that, we will analyze the inferential structure in different Tablets. Concretely, we will use some cases of the Aḫḫazu in the SA. GIG. I will also mention BAM 578 to compare the structure. Finally, we will try to understand Akkadian medical diagnosis in the context of general medical methodology in the History of medicine. To have a complete view of medical science in Ancient Near East, we think we need to see the medical reasoning from a wider perspective, and the philosophical and logical analysis can help to see the structure of the medical reasoning. These texts reflect an inferential structure proper of an ancient scientific medicine that could be found in a similar way in medicines as Greek medicine or Egyptian medicine.Selection of References[1] XX. "Abduction in Akkadian Medical Diagnosis". Journal of Applied Logics - IfCoLog. Volume 5, Number 8. pp. 1697-1722. 2018.[2] N. Heeßel. Babylonisch-assyrische Diagnostik. Alter Orient und Altes Testament, Band 3, Münster, 2000.[3] R. Labat. Traité akkadien de diagnostics et pronostics médicaux. Paris/Leiden, 1951.[4] C.S. Peirce. Collected Papers of Charles Sanders Peirce. Harvard University Press, Cambridge, 1931-1958.[5] J. A. Scurlock. Sourcebook for Ancient Mesopotamian Medicine. SBL Press, 2014.[6] E. Schmidtchen. Mesopotamische Diagnostik. Untersuchungen zu Rekonstruktion, Terminologie und Systematik des babylonisch-assyrischen Diagnosehandbuches und eine Neubearbeitung der Tafeln 3–14. De Gruyter 2021.[7] J. Woods. Errors of Reasoning. Naturalizing the Logic of Inference. College Publications, London, 2013.

Beck, Max

Philosophizing in fluent “broken English”. Carnap and Neurath on language in exile

Abstract: Rudolf Carnap and Otto Neurath were both forced to leave Europe to escape Austrofascism and National Socialism. Neurath emigrated to the UK via the Netherlands, and Carnap to the United States. Several other representatives of the Vienna Circle emigrated in the same way. Thus, the impact of the Vienna Circle shifted to the English-speaking world. English was not an unknown language for both Neurath and Carnap. Since the 1920s, the Vienna Circle had academic contacts in the English-speaking world, which was more open to the ideas of logical empiricism. Efforts by Carnap to acquire the new language through language courses, for example, have been documented long before exile. Nevertheless, Carnap and Neurath had difficulties in acquiring the new language fluently, as can be studied in various letters.The paper aims at reconstructing Neurath's and Carnap's relationship to the English language. First, it will be traced biographically how Carnap and Neurath encountered the English language and how English was discussed in letters, diaries, etc. during and before the emigration period. Second, the paper reconstructs the respective arguments used by Carnap and Neurath to argue that philosophy can be better expressed in English than in German. It will be worked out why the philosophical understanding of language of the Vienna Circle fits better to the English than to the German language. This makes it understandable why the representatives of the Vienna Circle had fewer problems with the transfer of their philosophical ideas into the English-speaking world than representatives of other philosophical schools – despite claims like Neurath's that he speaks "broken English fluently". The lecture is based on extensive analyses of Carnap's and Neurath's estates, for example, Neurath's letters from 1933 to 1945, which are stored at the Austrian National Library in Vienna.

Beck, Pieter

Revisiting the Chemical Revolution (Again): Good Reasons and Real Questions

Abstract: In this paper, we revisit a recent debate between Martin Kusch, Ursula Klein, and Hasok Chang on the Chemical Revolution (CR). Chang had argued that there were no decisive scientific reasons at the time for abandoning the phlogiston theory. In line with his more general call for pluralism in science, he concludes that the theory should therefore have been allowed to live. Kusch and Klein criticize Chang's work on different levels. We will use the debate as a way to tackle some outstanding historiographical and philosophical questions regarding the Chemical Revolution. We start from the disagreement between Kusch and Chang, which centers around the question of whether or not there were good reasons for abandoning the phlogiston theory in favor of Lavoisier's oxygen theory. According to Kusch, Chang has a too narrow view on what constitutes good reasons. If he had taken into account insights from the sociology of scientific knowledge (SSK), he would have seen that at the end of the eighteenth century there were indeed good reasons (broadly construed) to place one's trust in Lavoisier and his program. In his work, Chang had explicitly rejected social explanations. Kusch finds it difficult to see why Chang is so adamant in his rejection, and argues that he is outdated in equating the "social" with "irrational" or "external(ist)" and what is "rational" with "internal(ist)". In his reply, Chang reiterates his refusal to adopt the kind of SSK framework favored by Kusch, because it cannot take into account the judgment or dissent of individuals. He does not oppose sociology per se, but it should be a sociology which allows for a dialectic between the collective level and the individual level. We argue that Chang's framework of "systems of practice" has the potential of providing such a sociology, but then it needs to take into account some of the (perceived) "external" factors that Chang seems to avoid. To develop this argument, we look at the interaction between Klein and Chang. Klein uses Kuhn's later notion of a scientific revolution in terms of changes of taxonomies to argue that the CR was not a revolution. In his reply, Chang argues that the concept of a scientific revolution as developed in The Structure of Scientific Revolutions is more fruitful, and that there was indeed methodological incommensurability between the phlogiston and oxygen paradigm. We think Chang is right in preferring the early Kuhnian notion of a scientific revolution, but that he misses some opportunities for developing this point more forcefully in his favor. Chang's own analyses have drawn attention to the dynamical relation between questions and methodological change, and we propose to put this at the center of attention, along the lines of Nicholas Jardine's "scenes of inquiry". This allows us to show that there were indeed good reasons to prefer Lavoisier's oxygen theory, without thereby succumbing to the triumphalist historiography or the reductionist sociology that Chang wants to avoid.

Bellomo, Anna
Georg, Schiemer

Instrumental formalism - From Hilbert's program to Carnap's Wissenschaftslogik

Abstract: Formalism, as first introduced in the philosophy of mathematics, is the view that mathematics is purely syntactic in character and that semantic concepts are not relevant or reducible to purely syntactic ones. The focus in this talk will be on a particular version of formalism, namely instrumental formalism with its emphasis on non-semantic or "non-representational" roles of symbolic languages in mathematical reasoning. David Hilbert's foundational work of the 1920s is usually viewed as a culmination point in the early development of such a formalist position (cf. Detlefsen 2005). Interestingly, both in Hilbert's work as well as in related contributions to instrumental formalism, one can identify several relevant criteria of reliability for the use of formal languages or theories, including the consistency and the conservativity of sets of rules or axiom systems. Roughly put, the latter condition requires that a formal theory presents a conservative extension of an interpreted base theory. In Hilbert's own logical work, this central but implicit assumption has been described as his "conservativity program" (cf. Zach 2004). The focus of this talk will be on Hilbert's instrumental formalism underlying his proof-theoretic program as well as its more general philosophical impact. The aim here will be twofold. The first task will be to give an exposition of the development of the conservativity program in Hilbert's logical work from the second half of the 1920s until the introduction of the so-called epsilon-theorems in second volume of Hilbert's & Bernays' Grundlagen der Mathematik of 1939. A closer study of Hilbert's and Bernays' discussion of these central conservativity results regarding the logical epsilon calculus will allow us to give a more precise understanding of the instrumental formalism underlying their foundational program. The second aim in the talk will be to investigate the more general "intellectual context" of Hilbert's program (cf. Giaquinto 1983, Hallett 1990). More specifically, it will be to discuss how his results on the foundations of mathematics relate to forms of scientific formalism as well as to early contributions to the logic of science ("Wissenschaftslogik") in logical empiricism. In the talk, I will focus on several points of contact between Hilbert's logical work and Rudolf Carnap's early contributions to the logical reconstruction of mathematical and scientific theories from the late 1920s until the publication of Carnap's monograph Foundations of Logic and Mathematics in 1939. The philosophical comparison between Hilbert and Carnap will focus on two interpretative points: first, the extent to which Carnap's own project on "general axiomatics" from the late 1920s with its focus on metatheoretical properties of theories was motivated by Hilbert's foundational work. Second, the talk will discuss whether Carnap's later views on theoretical languages, in particular on the status of "theoretical" or "abstract" terms in science, were directly influenced by Hilbert's instrumental formalism in the philosophy of mathematics.

Bellomo, Anna

Permanence of forms: a formalist principle?

Abstract: Although formalism as a philosophy of mathematics and its foundations is a development of the early 20th century, formalist attitudes towards mathematics – algebra and analysis in particular – can be detected already in the 18th and 19th centuries. One notable example in that regard is the British algebraist George Peacock (1791-1858), usually credited with the first explicit formulation of a principle, called the Principle of Permanence of Equivalent Forms (PPEF), clarifying the relationship between arithmetic and algebra. Roughly, the principle states that the equations of arithmetic that one can express in algebraic notation (meaning using the symbols of the arithmetical operations and then letters instead of numbers) are to be considered true also in algebra, that is, also when the letters are taken to denote "any quantity" and not simply natural or integer numbers. Peacock's overall philosophy of algebra thus is as follows: algebra is what one obtains by regarding some arithmetical equations as regulating how we use certain symbols (specifically, +, -, ×, /). In addition, Peacock contends that not all variables in an algebraic expression have to refer to an existing quantity. In other words, Peacock regards algebraic symbolism as a language that can be used instrumentally, as well as referentially. Peacock's PPEF is approvingly mentioned in writings of Hilbert and Bernays, who most likely accessed it through the work of Hermann Hankel (Hankel 1866). If one looks however at the applications of the principle in Hankel's work and compares them with those in Peacock's, one can notice that there is a difference: Peacock uses the principle to generalise specific theorems regarding e.g. exponentiation or series expansion. Hankel, by contrast, uses the Principle of Permanence of formal Laws to define how addition, multiplication and their respective inverses (subtraction, division) are to behave once we expand the number domain beyond the natural numbers. Through a comparison of the overall project of Peacock's textbook and that of Hankel's, as well as their respective applications of the Principle, I will argue that, despite the commonalities between Peacock and an instrumentalist or formalist attitude towards algebra, the Principle of Permanence as he used it is not quite the precursor to a formalist understanding of number systems. That only starts with Hankel. I will suggest that Peacock's Principle is better understood as a methodological principle used to legitimise the practices of the so-called "formula-centred" (Sørensen 2005) approach to analysis that was typical of the late 18th century, and that in some way was still dominant in the French analysts whose approach Peacock was emulating and trying to promote at Cambridge University. This framing of the principle explains both the applications Peacock makes of it and the limited range of generalisation it can be subjected to.

Bentley, Joseph

Positivist or Post-Positivist Philosophy of Science? The Left-Vienna Circle and Thomas Kuhn

Abstract: It was once common to frame Kuhn as the killer of Logical Empiricism. He invokes the specter of the Logical Empiricists as the orthodoxy which his new historical approach to philosophy of science is a response to and rejection of. And it is true that Kuhn's historical methodology stands in marked contrast to some of the more technically-minded and formalistic examples of Logical Empiricist philosophy of science in the post-War anglophone world. However, this version of Logical Empiricism is not characteristic of the entire movement. More recently, this narrative has been challenged, and it has been argued that there is in fact important continuities between the work of Kuhn and the Logical Empiricists.The resulting debate between the so-called "revisionist" and "received" views of the relationship between Kuhn and Logical Empiricism (arguing for continuity and incompatibility respectively) has until now focused almost entirely on the relationship between Kuhn and Rudolf Carnap. But Logical Empiricism was not monolithic, and insight is to be gained on the movement by looking beyond its most famous representative. Here, I consider the relationship between Kuhn's thought and that of two other members of the Vienna Circle's left-wing; Otto Neurath and Philipp Frank. In so doing, it is argued that the attribution of the historical turn in philosophy of science to Kuhn obscures the historical methodology and orientation of much of the Vienna Circle's own work, and therefore distorts the legacy of the Vienna Circle and the history of twentieth century philosophy of science.I demonstrate that two of Kuhn's (seemingly) most innovative and influential contributions to the practice of philosophy of science, the "role for history" and a new "image of science", are in fact already present in the work of Neurath and Frank. It is then argued that the Left-Vienna Circle's programme for Unified Science, the replacement of traditional armchair philosophy with a bipartite metatheory of science, provides a clearer and potentially more radical role for the history of science within the philosophy of science than Kuhn's. To reach this conclusion, it is argued that members of the Vienna Circle's left-wing must be recognised as maintaining a far less sharp and robust conception of the distinction between contexts of discovery and justification than has typically been attributed to them. It is argued that without this misreading of a sharp context-distinction, there is not the profound disagreement between Kuhn and the Logical Empiricists that has so commonly been assumed.These revisions not only add important nuances to the story of twentieth century philosophy of science, but they establish the potential for fruitful dialogue between two philosophies of science that have frequently been treated as incommensurable.

Bertran-San-Millán, Joan

Frege's early notion of function

Abstract: Tappenden (1995) and Wilson (1992) describe the rich mathematical and historical setting of Frege's Grundlagen der Arithmetik (1884). They point to the connections between Plücker and Clebsch's understanding of functions – in the context of the duality principle in projective geometry – and Frege's functional approach.However, I think more should be said about Frege's early conception of function, fully developed by Frege in Begriffsschrift (1879). In this talk, I first provide new textual evidence to Tappenden and Wilson's claim that substantial sources of influence on Frege's early notion of function can be found in Clebsch and Plücker's works. I then argue that the concept of function developed in Begriffsschrift is instrumental in Frege's early mathematical project; shapes the syntax, quantification and calculus of the logical system; and should be distinguished from Frege's later notion of function.In the late 1870s, Frege was engaged with duality principles in projective geometry. Specifically, in the transcripts of some of the lectures delivered by Frege at the University of Jena in 1875–1876 (see (Frege 1969, 365–374)), Frege exemplifies the kind of analysis of functional expressions that allows a dual interpretation in Plücker (1868) and Clebsch's (1876) works. The flexibility by means of which Frege conceives the application of a decomposition of an expression in terms of function and argument in Begriffsschrift (1879, §10) bears striking resemblance to Clebsch's account.The logical system of Begriffsschrift, the concept-script, is shaped in terms of the distinction between function and argument. It is not uncommon in historical studies to defend that Begriffsschrift's function should be semantically conceived and thus reflects an ontology (see (Heck; May, 2013)). In contrast, I shall argue that the function-argument scheme in Begriffsschrift is independent of any ontology. Two lines of reasoning support my claim. First, I argue that the concept-script is meant to be applied to specific contexts – including logic – which provide their own ontology, and only in those contexts some limitations to the generality expressed by the letters can be given. Second, I claim that the nature of the language of the concept-script makes it impossible to determine the domain of interpretation of the letters. From an analysis of Frege's derivations I conclude that since letters are only interpreted in the context of a proof, they cannot have a pre-determined domain of interpretation.All in all, I defend that Frege's early notion of function relies, to a great extent, on Frege's mathematical work from the late 1870s, and reflects the influence of relevant contributions to projective geometry of that time. These precedents can be pinpointed in Begriffsschrift. The purely syntactical use of the function-argument scheme in the proofs of Frege's 1879 work shows that the notion of function at play is substantially different to Frege's mature conception, developed from (1891) on.

Berwe, Theodor
Jens, Lemanski

The Geometrical Method in the Weigel School

Abstract: The ideal of the geometrical method is ubiquitous in the methodological considerations of early modern science. The claim is to conduct science according to the model of geometry, i.e., more geometrico. However, the ideas as to what this geometrical method actually consists of are quite disparate.In our paper, we take a closer look at a variant of the mos geometricus that has received relatively little attention in research so far: the Weigel School, which was centered around Erhard Weigel in Jena, Germany, in the 17th century. Besides Weigel himself, especially his student Johann Christoph Sturm played a significant role in spreading the core ideas of this school. Leibniz, too, studied under Weigel in Jena and was familiar with Sturm's work.In our paper, we follow two main goals:Firstly, we examine two specific aspects employed by the Weigel School, which distinguish this version of mos geometricus from others: (1) In sharp contrast to, say, the Cartesian tradition, the Weigel School does not exclude syllogistics from its methodology. It views Aristotelian logic as an early, albeit insufficient, sample of the geometrical method, which it considers to be a universal formal method. Against this background, the Weigel school undertakes a re-foundation of syllogistics. (2) The Weigel School advocates the use of logic diagrams for representing logical reasoning. Abstract logical relationships can be represented through concrete diagrams.Both of these aspects can be exemplified by Sturm's Universalia Euclidea (1661). As the title suggests, this work is a generalization of Euclid's Elements, more precisely, the fifth book on proportions of magnitude, into a general theory of proportions that is meant to function as a mathesis generalis. The inference rules contained therein also form the basis for syllogistics, to which Sturm adds 12 new modes. In the appendix to his Universalia Euclidea, Sturm furthermore employs circular diagrams to demonstrate new valid forms of syllogisms. There is strong evidence that Weigel was the actual originator of the diagrammatic representation of logical reasoning despite the fact that logic diagrams are first found in Weigel's published writings in 1693, three decades after Sturm's publication. The second aim of our paper is to trace the influence of the Weigel School, which extends to Leibniz and beyond well into the 18th century. Contrary to the 18th-century portrayal of Leibniz as a staunch opponent of diagrams in geometry, remarkable methodological similarities between the Weigel circle and Leibniz's own position can be identified, particularly in terms of the possibility of representing abstract logical relations through concrete diagrams.

Blåsjö, Viktor

Descartes's geometrical constructions in comparative historical context

Abstract: Descartes tied the foundations of geometry to the tracing of curves by motion, but left many details in an ambivalent state. He gave a paradigmatic construction of the hyperbola, but strangely referred to Apollonius instead of giving his own constructions for the other conics, and he half-heartedly accepted pointwise and string constructions in some cases while banning them in others.Through a systematic comparative analysis of 17th-century methods for tracing conic sections, I offer a new contextual perspective on these questions. Many aspects of the Cartesian program are in accord with a broader consensus: mechanical linkage constructions were pursued systematically already before Descartes (e.g. Cavalieri and Oddi), and the theoretical superiority of instruments over pointwise constructions is acknowledged in remarkably consistent terms by many authors, such as Eutocius, Mydorge, La Hire, and l'Hôpital. Descartes's peculiar reliance on Apollonius's construction propositions in the Géométrie, and his brief reference to the conic compass in the Cogitationes privatae, can be better understood in light of how such constructions were treated and interpreted in the conics treatises of Cavalieri, Viviani, and Milnes.Descartes's followers, such as Van Schooten, De Witt, and l'Hôpital, are sparse with explicit methodological remarks, but their technical choices when developing Descartes's sketched program say a lot about what they considered to be its core principles. They did little or nothing to build on Descartes's endorsement of string and pointwise constructions, suggesting that they silently acknowledged the counterarguments voiced by people such as Huygens and Newton on these points. On the other hand, they all incorporated Descartes's turning-ruler hyperbola construction, suggesting that they regarded it as a prototype success case of the Cartesian approach. And how they devised analog constructions for the other conics suggests what they saw as its key attributes - arguably a desire to minimise non-local constraints on instrument motions, perhaps for the sake of minimising the risk of inconsistency when multiple conditions are imposed.While Newton's tirades against Descartes are well known in general terms, their technical culmination in a full-fledged constructivist program of Newton's own has been all but ignored in the literature. In manuscripts from the 1690s, Newton goes beyond flippant anti-Cartesian outbursts and crafts a credible alternative set of construction principles that seems designed to be technically superior to the Cartesian approach even by Descartes's own methodological standards. Here Newton, like many other 17th-century mathematicians, shows himself to be a more sensitive and philosophically sophisticated interpreter of ancient and early modern constructivist tradition than one one would know from his directly philosophical passages.

Bodner, Moritz

From mathematical practice to foundational reflection: The Case of Hilbert

Abstract: I aim to highlight connections between Hilbert's early algebraic and his later foundational work.Hilbert first earned a reputation as a mathematician with work in algebra, causing a stir with an indirect, non-constructive proof of what has become known as the Hilbert Base Theorem. Although Hilbert soon shifted his attention to other fields, he still recalled his early proof and the controversy it gave rise to in later work, and does so, in particular, when discussing foundational issues.For example, in his well-known address "Axiomatisches Denken", Hilbert recalls his non-constructive proof when discussing decidability, citing it as an example to show that settling a problem in principle, and doing so by a finite process, are separate mathematical issues to be addressed by different kinds of solutions. In "Die logischen Grundlagen der Mathematik", Hilbert refers to his early proof as an example of a piece of mathematics involving "transfinite modes of inference". In fact, Hilbert's published work gives only a partial picture: Both recently published lecture-notes on foundational topics, as well as pertinent manuscripts that remain unpublished in Hilbert's Nachlass, contain further references to Hilbert's early algebraic work: So one finds Hilbert listing the "Invariantenkalkül" as a paradigm example for a "formalised" calculus akin to the formalisation of logic Hilbert himself started developing in 1917/18 (in notes recording a lecture-course from 1921/22, Hilbert 2013).These citations show that Hilbert's early algebraic work later served Hilbert as a key benchmark for the views on foundational issues such as decidibility or finitism Hilbert put forward later.Some of these citations (e.g. that in "Axiomatisches Denken") suggest that Hilbert developed certain foundational views in order to retroactively justify his controversial algebraic work; others, that Hilbert merely found in his early work a useful illustration for certain distinctions (e.g. between solvability in principle as opposed to solvability by means only of "finitary" methods) without maintaining an active interest in having his own early work fall on the "right" side of these distinctions. In any case, that it kept coming to his mind at all is remarkable as it indicates that Hilbert saw connections between his early mathematical work and his later foundational thought and saw continuities between them. What Hilbert saw and what he made of it has barely been studied (with the exception of Abrusci 1981).Hilbert's references to algebra in the context of foundational discussion also reveals more clearly the contours of Hilbert's own position and, in particular, Hilbert's attitude towards formalism: In Hilbert's "Wissenschaftliches Tagebuch" (i.e. three volumes of notes on various topics dating from between 1885 and 1912) Hilbert contrasts his own approach with that of earlier algebraists (such as Clebsch, Jacobi or Kummer), whose work he faults for its "rabid formalism" (vol. I, p. 65); other unpublished documents likewise show Hilbert criticising the formalism espoused by German mathematicians in the late 19th century.

Boes, Gregor

What happened to ‘Rigorous Science’ in Phenomenology? A quantitative study

Abstract: Husserl was keen to present phenomenology as a method that finally transforms philosophy into a rigorous science. This scientific philosophy, so Husserl's late Crisis promises, would even transform our understanding of the empirical sciences, by reconnecting them to what has meaning in our lives. It has become a point of contention, whether Husserl really conceded at the end of his life, that this dream of scientific philosophy was over ('ausgeträumt'). But if not for Husserl, this dream seemed over for the next generation of phenomenologists. Husserl's project of founding the sciences in phenomenological philosophy was replaced by an existential-hermeneutic inquiry with Heidegger as its new champion.My paper first sketches reasons that for this shift. There are on the one hand, philosophical questions, such as the reservations many phenomenologists held about Husserl's idealism, but also problems in Husserl's treatment of indexical meanings. On the other hand, the rise of Nazism was less of a threat to the party member Heidegger, than for Husserl, who was born into a Jewish family.Such reasons for influence are difficult to disentangle. But philosophical reasons persist, political reasons change. If the transition from Husserl's to Heidegger's version of phenomenology was based on philosophical reasons, it should have started already around 1913, when Husserl's transcendental idealism became apparent in the Ideas I, and it should remain relatively stable after World War II. A shift back to discussing phenomenology as the foundation of the empirical and formal sciences, on the other hand, suggests a temporary suppression of this style of phenomenology for political reasons. To answer this empirical question, I process the titles and abstracts of philosophy articles from 1900 to 1980 that are available through JSTOR. The variables under consideration are the proportions among all phenomenology articles that discuss 1. formal and empirical sciences, 2. the hermeneutic method and the human condition, 3. both.

Boncompagni, Anna

Russell, Ramsey and Wittgenstein on belief

Abstract: In the late 1920s several philosophers were interested in the nature of belief. This paper starts by examining the views expressed by Bertrand Russell and Frank Ramsey in that period. Next, I expand on the "middle" Wittgenstein's take in the early 1930s, partly influenced by Ramsey. Finally, I examine Wittgenstein's later critical remarks on belief as "an adjustment of the organism". The goal is both exegetical and theoretical: on the one hand, I show some hitherto neglected continuities between Russell, Ramsey and Wittgenstein, and on the other hand I clarify the distinction between an empirical/scientific and a conceptual/philosophical investigation that the later Wittgenstein insisted on.My starting point is Ramsey's acknowledgment of Russell as the inspirer of his own pragmatism in "Facts and Propositions" (1927). In this paper Ramsey explained that at least in the case of animal belief, the pragmatist view was correct in establishing a strict connection between belief, its "objective factors", and behavior (in his famous example of the chicken: the belief that the caterpillar is poisonous; the caterpillar being effectively poisonous; and abstaining from eating it): "any set of actions for whose utility p is a necessary and sufficient condition might be called a belief that p, and so would be true if p, i.e. if they are useful". He also claimed that the importance of belief lies in its causal properties; and that what he was mostly interested in, however, was "belief expressed in words". All three aspects – the connection between belief and behavior, the causal role of belief, and the need to investigate "belief expressed in words" – are also highlighted by Russell in the second half of the 1920s, particularly in An Outline of Philosophy and his Encyclopedia Britannica entry titled "Theory of Knowledge". These and other writings show the influence of the pragmatist tradition, especially James and Dewey, in this period of Russell's work. Going back to Ramsey, besides praising Russell's pragmatism he also pointed out that the pragmatic element was missing and needed in Wittgenstein's Tractatus. My next step is seeing some Wittgensteinian reflections in the early 1930s as indebted to Ramsey's criticism and pointing towards a pragmatist direction, also interlacing with topics discussed in the Vienna Circle (e.g., verification, hypotheses). However, a remark from Wittgenstein's later period will show the distance that he increasingly put between his views and a pragmatist-naturalist approach, as well as the scientific-oriented Vienna Circle discussions. During his lectures on philosophical psychology (1947), he commented on the idea that belief is "an adjustment of the organism", or "an attitude of the organism to facts", attributing it to either Russell or Dewey (the notes taken by different students are not in agreement on that). He observed that this claim shows some confusion between the experiential or empirical and the conceptual. I argue that this is an instance of Wittgenstein's distinction between a scientific and a philosophical reflection, which ultimately kept him apart from both pragmatism and logical empiricism.

Borge, Bruno

Lakatos on the Metaphysics and Epistemology of Laws of Nature

Abstract: There is a well-known tension between fallibilism and epistemological optimism in Lakatos's philosophy. On the one hand, the Methodology of Scientific Research Programs presupposes a strong fallibilism about scientific knowledge. On the other hand, Lakatos's work supports the conviction that the growth of knowledge has to be somehow related to the increase of verisimilitude (Cf. Lakatos 1978: 113-114, 122, 156). Several authors have identified this tension (e.g. Newton-Smith 1981; Larvor 1998). Despite their differences, they share the belief that Lakatos's epistemological optimism falls through. In this talk, I delve into Lakatos's perspective on the metaphysics and epistemology of laws of nature to challenge that belief. I contend that Lakatos's view on laws serves as the linchpin for reconciling the tension between fallibilism and epistemological optimism. For this purpose, I rely on the work Necessity, Kneale and Popper (Lakatos 1978 [1960]), the unpublished manuscript God, Angels, and the Triviality of Truth (Lakatos n.d.-a), and other archival material from the Imre Lakatos Collection at the Archives Division of the London School of Economics (e.g. Lakatos n.d.-b). In these works and notes, Lakatos vindicates a form of nomological realism that combines epistemic realism with fallibilism. The cornerstone of his view on laws is the idea that since the universe is infinitely varied, only universal statements of infinite length (i.e. lawlike statements with infinite qualifications) can be true. Then, (finite) lawlike statements are always false. The primary aim of this talk is to formulate an interpretation that ascribes to Lakatos a form of Moderate Nomological Realism (MNR). MNR provides an account of the incomplete and fallible nature of our understanding of natural laws while also preserving the idea that the growth of knowledge aligns with a closer approximation to truth. MNR is intended to be an organic reading of Lakatos' ideas, however it is also meant to be in dialogue with contemporary theories about the metaphysics and epistemology of laws of nature. For this reason, I introduce two interpretative tools that are foreign to the Lakatosian framework. First, I use Hempel's (1988) theory of provisos to clarify the notions of "universal statements of infinite length" and "statements with infinite qualifications". Second, I make use of Yablo's (2013) notions of partial content and partial truth.ReferencesHempel, C. G. (1988). Provisoes: A problem concerning the inferential function of scientific theories. Erkenntnis, 28(2), 147-164.Larvor, B. (1998). Lakatos: An Introduction. Routledge.Lakatos, I. (n.d.-a). God, Angels, and the Triviality of Truth [Unpublished manuscript]. Imre Lakatos Collection (Box 5, Folder 2), London School of Economics and Political Science, London, United Kingdom.Lakatos, I. (n.d.-b). Untitled [Pocket file of notes relating to Necessity, Kneale and Popper]. Imre Lakatos Collection (Box 5, Folder 2), London School of Economics and Political Science, London, United Kingdom.Lakatos, I. (1978). The methodology of scientific research programmes: Philosophical papers. Vol. I (J. Worrall & G. Currie, Eds.). Cambridge: Cambridge University Press.Newton-Smith, W. H. (1981). The rationality of science. Routledge.Yablo, S. (2013) Aboutness. Princeton, NJ: Princeton University Press.

Brooks, Daniel

The Problem of Organization in Biology and Its Reception in Mainstream Philosophy of Science, 1920-1961

Abstract: In the early to mid-20th century, English-speaking organicist biologists strongly advocated the idea of organization in biological phenomena as a means of elevating biology to a legitimate natural science on par with physics and chemistry. Motivated largely by a rejection of reductionist mechanism and extravagant vitalism, the organicists sought in their attention to organization to pave an alternative disciplinary path for biology that was intellectually rigorous, experimentally well-founded, and adequately productive in identifying scientific problems and their solutions. The "problem of organization" in biology, as it was sometimes known, thus represents a key contribution to biological thought by organicists during that period. However, relatively few traces of this 'problem' remain in contemporary philosophy of biology. One reason for this appears to be that mainstream philosophers of science of the time, particularly Ernest Nagel and his student Morton Beckner, actively downplayed and rejected as irrelevant to biological research the significances attributed to organization by the organicists. In this paper I revisit the history of the problem of organization, looking particularly at its initial development, its subsequent rejection, and finally its enduring empirical basis in developmental biology.

Brown, Matthew
Parysa, Mostajir

Was Dewey Scientistic?

Abstract: Epistemological scientism is the view that science constitutes the best (or only) form of knowledge, and typically carries a couple of implications: First, non-scientific practices such as arts and humanities disciplines would be improved by adopting methods of the natural sciences, to the exclusion of traditionally used methods; Second, in failing to produce scientific knowledge, such practices make less important contributions to society (Peels 2018).An initial reading of Dewey might have us labeling him a scientizer, as many critics have (Hickman 1995). Dewey urges that scientific methods be employed in philosophy, value theory, education, and religion (LW1; LW9; LW13); He contributed to the Encyclopedia of Unified Science, which took the unification of knowledge under 'scientific method' as its principal goal; And he places strict constraints on what constitutes a form of knowledge. In his aesthetics, for example, Dewey insists that treating art as a form of knowledge was "a prime defect of philosophies of art" (LW 15, 99).Dewey's presumed "scientism," however, is an interesting case. Despite his enthusiasm for science, Dewey's interpretation of scientism's central concepts-specifically 'science' and 'knowledge'-significantly alter the import of his scientistic claims. Firstly, when Dewey exalts 'the scientific method,' he is not referring to the methods of the natural sciences per se, but to something he calls 'the method of intelligence.' This method involves analyzing the components of a problem in such a way that the relationship between certain procedures and their resulting consequences becomes clear. The relational clarity between means and consequences permits the development of techniques for the alleviation of the original problem. To employ the 'scientific' method in value theory, then, would not require the substitution of traditional axiological concepts with biological or neurological ones, nor would it require laboratory equipment. It would conceptually and empirically analyze the relationships between valued outcomes, their means of realization, and their consequences, to iteratively hone a refined system of values. This is far from the reductive image usually evoked when suggesting the expansion of scientific methods into other fields.Secondly, Dewey's insistence that non-scientific activities are not a form of 'knowledge' does not constitute a negative appraisal. Knowledge was not an end in itself, but was undertaken for the purpose of enriching lived experience as a whole, by enhancing our abilities to navigate our environment. This left room, in Dewey's philosophy, for plentiful non-cognitive activities to contribute distinct but important enrichments of lived experience. In his aesthetics, Dewey refers to important non-cognitive functions, including social communication, an expansion of perceptive and conceptual abilities, and a deepening of experience (LW10). Characterizing an activity as non-cognitive does not thereby reduce its social value.In Dewey's philosophy, we therefore find a version of scientism which made central scientistic claims whilst rejecting some of their usual implications, challenging

contemporary characterizations of the position. Dewey's philosophy can demonstrate how assumed meanings of scientism's central concepts radically influence its interpretation, reveal contingent elements of the present-day debate's structure, provide alternative framings, and complexify ongoing philosophical discussions.

Brown, Matthew

The Sources of John Dewey's Understanding of Science

Abstract: That John Dewey's philosophy of science is enjoying something of a renascence implies that there was a time where its reputation and interest in it had waned. This waning is reflected in the statement of Ernest Nagel, one of Dewey's most important students and colleagues: "The great William Harvey is reported to have said of Francis Bacon that he wrote about science like a Lord Chancellor. Of Dewey it can be said with equal justice that he writes about natural science like a philosopher, whose understanding of it, however informed, is derived from second-hand sources" (Nagel 1950, p. 247). I argue in this talk that Nagel should have known better. In this paper, I show that John Dewey had both first-hand knowledge of the sciences through his training and practicing as a scientist as well as intimate interactions with scientists that go beyond what we would call a reliance on "second-hand sources."In the first part of the paper, I will discuss Dewey's early training in psychology with G. Stanley Hall at Johns Hopkins, his empirical work in psychology and education research at the Universities of Michigan, Minnesota, and Chicago and his contributions to sociology at Chicago. (For those tempted to get hung up on Nagel's focus on "natural science," it is worth pointing out that Nagel himself analyzed the relation of the natural and social sciences at length and concluded that there is no sharp distinction in principle between the two.) In the second part of the paper, I will discuss Dewey's time at Columbia, focusing on his role in an empirical, ethnographic study of the Polish immigrant community in Philadelphia during 1918. The political implications of this project have received significant attention; less so its status as a scientific project and its relevance for understanding Dewey's philosophy of science. During this time, Dewey also connected with education researchers at Teachers College, and he attended lectures in the Psychology department. He also collaborated with child psychologist (and close friend) Myrtle McGraw on her Normal Child Development Study and other research during the 1930s. In the final part of the paper, I will discuss what Dewey learned from his close personal relationships with working scientists. I will emphasize in this section what John learned from his daughters Evelyn and Jane: Evelyn continued her father's work in empirical education research, while Jane was an accomplished quantum physicist. Their roles in keeping their father current on work in social and natural science has received insufficient attention. These are perhaps the most important two of many such relationships that informed Dewey's philosophy of science, including sometimes quite sophisticated commentaries on physics found more in private correspondence than in published writings. His grandson Gordon Chipman Dewey was also a physicist who worked on the Manhattan project, and his conversations with Jane and Gordon no doubt informed his understanding of modern physics. His close relationships with psychologists including George Herbert Mead and McGraw also informed his understanding of science.

Bujalance Fernández-Quero, Laura

Mary Hesse to the rescue in contemporary realism debates

Abstract: Contemporary debates on scientific realism take the 1999 standard version by Stathis Psillos as a starting point. His proposal delves with the long-discussed topic of realism from three stances: ontological, semantic and epistemic. In this paper I suggest to use Mary Hesse's insights on history of scientific change, especially in her 1961 book Forces and Fields (Hesse 1961), to counteract some of the criticisms that standard realism has received in the last two decades. Specifically, the focus will be on responding to the Pessimistic Meta-Induction (PMI) Argument, since PMI is the main argument against Scientific Realism. In order to understand Hesse's contributions to the Scientific Realism debate, I will first present Hesse's approach to explanation in science; Hesse introduces an account of science from what she calls 'satisfactory models', and presents it as an alternative to the Positivist and Realist's accounts of that time. Key characteristics of satisfactory models à la Hesse are; a) these models should have 'open texture' and b) they are characterized as intelligible thus, they are meaningful, testable and extendable. Within this context, giving an account of history of scientific change translates into giving an account of how models 'transition'.The main challenges Hesse's faces are a) explaining continuity in science when a model has to be abandoned; b) explaining what happens with concepts that have been disregarded by current theories. To the first, Hesse affirms that there are 'traces' left by the theory and that those 'traces' account for continuity in science; Regarding the second, she posits that there is not such a thing as a 'death metaphor', since most key theoretical concepts are still relevant because of the aforementioned 'traces'. Both answers to these challenges have something in common: none of them make use of neither the term 'success' nor the term 'truth'. This is noteworthy because current challenges to Scientific Realism, especially PMI, hinge on the use of these terms by scientific realists in their explanations and definitions. For instance, Anjan Chakravartty's (2017) epistemic stance on Scientific Realism depends on the concept of truth. It is also worth noting that PMI is a challenge that comes from the History of Science, and it is from the same realm that Hesse's response originates. Larry Laudan first articulated PMI in 1981. To counter this argument, Psillos employs the 'non-miracle' argument to support realism. According to this perspective the success of a theory would, from this point of view, legitimize some sort of realism. However, critics argue, using pessimistic induction arguments, that the 'non-miracle' argument may be a fallacy. This is because history reveals that scientific theories, once considered valid, true, or proven, are often rejected, falsified, or abandoned. Consequently, this casts doubt on the validity of previous and possibly current theories (Levitt, 2005; Wray, 2018). In her analysis of the history of scientific change, Mary Hesse challenges this notion. Her ability to do so arises from her avoidance of explaining continuity in science using the terms 'success' or 'truth'.

Camilleri, Kristian

The Historical Turn in Philosophy of Science

Abstract: While historical epistemology has a long tradition in France, going back to the early twentieth century, the connections between history and philosophy of science have a rather more recent history in the Anglophone world. Thomas Kuhn's Structure of Scientific Revolutions is generally thought to have marked a significant turning point in inaugurating what has come to be known as the "historical turn" in philosophy of science in the twentieth century. But while Kuhn has often been seen at the vanguard of integrated history and philosophy of science, he himself expressed strong reservations about whether history could, or even should, play a substantive role in philosophy of science. Kuhn insisted that history and philosophy were, in some sense, incommensurable, and could not be practiced at the same time. Indeed, by the end of life, Kuhn proposed that evolutionary philosophy of science, in which science was understood as a dynamic rather than static enterprise, could be pursued without paying much attention to the historical record. Yet, for all Kuhn's misgivings, a number of distinct programs for a historical approach to philosophy of science did take shape in the 1970s and 80s, as a new generation of philosophers turned to the study of historical episodes both as a source of inspiration and evidence for different views of scientific change. But the question of how history can and should inform the philosophy of science would continue to be the subject of debate and disagreement. Philosophers increasingly found themselves forced to grapple with complex historiographical questions and even to reflect on the very aim of philosophy of science itself. In this paper, I reflect on the various forms that the "historical turn" took in the years following Kuhn's Structure, and the transformative effect that the use of historical case studies had on very identity and practice of philosophy of science. Here I argue that the debates over (i) the normative versus the descriptive, (ii) the general versus particular, and (iii) rational reconstructions versus historical explanations shaped a number of new approaches to hyphenated history-and-philosophy of science that were forged in new institutional contexts and disciplinary alignments. By the turn of the century, a new role for philosophical case studies had emerged, alongside a series of 'manifestos' for doing integrated HPS. This paper serves as a preliminary attempt to understand this transformation that occurred in philosophy of science in the last third of the twentieth century.

Cañas, Dubian

How historiographical standards adjudicate philosophical disagreements

Abstract: The use of historical case studies as evidence for and against philosophical claims is a central methodology in the selective realism debate (e.g., Lyons & Vickers 2021). However, a second-order argument against this methodology has been put forward recently (Kinzel 2015, 2016; Pitt 2001; Schickore 2011, 2018). According to this argument, historical evidence cannot adjudicate philosophical disagreements because historical reconstructions are theory-laden, thereby historical data are not neutral vis-à-vis the philosophical claims being tested. This argument identifies two types of standards that are insufficient to settle conflicts between competing philosophical positions regarding the same historical case. On the one hand, philosophical standards (PS) are not neutral because they make up the auxiliary theory to interpret historical material; on the other hand, historiographical standards (HS) underdetermine the rivals provided that such standards are neutral but too weak. As a result, the history of science cannot adjudicate the selective realism debate (Chakravartty 2017; Vickers 2017).This paper aims to respond to this second-order argument. I examine and evaluate the disagreement about the strategy of 'selective confirmation' (SCD) concerning the historical case of the caloric theory of heat (Psillos 1999; Chang 2003; Stanford 2006). SCD has been over the possibility of establishing an explicit, historically reliable criterion for identifying those theoretical elements responsible for the empirical success and retained across theory change. I want to defend the thesis that SCD is to be explained in terms of a resolvable conflict considering the relevant historical evidence, in which HS play a crucial role in adjudicating this disagreement in terms of the quality of the historical reconstructions of the caloric episode.Bolinska & Martin (2020) have argued that applying PS can settle philosophical disputes as long as PS are adopted on independent grounds to connect historical evidence with philosophical claims. However, I show that this goal cannot be accomplished because independent reasons for PS are also a matter of dispute in the context of SCD. For that reason, I examine how HS are specifically applied in SCD and contend that HS can arbitrate the conflict successfully.Here is a plan. First, I propose that there is a standard of "diachronic historical adequacy" in the historiography of science that is independent of the philosophy of science. Second, I show how this standard is relevant for SCD, and accepted by and neutral regarding the competing philosophical positions. This standard is implied by the fact that selective realism must be tested against past scientists' judgments of selective confirmation. Third, I argue that when the standard is applied, the textual evidence from the caloric episode suggests that scientists' judgments of selective confirmation are unreliable. On these grounds, I claim that we have good reasons to believe that conflicting historical renderings of the caloric episode can be ranked in terms of historical adequacy, thereby the historical evidence can support

Stanford's position better than Psillos's. Finally, I conclude by suggesting that the canons of historical analysis could adjudicate philosophical disagreements more generally.

Cerapaite, Kristijona

Exploring the Linguistic Dimension of Leibniz's General Science: The Influence of the Universal Characteristic

Abstract: Gottfried Wilhelm Leibniz (1646–1716) stands as a towering polymathic mind in the history of philosophy, celebrated for his multifaceted contributions that encompass metaphysics, epistemology, mathematics, and much more. Among his diverse philosophical endeavors, the concept of the general science (scientia generalis) has emerged as a subject of significant interest. In Leibniz's expansive vision, the general science is "nothing else than the science of thinking [...], which is not only a logic [...] but also an art of invention and a method of ordering knowledge, a synthesis and analysis, didactics and a science of teaching" (Couturat 1903: 511). Closely related to Leibniz's general science is the concept of the universal characteristic (characteristica universalis). It is envisioned as a universal language or symbolic system, a language of thought and "the art of forming and arranging the characters in such a way that they agree with the thoughts, i.e. so that they have amongst them the same relation that subsists amongst thoughts" (Leibniz, cited in Mugnai 2018: 178).Despite the prominence of the general science and the universal characteristic in Leibniz's writings, the relationship between the two remains a subject of debate. While scholars generally acknowledge the close interconnection of these concepts (Burkhardt 1987, Gensini 1992, Pelletier 2018), there is a sense of mystery surrounding the linguistic influence of the universal characteristic within the general science. Therefore, this presentation seeks to elucidate the relationship between Leibniz's universal characteristic and the general science and offer a novel aspect of interpretation about general science's linguistic dimension. By delving into the linguistic dimensions of Leibniz's thought, we shed light on the processes by which ideas are formulated, communicated, and advanced within the general science. Leibniz himself expressed a profound interest in the idea of a universal language that could serve as a means of precise communication among thinkers of diverse backgrounds. His aspiration for the universal characteristic to constitute such a language prompts us to reevaluate the fundamental nature of general science.In this exploration, we navigate the intricate terrain of Leibniz's philosophy, scrutinizing the intersections of language, thought, and scientific methodology. By engaging with Leibniz's insights on the linguistic dimension of general science, we not only deepen our understanding of his philosophical legacy but also contribute to ongoing debates concerning the role of language in scientific inquiry, the nature of scientific representation, and the methodology of scientific thought.ReferencesBurkhardt, H. 1987. The Leibnizian characteristica universalis as link between grammar and logic. In: Speculative grammar, Universal Grammar, Philosophical Analysis, ed. Buzzetti, D., Ferriani, M. Amsterdam: John Benjamins Publishing Company, 43–64.Couturat, L. 1903. Opuscules et fragments inédits de Leibniz. Paris: Felix Alcan.Gensini, S. 1992. « Filum meditandi »: Semiotics and Scientific Knowledge in the thought of G. W. Leibniz. In: Histoire Épistémologie Langage 14(2): 107–127. Mugnai, M. 2018. Ars

characteristica, logical calculus, and natural languages. In: The Oxford Handbook of Leibniz, ed. M. R. Antognazza, 177-207.Pelletier, A. 2018. Scientia generalis and encyclopedia. In: The Oxford Handbook of Leibniz, ed. Antognazza, M. R. New York: Oxford University Press, 162–176.

Chandler, David

Carnap’s distinction in modes of speech and the intersubjectivity of science from 1928 to 1932

Abstract: This paper attempts to unravel some of the potential significance of Carnap's efforts to distinguish between the formal and the material mode of speech throughout specific sections of his 1932 article "Die physikalische Sprache als Universalsprache der Wissenschaft" ("The Physical Language as the Universal Language of Science"; translated into English and republished as Unity of Science in 1934, hereafter "Universalsprache"). My thought is that we can offer an informative comparison between the published "Universalsprache" and its unpublished counterpart-otherwise known as "Proto-Universalsprache"-whilst maintaining the sense that these are two versions of the same text so as to draw out the significance of Carnap's introduction of a distinction in modes of speech in the former. This way, I plan to observe the role of such a distinction and the implications of its inclusion therein with respect to a problem concerning intersubjectivity that was previously addressed in the earlier Der Logische Aufbau der Welt in so far as this issue resurfaces in both "Proto-Universalsprache" and "Universalsprache" and yet is met with a different response in each. While Carnap sought to accommodate for the intersubjective nature of science by establishing the intersubjectivity of physical language since it was this language that would serve as the common reduction basis for all scientific statements in the published 'Universalsprache", the situation in "Proto-Universalsprache" is observably more complicated.My argument moves through the following steps. I begin with some preliminary comments on the Aufbau. There, I will provide a sketch of what I will continue to refer to as the 'problem of reduction'. Suppose we reason that all scientific statements are reducible to a certain basis, and we recognise the intersubjective nature of science. In that case, we must account for the intersubjectivity of statements within this language. From this, I turn to "Proto-Universalsprache". My reason for consulting this draft manuscript is that while Carnap considers the matter of the intersubjectivity of physical language in both the 1930 and 1932 versions of the text, he does so in the former without the use of the distinction between the formal and the material mode of speech. Therefore, as I will argue, the consequence of this absence is the imposition of a restriction on the universality of physical language, i.e., the extent to which statements can be reduced to a physicalistic basis. I will show that such a constraint is lifted in the published version of the text since the introduction of the distinction between the formal and the material mode of speech within "Universalsprache" allowed Carnap to drop the restriction in "Proto-Universalsprache" concerning the translatability of scientific statements into physical language to only those sentences which are intersubjective since in that such a distinction allowed him to dismiss the worry that appeared as a result of this restriction in "Proto-Psychology" that phenomenalistic statements were untranslatable, or only partially reducible, into physical language.

Chatzigeorgiou, Konstantinos

Philosophy and the Historiography of the Scientific Revolution: Revisiting the Burtt-Strong Debate

Abstract: The first few decades of the 20th century witnessed a systematic effort in providing unifying and explanatory accounts for the various changes occurring in the natural sciences from Copernicus' De Revolutionibus to the publication of Newton's Principia - the 'scientific revolution', as popularised by Alexandre Koyré. Two influential attempts from that period were Edwin Arthur Burtt's 1924 The Metaphysical Foundations of Modern Physical Science and Edward Strong's 1936 Procedures and Metaphysics, A Study in the Philosophy of Mathematical-Physical Science in the Sixteenth and Seventeenth Centuries. In the case of Burtt what is typically detected is the view, subsequently elaborated by Koyré himself, that the scientific revolution of the 16th and 17th century should be understood as a Platonic or Pythagorean mathematisation or geometrisation of nature, leading to a separation of all qualitative characteristics – and humans themselves – from nature's domain. In contrast, Strong has been treated as a critic of the mathematisation thesis, maintaining that it was the Euclidean and Archimedean instrumentalistic treatment of mathematics that formed the key inspiration of early modern science, not the Platonic one. Hence, and with certain exceptions, no Platonic metaphysics can be attributed to the 'scientists' in question. Both works have often been treated as minor classics in the historiography of the scientific revolution, and, therefore, key texts of 20th-century science studies.What is less appreciated is that both Burtt and Strong composed their respective treatises as dissertations in Columbia's philosophy department under the supervision of F. J. E. Woodbridge. It is thus no surprise that a more careful reading reveals an array of philosophical arguments underpinning each historical narrative. For instance, in an extensive concluding section, Burtt argues against the mind-brain localisation thesis, whilst undermining any attempt to ontologically privilege mathematical physics. Strong, in turn, offers an elaborate appendix that argues in favour of an 'operational' or 'pragmatic' understanding of key concepts of Euclidean geometry like the point and line. Throughout his dissertation, Strong further argues against those who conflate 'mathematics' with 'physics' or other intellectual enterprises. When such facts are recognised, one may reasonably conclude that Burtt and Strong are perhaps responsible for the first extensive works of integrated history and philosophy of science on American grounds.In my talk, I will begin by briefly summarising the historical narratives of Burtt and Strong's dissertations, before examining in greater detail their respective philosophical underpinnings. One surprising conclusion is that, despite genuine historiographical and historical differences, a common philosophical core can be extracted. Both Burtt and Strong strongly oppose an 'empiricist' conception of the human mind, whereby human experience consists of intermediary mental objects between ourselves and the world. Furthermore, both agree that mathematical physics cannot offer a complete picture of nature.

Chikurel, Idit

Maimon as a Baconian: Induction, Empirical Objects and Natural Histories

Abstract: In my talk, I assert that Salomon Maimon's (1753-1800) philosophy was very much affected by Bacon's work, and show that he was not solely influenced by the commonly mentioned philosophers such as Kant, Leibniz and Spinoza. Based on Maimon's commentary on Bacon's Novum Organum [Bacons von Verulam Neues Organon, 1793], I discuss this influence in three main aspects: His use of induction, the employment of natural histories and his approach to empirical objects. Moreover, I show how turning to these three elements is intertwined with his skeptical stance towards necessary knowledge. For instance, Maimon employs induction to arrive at a higher degree of subjective necessity, a process that is infinite since, according to him, objective necessity of empirical knowledge cannot be achieved. Maimon also adopts Bacon's method of founding philosophy on the basis of natural histories. He presents a short history of mathematical inventions based on Montucla's History of Mathematics (1758), which includes many empirical discoveries, as well as a short essay describing philosophical systems, based on Bayle's Historical and Critical Dictionary (1697). Both histories serve as the grounds on which Maimon develops his own philosophical inquiry. This method embodies the idea of establishing knowledge on facts, not merely on symbolic cognition and ideas. In his regards to empirical objects, Maimon adopts a skeptical stance, since he believes that we may increase our knowledge of empirical objects and connections between phenomena, but we cannot show that judgments on empirical objects are objectively necessary. Accordingly, he rejects Kant's claim that judgments of perception can become judgments of experience and asserts that this transformation is impossible.

Chin, Adam

Hypotheses and the Aims of Natural Philosophy

Abstract: As is well known, by the start of the 18th century, a twilight had fallen on the use of hypotheses. But as noted by Laudan (1981), this is puzzling for by the mid-1800s, the condemnation had turned to acclaim. Nineteenth century scientists and philosophers of science embraced the method of hypotheses, writing treatises praising and justifying its use. As Laudan points out, however, the arguments found in favour of the use of hypotheses were not new---in fact, they could even be found within the eighteenth century.For despite the general twilight, a sparse handful of natural philosophers defended the use of hypotheses, even encouraged it. Prominent among these dissenters were Emile DuChatelet (1706-1749) and Georges-Louis LeSage (1724-1803). Not only did these natural philosophers advocate for the use of hypotheses, but they claimed that their contemporaries and all the greats of science from Kepler to Huygens in fact used hypotheses, and used them in ways essential to the progress they had engendered. Even Newton himself was accused of such (by the common consensus) blasphemy!This presents a further puzzle. On the one hand we have the method of hypotheses widely and publicly condemned. Yet on the other hand we have proponents of the method who declare that it is widely employed. Was the larger scientific community simply blind to their own methods, so caught up in their worship of Newton that, no matter their actual practices, they had to insist that they did not hypotheses fingunt? Or was this small contingent of natural philosophers just wrong in their description of their peers, projecting their own philosophy upon the actions of others?I will begin with a sketch of the defenses offered by DuChatelet and LeSage and set up the puzzling debate. However, as I will demonstrate, the two sides of ``the debate" use the term ``hypothesis" in different ways. However, the disagreement between these two groups is not merely verbal; there is still real disagreement, not over the use of ``hypotheses," but rather over the aims of natural philosophy. I detail this ``teleological" difference and tie it back to the semantic difference. Together, these two differences can explain the often vehement rhetoric of the supposed debate over hypotheses, but they cannot explain why the two groups had different notions of ``hypotheses." In the conclusion I offer some possible explanations for the emergence of the semantic divergence. In resolving the puzzle over the condemnation of hypotheses, I shed some light as to why the condemnation of hypotheses was as harsh as it was and why their use fell out of favour: they were taken, at least on one reading of ``hypothesis," to fall outside the realm of natural philosophy. And in so doing, I further reveal the character of some of the pieces in Laudan's larger puzzle over why the method of hypothesis re-emerged in the th century, essentially unchanged by the interim.

Chirimuuta, Mazviita

Origins of the Idea that Thinking is Modelling

Abstract: Nowadays it is commonplace for cognitive scientists and AI researchers to assume that thinking about the external world amounts to generating a model of the world.[1] This paper investigates the origins of this idea through a study of the writings of Helmholtz and Mach, both active in physics and in physiological psychology during the period between 1850 and 1900. The proposal that representational practices in physics in this period engendered a different way of understanding what it means, in general, to perceive and to know an external object, is consistent with Daston and Galison's thesis that the period sees the emergence of a new notion of "structural objectivity" whereby what is knowable and communicable comes to be identified with "pure relation" (2007:283). I show how the thinking is modelling proposal was grounded in certain notions of concept formation that were directly tied to an understanding of the scientific process as being that of finding invariant regularities in nature and representing them with structurally analogous schemes (e.g. mathematical dependencies). Since thinking is equated with conceptualisation, it follows that to think is to devise some such structure. Helmholtz and Mach were diametrically opposed on many fundamental philosophical and scientific points but there are interesting commonalities in their treatments of conceptualisation which, incidentally, find a synthesis and expression decades later in Cassirer's 1910 Substanzbegriff und Funktionsbegriff, a work influenced by both of these thinkers. Helmholtz's 1878 lecture 'Die Tatsachen in der Wahrnehmung' argues that what comes to be known through perception are the lawlike regularities amongst fleeting sensations, rather than any semblance of the external object; and that to isolate an invariant amongst a set of varying particulars is to form a concept of them, and therefore think of them ("if 'conceive' means to form concepts, and if we try to summarize in the concept of a class of objects their similar characteristics, then it follows completely analogously that the concept must try to summarize a changing series of phenomena over time, one which remains the same in all its stages", 1878/1995:360). The processes of inductive and causal inference required for this conceptualisation are directly compared to the operations of scientific reasoning, and the product of science is similarly the derivation of a law which describes an invariant regularity. Mach's 1882 lecture 'Über die ökonomische Natur der physikalischen Forschung' describes physical laws in a way highly reminiscent of the current notion of data models, i.e. as very compact summaries of observable data. In various ways Mach describes how these "economical" methods are continuous with the thought processes of ordinary life. Mach's monistic ontology suppresses questions regarding the existence of physical objects 'behind' the sensory data, whereas Helmholtz invites a notion of external reality whose structures we dimly determine behind a veil of subjective sensations. [1] In this paper I use the term of 'model' in its current wider sense, to include mathematical as well as concrete models.

Christian, Fleck

Robert K. Merton's skirmishes with the next generation of constructivists

Abstract: Robert K. Merton (1910-2003) is seen by most historians of the social sciences as the founder of a new subdiscipline within sociology, the sociology of science. From his PhD thesis written in Harvard under the supervision of Pitirim Sorokin and George Sarton (published as Science, Technology, and Society in Seventeenth Century, 1938 in Osiris) until the late 1960s Merton participated in several organizational attempts to establish the new field. He published on the topic, won several major grants to subsidize graduate students to do research under his direction, and took an active role in the research committee sociology of science within the International Sociological Association and the division of the same name inside the American Sociological Association. At the end of the 1960s several younger sociologists (but also members from other disciplines) started criticizing Merton and his approach. The controversies were fueled by generational rivalry (remember that back then youngsters did fight their elders as representatives of an establishment), political cleavages (Merton was seen by the representatives of the 'disobedient generation' as belonging to the liberal mainstream, Sica & Turner 2005) and competition between elite departments and the rest.As a consequence, Merton resigned from involvements in professional organizations but did remain influential as a mentor for several publication projects, as a referee for advanced fellowships, and writer of recommendation letters (Korom 2020).In using the Robert K. Merton Papers at Columbia University my paper will cover several of these activities. Starting with Merton's involvement with the establishing of the Institute of Unified Science in Harvard by Philipp Frank, I will then discuss the fall-out between Merton and the younger generation around 1968 and finally present his interventions against a too heavily constructivist presentation of the field in the International Encyclopedia of the Social & Behavioral Sciences, edited by Neil Smelser and Paul Baltes in 2001.Literature:Korom, Philipp. 2020. „Der talentierte Briefeschreiber Robert K. Merton als einflussreicher Gate-Opener: Eine Analyse von 1460 Empfehlungsschreiben." Zeitschrift für Soziologie 49 (4): 249–64. doi:10.1515/zfsoz-2020-0022.Merton, Robert K. 1938. "Science, Technology and Society in Seventeenth Century England." Osiris studies on the history and philosophy of science, and on the history of learning and culture ; Vol. 4, P. 2. Bruges: Saint Catharine Press.Sica, Alan and Stephen P. Turner, Eds. 2005. The Disobedient Generation: Social Theorists in the Sixties. Chicago: Univ. of Chicago Press.Smelser, Neil J. and Paul B. Baltes, Eds. 2001. International Encyclopedia of the Social & Behavioral Sciences. Amsterdam: Elsevier.

Čížek, Jan

Samuel Pike's 'Philosophia Sacra' (1753): Between 'modern' Newtonianism and 'outmoded' Biblical physics

Abstract: This paper deals with the work 'Philosophia Sacra: Or The Principles of Natural Philosophy. Extracted from Divine Revelation,' written by the relatively unknown English clergyman Samuel Pike (circa 1717–1773) and published in 1753. On the one hand, this work can be characterized as a belated continuation of the tradition of so-called 'Mosaic physics,' a distinctive early modern effort to present natural philosophy based on a literal interpretation of the Holy Scripture, particularly the initial chapters of the Book of Moses, Genesis (hence the terms 'Biblical', 'sacred' or 'Mosaic' physics) that flourished especially during 16th and 17th centuries. For this reason, I will first elucidate the main ideas of this movement, as expressed by its founding representatives, namely Lambert Daneau, Otto Casmann, and Kort Aslakssøn.Subsequently, I will analyze Pike's own natural-philosophical concept with a particular focus on its Newtonian dimension. Additionally to its somewhat 'outmoded' Biblical foundation (Mosaic physics was heavily criticized already in the first part of the 18th century; there were, therefore, not many representants of this movement during Pike's lifetime), Pike's work tends to be very 'modern', especially in its decisive advocacy of Newtonianism with all of its crucial concepts, i.e. the theory of universal gravitation, defense of Heliocentrism, etc. Moreover, Pike believes he could even surpass Newton, for he could present not only a description of how nature works (as Newton did) but also identify the very first causes of all its processes. In this regard, Pike employs another source of his natural philosophy: the ideas of the Moravian theologian and philosopher Johannes Amos Comenius, as explained in his treatise 'Physicae synopsis' (first edition 1633; extended edition 1663). Samuels Pike's 'Philosophia Sacra' appears to be a remarkable example of how Newtonian natural philosophy was defended and, most notably, combined with approaches that seemed abandoned for quite a long time, namely Mosaic physics with its various representants. (Note that Comenius was by many early modern historians of philosophy regarded not only as a successor of the first generation of Mosaic physics, Daneau, Casmann, and Aslakssøn but even as a consummator of this intellectual endeavor as such!). Pike's views of how natural philosophy should be established and its purpose determined represent a noteworthy contribution to the early modern discussion concerning the essence of natural philosophy – or the emerging modern science.

Colomban, Dinh-Vinh

Statistics in the 18th Century: Tool of Government to Scientific Instrument? The Case of Demography in Buffon's Natural History

Abstract: While statistics emerged in the second half of the 17th century, they were confined to political uses rather than scientific ones. Statistics dealt with economic and social phenomena. They helped grasp trends useful for the government that remained otherwise invisible when considered only through individual cases (life expectancy, death rate, sex ratio at birth, wedding rate). Meanwhile, statistics have been consciously absent from natural philosophy for most of the 17th and 18th centuries. Even the new experimental method of science heralded by the Royal Society, which held a strongly 'probabilistic' view before the Newtonian turn, seemingly did not use statistics to quantify empirical observations and experimental data.However, the birth of statistics is closely intertwined with demography. If demography was first conceived as a tool of government (as with William Petty's (1613-1697) "political arithmetic"), it can also be seen as an endeavour toward a scientific study of human life. French naturalist Georges-Louis Leclerc de Buffon (1707-1788) includes demography in his Histoire naturelle [Natural History] (1749) for his study of L'histoire naturelle de l'homme [Natural History of Human] which partly relies on statistical life tables. However, Buffon conceives his natural history as a scientific study of nature that departs from the Newtonian model. Buffon indeed rejects the use of mathematics in natural history because of their "fictional and abstract" nature. He advocates instead to focus on the real individuals that compose the natural world and to study both their variations and common features. This inclusion of statistics through demography in Buffon's natural history, which otherwise seems to reject mathematics, then begs the question of the very epistemological status of statistics. Does this mean that Buffon, a mathematician by training who also extensively worked on probability calculus[1], does not see statistics as belonging to mathematics? In his 1777 additions to the Histoire naturelle, Buffon brings together his previous mathematical works on probability ("Essai d'arithmétique morale" [Essay of Moral Arithmetic]) before studying "Les probabilités de la durée de la vie" [The Probabilities of the Length of Life] through multiple statistical life tables. Demography seems therefore to belong both to "political arithmetic" and to "natural history". How does this dual nature affect its use by Buffon? And how did the epistemological framework of natural history (contra Newtonian physics) lead Buffon to value statistics as a legitimate scientific tool?[2][1] See Buffon's « needle problem » or his work on the « St. Petersburg paradox ». Lorrain Daston (1988) emphasized the importance of the latter for probabilistic reasoning in the 18th century.[2] Giuseppe Leti (2000) notes that as long as statistics were merely descriptive they could not serve as a proper tool for a scientific inquiry of nature, which looks for laws of phenomena and not mere description.

Connolly, Patrick

Newton's Letters to Bentley in Context

Abstract: There is widespread agreement that Isaac Newton's letters to Richard Bentley are a key resource for understanding some of his most basic metaphysical commitments. Unfortunately, there is widespread disagreement over the correct interpretation of Newton's claims in these letters. Does he adamantly deny the possibility of action at a distance? Or are the letters neutral on the topic? Does he suggest that God is directly responsible for gravitational attraction? Or do the letters prove that he believed in some secondary cause? Scholars disagree on these issues. This paper shows that these interpretive debates fail to give sufficient consideration to several key features of the context in which the correspondence occurred. It then argues that this context has important ramifications for the correct understanding of Newton's claims. The correspondence was occasioned by Bentley's appeal to Newtonian natural philosophy in his Boyle Lectures. Eric Schliesser, one scholar who has attended to the context of the correspondence, highlighted 1) that Newtonian natural philosophy, even at this early stage, could be seen as suspiciously similar to atheistic Epicureanism, 2) that the Boyle Lectures represented a precious opportunity to align Newtonianism with orthodox Christianity, and 3) that Bentley was an eminent and well-connected figure, someone Newton could ill-afford to antagonize. This paper builds on and expands these insights. Specifically, the paper focuses on the precise timing of the exchange and on the content of Bentley's systematic metaphysics. It is sometimes incorrectly claimed that Bentley wrote to Newton while preparing his lectures for the press. By the time of Bentley's first letter to Newton, all eight of Bentley's lectures had already been publicly delivered and the first six had been published. Further, Newton knew the first six lectures were already in print; Bentley told him as much. Thus, by the time of the correspondence, the content of the lectures and their argumentative structure was largely fixed. Further, the content of the lectures was fixed in a way with important ramifications for the issues discussed in the correspondence. This is because of the strong metaphysical positions Bentley had taken in the first six lectures. His arguments relied on substantive claims about the nature of matter, causation, and divine agency. Most importantly, he asserted that matter was completely passive and relied on this assertion as a key premise in his argument for God's existence. As he explained to Newton, and clearly presaged in the fourth lecture, the seventh lecture had argued that God must be responsible for gravity insofar as passive matter could not account for its effects. Seen in this light, Newton is not dispensing wisdom to a young acolyte. Nor are he and Bentley having a free-wheeling metaphysical discussion. Their focus is on a specific argument whose success requires some highly restrictive assumptions. This, I argue, allows us to see why Newton limits his claims to "inanimate brute" matter, why he emphasizes mutual contact, and why some of what he says is at variance with his views elsewhere.

Cooper, Andrew

Induction as action: resolving the problem of Whewell's idealism

Abstract: William Whewell is a towering and yet ambiguous figure in Victorian science. Together with John Herschel and John Stuart Mill, he sought to develop a logic of induction that could vindicate the cognitive autonomy of scientific knowledge. Yet his theory of induction was severely criticised by the likes of Mill for advancing a 'German, a priori view of human knowledge', raising what Robert Butts (1967: 177) terms 'the problem of Whewell's idealism': was Whewell philosophically confused, and failed to see that Kant's transcendental idealism is incompatible with the emerging inductive sciences, or did he successfully harmonize what seem to be historically incompatible philosophical alternatives?In this paper I take a step back from the Mill-Whewell debate to examine the role of idealism in a broader exchange of ideas between Germany and England, giving particular focus to the institutional context of Trinity College in the first half of the nineteenth century. To address the problem of Whewell's idealism, I examine his published work against his unpublished letters and notebooks to show that his early encounter with Kant provided a framework to shift inquiry from the objects of science to the practice of science itself. While Whewell's idealism is not transcendental in Kant's sense of the term, his philosophy of science nevertheless extends a key Kantian distinction between objects and ideas, such that the task of a philosophy of science is to discern the forms of judgment-what Whewell terms 'mental tendencies'-that determine the inferential structure of scientific inquiry. The result is a distinctly Victorian idealism that, despite having clear continuities with Kant, must be understood on its own terms. I conclude by suggesting that Whewell's response to British empiricism can assist us to better appreciate the importance of idealism to the early development of philosophy of science.

Costantini, Marco

Density of Matter and Excellence of Spirit: The Astronomical Roots of 1755 Kantian Psychology

Abstract: In the Universal Natural History and Theory of the Heavens (1755), Immanuel Kant (1724–1804) theorizes a correspondence between the coarseness of the fabric of the human body and the degree of development of the cognitive power of the mind. The lighter, thinner and more flexible the machine of the body is, the more perfect the mind that inhabits it will be, that is, it will have clearer concepts and will be more able in judgment. This thesis is placed by Kant within an astronomical framework, which allows him to hypothetically formulate a general thesis regarding every form of life in the universe: thinking natures will be all the more excellent the more the places they inhabit are distant from the star located at the center of their planetary system. Indeed, according to Kantian cosmogony, in the process of formation of the universe the denser materials with a greater gravitational force were naturally gathered closer to their center of gravity. An important aspect of Kantian cosmogony is the fact that the different genres of matter were distributed at the most favorable distance from the Sun for them. Indeed, greater exposure to light and heat in less dense materials would have caused their destruction. The birth and prosperity of certain life forms on different planets is thus considered by Kant a sign of the divine providence.

Cozzoli, Daniele

Darwinism meets Quantum Mechanics: the work of Mario Ageno on the Origin of Life (1915 – 1922)

Abstract: This contribution focuses on the work of Mario Ageno (1915-1992), initially director of the physics laboratories of the Italian National Institute of Health and later professor of biophysics at the Sapienza University of Rome.It aims to assess the role Ageno played in the intertwined debate on the origin of life. It focuses particularly on how Ageno tried to couple Quantum Mechanics and Darwinism in explaining the phenomenon of life. A physicist by training, Ageno became interested in explaining the origin of life by means of quantum mechanics after reading a book by Schrödinger, who argued that quantum mechanics was consistent with life but that new physical principles must be found. Ageno turned Schrödinger's view into a long-term research project.Ageno's research focused initially on nucleic acid of phages acid of phages and bacteria. In the mid-1960s he discussed with Wigner about Quantum mechanics and the origin of life. In 1965, Wigner and Landsberg stated that, as quantum mechanics cannot explain life, the notion of consciousness should be included therein. Ageno replied that, on the contrary, quantum mechanics did not contradict the possibility of life. As of 1979, Ageno focused on experimental research on the organization of the bacterial cell and developed a growing interest in the philosophical basis of Darwinism. The two projects were connected, as he tried to explain the evolutionary meaning of the avoidance reaction among bacteria in a liquid culture. His aim was to build a functional model of the simplest kind of organism as the first step towards his grand design of explaining the transition from non-living matter to a living organism by quantum mechanics. In 1986 Ageno published Le radici della biologia(The Roots of Biology), in which he framed his views within the synthetic evolutionary theory by working out Ernst Mayr's biological concept of species. Indeed, in the 1980s and the 1990s his approach led him to participate with the philosopher Vittorio Somenzi and the physicist Marcello Cini in a debate on the nature of the theory of evolution and its broader scientific and philosophical meaning. The interest of the Italian researchers for Darwin's theory mirrored the wider debate among evolutionary biologists and philosophers of science on evolution, its epistemological status and its consequences that unfolded between the 1970s and the 1990s.

Crewe, Bianca

The Scientific Worldview as Synthetic Technique: Philipp Frank on the Precedents and Rivals of Logical Empiricist Unified Science

Abstract: As a piece of philosophical terminology, synthesis recalls the aspirations of Naturphilosophie in the early nineteenth century-a priori unification of the special sciences-and the popularization of neo- Hegelian vocabulary and methods in interwar German academic philosophy. Why then do we find Philipp Frank repeatedly presenting logical empiricism and the scientific world-conception as aspiring not just to the analysis and clarification of knowledge, but also to its synthesis? I suggest that for Frank this shows the centrality of the scientific world-conception as a method for integrating science into a general framework of human knowledge, and as a point of connection between the various unity of science projects pursued by Vienna Circle affiliates. His emphasis on synthesis registers logical empiricism as a viable candidate in the theatre of socially relevant and pragmatically engaged philosophy by repurposing the vocabulary of an intellectual culture generally critical of the integrative potential of science and positivism. In his work we find an important point of contact for situating early scientific philosophy in relation to this broader culture, which likewise pursued unified knowledge through very different ideological and methodological channels. This focus will also bring forward Frank's defense of positivism, and particularly his historical presentation thereof around the turn of the century. He writes of the philosophy of science in Poincaré, Mach, and Hertz (among others) as initiating a break with past attempts to synthesize science and philosophy through reduction to mechanics or through an evocative philosophical vocabulary. By attending to how approaches to unity and integration are themselves at stake in this historical presentation, I hope to contribute to a literature that has begun to take stock of Philipp Frank as a philosopher of science, and to simultaneously consider how the social and political commitments of the logical empiricists were carried over into their theoretical work.

Cristalli, Claudia

What is Scientific Philosophy? Charles W. Morris and the Fortune of Logical Empiricists in the United States

Abstract: Among the American philosophers who actively contributed to the success of Logical Empiricists in the United States, Charles W. Morris (1901–79) is generally acknowledged as a pivotal figure (Richardson 2008, 256-8). However, in spite of his historical significance, very few studies interrogate the reasons for his success, as well as for his interest in logical empiricism in the first place. Indeed, Morris was originally trained as a pragmatist under Herbert Mead (1922-25), and part of his reputation is connected to his editorial work in publishing the latter's lecture notes as Self and Society (1938). This work seeks to recover the early Morris from its neglect and to explain his early interest in the logical empiricists' philosophy. The ideal point of departure is Morris' (1925) doctoral dissertation, where he attempts a fist formulation of his philosophical stance. In this early writing, Morris already exhibits a scientific philosophy that he explicitly presents as "philosophy of science." I maintain that it is the attitude of scientific philosophy, more than anything else, what is common to the community of American philosophers who were receptive of Logical Empiricism, regardless of their different philosophical banner (i.e., pragmatism, realism, or idealism). Moreover, while it is generally accepted that the scientific philosophy of logical empiricism was largely based on physics wile among pragmatists biology and psychology were the obvious sources of inspiration, a close reading of Morris dissertation shows that the impact of physics and particularly of Einstein's theory of relativity was felt also among pragmatists, and that it deeply influenced the still biological articulation of their theory of experience and of reality. As a habit of thought, scientific philosophy was the bridge between the two shores of the Atlantic and it enabled a fruitful dialogue between a plurality of philosophical schools in the 1920s and 1930s.

Crull, Elise

Heisenberg and the Incompletability of Quantum Mechanics

Abstract: That there was heated debate in the 1930s regarding the completeness of quantum mechanics is well known. Less familiar is the fascinating contribution to this conversation made by Heisenberg, who argued somewhat radically that quantum mechanics was not only incomplete, but incompletable. By working with various primary source materials heretofore little known (including an unpublished response to the EPR paper arguing for incompletability written at the request of Pauli and under the significant influence of Grete Hermann's thinking, as well as correspondence with Hermann and others on this matter), a richer - and strikingly different -- account of Heisenberg's views on completeness emerges than can be distilled from his (albeit more familiar) post-War discussions regarding closed vs. open theories.

Cuffaro, Michael

Kantianism with a Human Face

Abstract: The interest in Grete Hermann as a philosopher of quantum mechanics has grown rapidly in recent years, and she is now well known for her in-depth analysis of causality and hidden variables. There has also been increased interest in how Hermann's work on quantum mechanics fits into the wider framework of her neo-Kantianism, which takes the (neo-)Friesian views of Jakob Friedrich Fries and Leonard Nelson as its starting point. Hermann's particular version of neo-Friesian neo-Kantianism is deeply rooted in the insights she gained from studying quantum mechanics and from the (mutually beneficial, according to all concerned) debates on the topic that she had with the circle of physicists associated with Niels Bohr (especially Werner Heisenberg and Carl Friedrich von Weizsäcker) in the 1930s, which she developed, during the years 1936-37, into a comprehensive programme of natural philosophy. In this talk my focus will primarily be on how this programme relates to her views on quantum mechanics and on (special and general) relativity. Her thoughts in this respect are for the most part contained in her 'Die Bedeutung der modernen Physik für die Theorie der Erkenntnis' ['The significance of modern physics for the theory of knowledge'].Modern physics, according to Hermann, shows us that we must give up the assumption that physics should be able to provide us with an absolute description of natural phenomena, for there is no single model of physical phenomena in space and time that can be considered to be causally determined through the interaction of extended substances. That this assumption is given up already in relativity may be seen, according to Hermann, from the fact that the bodies and forces of classical mechanics are no longer invariant concepts and are supplanted by four-dimensional invariants in the theory. It is abandoned even further in quantum mechanics, where the interpretation of any state description must always be given relative to a given context of observation.That said, the assumption of an absolute description, for Hermann, is the only assumption of the classical picture of nature that one needs to relinquish. Quantum mechanics presupposes the principle of causality in that every assignment of a state description is reducible to a previous cause, and special and general relativity presuppose the intuitions of space and time as the basis for measurements of the four-dimensional metric structure. The inclusion of these insights into a theory of experience, Hermann concludes, is a wider task which prompts one to revisit two fundamental Kantian ideas: the doctrine of a priori knowledge as a necessary presupposition of our experience, and the doctrine of the antinomies as showing us that nature cannot reveal to us a reality that is determined, in some sense, in itself. Hermann did not, in the end, carry out this project, and in the last part of my talk I will speculate regarding how she might have done so if she had.

D. Toader, Iulian

Categoricity Transfer in Quantum Theory

Abstract: If the question of categoricity is well-posed for quantum theories, one can object that the latter cannot be categorical because, like most physical theories, they incorporate first-order arithmetic, which allows non-isomorphic models and, thus, is not categorical. Moregenerally, the categoricity of a quantum theory would seem to require that all mathematical theories, including their background logic(s), incorporated as components in the quantum theory, must be categorical.I reply to this objection, by arguing that the general view assumes a questionable transfer of non-categoricity from the logical or mathematical components to the physical theory; or conversely, a questionable transfer of categoricity from the physical theory to its mathematical and logical components. Against this, I show that a provably categorical quantum theory cannot entail the categoricity of its mathematics or logic. This follows from the relationship between the global models of mathematical and logical theories, on the one hand, and the local models of quantum theories, on the other hand. To illustrate, the non-categoricity of Birkhoff and von Neumann's modular quantum logic, pointed out already by Weyl, fails to transfer to a quantum theory that includes this logic.

Da Cunha, Ivan F.

Thought Experimenting and Imagination in Neurath's Planning for Freedom

Abstract: Otto Neurath advances that in a democratic, non-technocratic, perspective of planning it is important to inform and educate people, so that the relevant decisions can be taken by the interested community. Political decisions, claims Neurath, are outside of the scope of experts. This stance derives from Neurath's epistemology of science, according to which practical directions cannot be determined by scientific knowledge. Hence the ideal situation is one in which non-experts have enough knowledge of the available plans so as to be able to discuss future paths with one another and with experts and then to make informed decisions in regard to the plans presented. In "International Planning for Freedom" (1942), Neurath observes that this democratic model might seem less efficient than one in which decisions are taken by experts in function of some systematic optimum standard. In spite of that, he argues that muddled decisions present a better outlook in the long run, as communities take a leading role in the planning process. Yet, there is another sense in which information and education are important in the process of planning and deciding, that of fueling imagination. Neurath points in that direction in "Foundations of the Social Sciences" (1944), when he claims that a serious obstacle to engineering is the limitation to invent and imagine possible solutions to given puzzles. Also, in "Die Utopie als gesellschaftstechnische Konstruktion" (1919), Neurath argues that a situation should be avoided in which people get attached to one single plan, instead of discussing and comparing groups of plans. Such passages imply that Neurath sees the need for creativity in planning – in the social-scientific effort to improve society that Neurath calls "scientific utopianism". Hence, in the Neurathian conception of planning, community education is important not only for informed decisions to be made, but also for the development of an informed or educated imaginative habit. This talk will present Neurath's scientific utopianism as a thought-experimental methodology for the social sciences in a technological context of planning. In this characterization, a contribution to the understanding of the role played by imagination in Neurath's conception of planning will be offered.

Dal Barco, Federico

Kant's Analogical Method: from the Precritical Works to Pierre Duhem and Structural Realism

Abstract: The contemporary debate on Kant's use of analogies focuses on the analogies of experience in the Critique of Pure Reason or the biological analogies of the Critique of the Faculty of Judgment (Buchdahl, 1969; Gill, 1996; Shabel, 1998; Callanan, 2008). However, Kant's precritical writings, especially those of his so-called naturalistic phase (1747-1764), offer an interestingly different conception of analogy, a use which has not been analyzed sufficiently yet and pertains to modern natural sciences. Specifically in the Allgemeine Naturgeshichte und Theorie des Himmels (1755) and in the Inaugural Dissertation (1770), Kant uses analogies as a methodological tool to interrelate the domains of physics and metaphysics, thus aiming to create a proper scientific base for metaphysics, which needed to be reformed by the philosopher. This paper aims to shed light on Kant's use of analogies in his first works to show how influential his early analogical methodology for metaphysics has been in the history of science. I claim that by conceiving metaphysics as a proper science only when it grants a symbolical-analogical knowledge of the noumena, Kant anticipated some of the most influential philosophical currents of the 19th century, for example, Pierre Duhem's proto-structural realism. The structural realist claim concerning the possibility of knowing the relations between entities through physical laws (instead of the constitutive elements of invisible phenomena) potentially comes from Kant's early metaphysics of proportional analogies. To support my thesis, I divide the paper into three sections. In the first one, I introduce Kant's precritical use of analogies by discussing his indebtedness to Newton's third law of philosophizing. As I will show, Newton's analogy of nature lies at the basis of Kant's idea of symbolical knowledge of noumena until 1770. In the second section, I deal with the Inaugural Dissertation and explain why the analogical knowledge of noumenal perfection represents one of the pivotal results of his entire precritical project. In the third part, I introduce Pierre Duhem's form of structural realism and discuss his indebtedness to Kant. In order to understand how the Prussian philosopher inspired the French physicist, it will be crucial to analyze to what extent Duhem knew and understood Kant's precritical writings. A proper analysis of the development of Kant's ideas will shed light on the history of the concept of analogy and structural realist claims. In this context, it is instructive to see how much structural realism took from a precritical conception of analogies that most Kant scholars neglect.

Damböck, Christian

Carnap in Los Angeles

Abstract: The last sixteen years of Rudolf Carnap's (1891-1970) life, spent in Los Angeles as Hans Reichenbach's successor at UCLA, are notoriously under-researched. One obvious reason for this is that Carnap did not publish much during this period. The main philosophical results of his LA years are the so-called Schilpp volume, viz. The Philosophy of Rudolf Carnap (ed. Schilpp, 1963); Philosophical Foundations of Physics (ed. Garnder, 1966); plus about six research papers in philosophy of science and inductive logic, and his contributions to the two volumes of Studies in Inductive Logic edited after his death by Richard Jeffrey (1971, 1980). Also, apart from an enormous amount of drafts for the second volume of Logical Foundations of Probability (1950), which Carnap had already planned in the late 1940s but never finished (cf. Sznajder forthcoming, who points out that these drafts hardly add many new insights to the 1950 book), there are not many manuscripts from this period to be found in the estate: a few important pieces on ethics, modal logic, and inductive logic, some of which have already been published (Carnap 2017, cf. Carus 2017). Finally, it is worth mentioning the two interviews Carnap gave to Willy Hochkeppel in 1960, which provide interesting insights into his overall philosophical stance. Nevertheless, the picture that can be drawn on the basis of his published works and unpublished manuscripts is very incomplete, because in the last two decades of his life Carnap shifted his writing activities more and more to the reports he wrote in his diary: almost half of the total material of the diary that Carnap kept over more than six decades comes from these last two decades. Carnap found it increasingly difficult to formulate his philosophical ideas in the usual form of books or articles, and instead engaged much more intensively in personal discussions with his friends, colleagues and students, imagining, as he once told David Kaplan, that these younger philosophers might then work out his ideas. Fortunately, Carnap often and extensively reports on these philosophical discussions in his diaries, and we can therefore gain direct access to the key ideas he communicated. The diaries also provide insights into Carnap's political and cultural activities and interests. In this talk, I will contextualize Carnap's published writings and the main unpublished manuscripts of his LA years by means of (a) the diaries, which have been transcribed in an ongoing edition project (the first two volumes were published in 2022), and (b) a facsimile edition of Carnap's correspondence, which has been made available online in the course of this project. These are my working hypotheses about the philosophical relevance of Carnap's writings from his LA years. While Carnap became somewhat lost in the technical subtleties of inductive logic, he developed very interesting ideas on the big picture side of his philosophy, including elaborations on his philosophy of values and the explication of human decision making qua subjective values and limited empirical knowledge.

De Groot, Jean

Archimedes' Centre of Gravity and Philosophy of Science

Abstract: The notion of centre of gravity (kentron bareos) appears for the first time in Greek science and mathematics in Archimedes' On Equilibrium of Planes (EP). The consensus has long been that "centre of gravity" is introduced by Archimedes in EP Proposition 4 (numbering of postulates and propositions as in Heiberg, ed. 1888, 1915) without its following from or even having a strong connection to what precedes it in EP 1. The references to centre of gravity in Postulates 4, 5 and 7 also presuppose the notion. Either Archimedes introduced centre of gravity in a treatise now lost (possibly Peri zugōn), or the notion had been elaborated before him by someone else. There is no decisive evidence for either alternative, as Dijksterhuis expertly concluded. My paper makes the case for centre of gravity as a product of the mathematicised framing of the equilibrium postulates 1–3 (P1–3) and Props. 1–3. Within this framework, there is a straightforward path from the laws of equilibrium in EP 1 to the idea of weights having a "centre" in a single point. Archimedes may also have considered the notion to be of a piece with the substitution postulate (EP P6) and the basic and fruitful idea that the centre of weight (i.e., balance) of two magnitudes is on a straight line between them (Heiberg 1888, 148.8–12). Reading baros as weight is important for understanding Archimedes' reasoning. My argument is not entirely new. Some earlier commentators draw upon P6 (e.g., Child, Dijksterhuis), and others highlight the idea of there being a centre of weight of two magnitudes on a straight line between them (Vailati, Hölder). My paper presses these interpretations further, seeking the philosophy of science implicit in Archimedes' use of centres of gravity in mechanics and geometry. I argue that the opposition of the empirical and logic (e.g., Mach, Palmieri) distorts Archimedes' reasoning in the early propositions of EP 1. I refer to recent analyses of Vailati and Hölder written by van Dyck and Schlaudt respectively.ReferencesChild 1921. 'Archimedes' Principle of the Balance and Some Criticisms Upon It.' Studies in the History and Method of Science, ed. Charles Singer, 2 vols. Oxford. Vol. 1, 490–520.Dijksterhuis 1987. Archimedes. Princeton.Hölder 1924. Die Mathematische Methode. Springer.Mach [1893]. Science of Mechanics. Open Court.Palmieri 2008. 'The empirical basis of equilibrium: Mach, Vailati, and the Lever.' Studies HPhS 39: 42–53.Schlaudt 2013. 'Hölder, Mach, and the Law of the Lever: A Case of Well-founded Non-controversy.' Philosophia scientiae 17 (1): 93–116. Vailati, Giovanni 1911 [1897]. 'La dimostrazione del principio della Leva data da Archimede nel libro primo sull'equilibrio delle figure piane.' Scritti CVI, Leipzig: Barth, 497–501.Trans. Palmieri 2009. 'Response to van Dyck,' Studies HPhS 40: 319–321._____ 1911. 'Del concetto di Centro di Gravità nella Statica di Archimede.' Scritti XX, 80–90.Van Dyck, Maarten 2009. 'On the epistemological foundations of the law of the lever.' Studies HPhS 40: 315–318.

De Paz, María
María J. , Gutiérrez-Márquez

Hesse's modified realism and science as a process

Abstract: From the 1960's onwards, there are significant critical voices in the philosophy of sciences against the dominance of positivism and traditional realism. These introduced new elements to consider, such as the role of the history in science or the relevance of the scientific community. Mary Hesse's work is part of this reconsideration, whose new realist approach departs from the more traditional conceptions, although she rarely appears in general histories of philosophy of science or even in handbooks. In this talk we will address her moderate approach to realism, named as modified realism. With this proposal Hesse distances herself from logical positivism, from Popper's critical realism and also from Kuhn's conception. However, in spite of this critical attitude, she does not suggest an absolute rejection either of the traditional theses, or of those derived from positivism, or of those proper of the 1960's in philosophy of science. Among the problems she considers, there is the idea whether, within science, a disruptive or a cumulative view should be chosen in order to account for scientific activity (Hesse, 2022: 283).We will therefore first look at her realism as compared to traditional views. This will lead us to consider whether there is a middle ground between the extremes of scientific realism and relativism in her philosophy. To tackle this issue, we will thus approach her notions of truth (Hesse, 1970), and of scientific progress (Hesse, 1992). In our approach, we will consider a differential element transversal to all of Hesse's theses: the role of language. Through her network model it will be possible to establish both the influence of Duhem's and Quine's holism as well as the resonance of her ideas in later authors. Thus, the analysis of modified realism will allow us to situate her philosophical conception within the different possible considerations of science: either as a process or as a product. Hesse's proposal confronts naive positions which caricature or ignore scientific practice. We consider that in the light of her modified realism, a dynamic view within the different elements that conform scientific practice can be grasped. The purpose is to devise an account of science different from the incomplete views provided by some of the classical protagonists of 20th century philosophy of science. Hesse's views encompass both traditional scientific procedures and the new features of the philosophical turn of the 1960s, hence, her position is particularly relevant within 20th century philosophy of science. But also, given that she treats science as historical, social, and pragmatic, without forgetting its empirical character, it is a position worth of serious consideration for the development of 21st century philosophy of science.ReferencesHesse, M. B. (1980). Revolutions and Reconstructions in the Philosophy of Science. Bloomington: Indiana University Press.Hesse, M. B. (1992). 'Science beyond Realism and Relativism' in Cognitive Relativism and Social Science (Edited by D. Raven; L. van Vucht Tijssen; J. de Wolf). New York: Routledge. Hesse, M. B. (1974). The structure of scientific inference. California: University of California Press.

Del Sordo, Caterina

Questions of Realism in Carnap's Quasi-Analysis

Abstract: Certain epistemological, historical-philosophical and technical-scientific aspects of the received view of logical empiricism have undergone a profound reassessment over the past decades. This field has witnessed a reappraisal of quasi-analysis, the formal method developed by Carnap in the early 1920s and applied in Der logische Aufbau der Welt. This paper is intended to contribute to its re-evaluation by referring to Carnap 1922, 1923 and 1928 in parallel. Its aim is to sketch out the question of realism as it emerges in Proust 1989's objection to Goodman 1951 (see also Mormann 2009) and, retrospectively, in the genesis of quasi-analysis in the early Carnap's writings.The paper is divided into the following sections. The first two sections introduce Carnap's quasi-analysis from a scientific and philosophical point of view. The first section shows that the quasi-analysis of Carnap 1923 has a formal structure analogous to mathematical theorems for lattice representation. In the second section, it is shown that the three above-mentioned early texts by Carnap embed quasi-analysis within different object-networks, which can be regarded as akin to life-philosophy in Carnap 1922 (Mormann 2016), as phenomenological-perceptual in Carnap 1923 and as structural in Carnap 1928. The third part of the paper delves into Goodman's critique of quasi-analysis. From the axiomatic point of view, the quasi-analysis discussed by Goodman 1951 mirrors the model of Carnap 1928, while it mirrors the model of Carnap 1923 from the point of view of the phenomenological-perceptual object-network in which it is embedded. The idea is introduced that Goodman's critique of Carnap's project of rational reconstruction erroneously assumes positions of absolute realism with respect to the world which quasi-analysis aims to reconstruct (cf. Proust 1989, pp. 192-3).The fourth section dwells on Carnap 1922 and Carnap 1923, showing that the manuscripts present assumptions of relative, or empirical, realism. The paper ends by approaching the debate about the status of contemporary analytical metaphysics, illustrating how the debate on quasi-analysis yields what is at most a deflated view of ontology, based on the global-local opposition rather than the absolute-relative one.Essential bibliography:Carnap, R. (1922). Vom Chaos zur Wirklichkeit, Unpublished Ms., Archive of Scientific Philosophy, Special Collections Department, Hillman Library, University of Pittsburgh, RC-081-05-01. Carnap, R. (1923). Die Quasizerlegung – Ein Verfahren zur Ordnung nichthomogener Mengen mit den Mitteln der Beziehungslehre, Unpublished Manuscript, Archive of Scientific Philosophy, Special Collections Department, Hillman Library, University of Pittsburgh, RC-081-04-01. Carnap, R. (1928). Der logische Aufbau der Welt, Weltkreis.Goodman, N. (1951). The Structure of Appearance, Harvard University Press.Mormann, T. (2009). New Work for Carnap's Quasi-Analysis, «Journal of Philosophical Logic», 38, pp. 249-282.Mormann, T. (2016). Carnap's Aufbau in the Weimar Context, in C. Damböck (ed.), Influences on the Aufbau, «Institute of the Vienna Circle Yearbook», 18, pp. 115-137.Proust, J. (1989). Questions of form, logic and the analytic proposition from Kant to Carnap, University of Minnesota Press.

Demarest, Boris

Canguilhem on Science and its Place in Life

Abstract: In this talk, I argue that Canguilhem's overarching philosophical project as developed in Le normal et le pathologique and La connaissance de la vie is best understood as what I will call a philosophy of life. In his recent book, Beiser (2023: 29) primarily characterizes philosophy of life as "less a metaphysics about life than an ethics about how life should be led", and situates it in the period 1870-1920. I first argue that philosophies of life span a larger period and context, and that they involve 1) a central conception of life that is used to develop 2) a conception of human life as a specific manifestation of life and 3) a conception of science as a specific manifestation of human life. I will also suggest that this makes philosophies of life naturalist in the sense that they seek to understand human life through life in general, and to understand science as grounded in humans as living beings. Their naturalism is heterodox, however, because in contrast to more typical biologistic and psychologistic naturalisms, they seek to ground the normativity found throughout human life in nature without reducing it to a non-normative aspect of nature.On the basis of Canguilhem's early (co-authored) traité de logique et de morale, I will argue that we find all the features of a philosophy of life as characterized above in Canguilhem in the form of his distinct picture of life as fundamentally normative, his conception of technology as primarily a manifestation of life, and his conception of science as emerging from yet discontinuous with technology. I also argue that Canguilhem's interest in the history and philosophy of the life sciences and of medicine is at least partly motivated by his conviction that this history reveals a tension between science and technology as practices. In addition, I build on work on Canguilhem's philosophy of biology (such as that found in Dagognet 1997, Gayon 1998, Lecourt 2008, and Bianco, Wolfe & Van de Vijver 2023), to show how Canguilhem sought to ground human practice in biological normativity as a fundamental feature of reality. In conclusion, I briefly show how my interpretation impacts on the pictures of Canguilhem's method and his place in the tradition of historical epistemology found in Lecourt 1972 & Chimisso 2003.Beiser, F. (2023). Philosophy of Life. German Lebensphilosophy 1870-1920. Oxford: Oxford University Press.Bianco, G., Wolfe, C. & Van de Vijver, G. (2023). Canguilhem and continental philosophy of biology. Dordrecht: Springer.Chimisso, C. "The tribunal of philosophy and its norms: history and philosophy in Georges Canguilhem's historical epistemology", Studies in History and Philosophy of Biological and Biomedical Sciences 34: 297-327.Dagognet, F. (1997). Georges Canguilhem, philosophie de la vie. Paris: Empêcheurs de penser en rond.Gayon, J. (1998). "The Concept of Individuality in Canguilhem's Philosophy of Biology. Journal of the History of Biology 31: 305-325.Lecourt, D. (1972). Pour une critique de l'épistémologie (Bachelard, Canguilhem, Foucault). Paris: Maspero.Lecourt, D. (2008). Georges Canguilhem. Paris: Presses Universitaires de France.

Demarest, Boris
Hein, Van Den Berg

Medicine as Proper Science in the 18th century

Abstract: In this paper, we discuss 18th century attempts to transform medicine into a proper science rather than a mere art and to put an end to constant dispute and the threat of quackery by drawing on a Newtonian conception of proper science. Van den Berg and Demarest (2020) have argued that key contributors to the constitution of German biology explicitly drew on Wolffian and (post-)Kantian ideals of proper science in providing biology with scientific status. They also argued that these ideals of proper science are versions of the classical ideal of science (see de Jong & Betti 2010) on which a science consists of fundamental principles and statements demonstrated from these fundamental principles, and of fundamental concepts and concepts defined in terms of these fundamental concepts. We show that some other 18th century medical authors drew on Newtonian conceptions of proper science to transform medicine into a proper science, and, against Zammito (2018), that Newtonianism in the 18th century life sciences is not mostly a theory-averse experimental and empirical outlook, but also involves a quest for scientific demonstrations and systematic theories. We first articulate the methodological views of the so-called "medical Newtonians", to which Guerrini (1985, 1986, 1987, 1989), in her important studies on these figures, pays relatively little attention. We argue that they explicitly sought to transform medicine into proper science by drawing on a Newtonian version of the classical ideal of science on which mathematics plays a crucial role in providing the certainty required of proper science. We then discuss Christian Wolff's views on the transformation of medicine into proper science, which, as Favaretti Camposampiero (2016) has noted, are influenced by the medical Newtonian Pitcairne. We argue that Wolff's adoption of the classical ideal of science informs his views on medicine, and, building on van den Berg (2023), that Wolff's methodological views were adopted by de Sauvages.De Jong, W.R., and Betti, A. (2010). "The Classical Model of Science: A Millennia-old Model of Scientific Rationality". Synthese 174 (2): 185–203.Favaretti Camposampiero, M. (2016). "Bodies of Inference: Christian Wolff's Epistemology of the Life Sciences Medicine". Perspectives on Science 24: 361–379.Guerrini, A. (1985). "James Keill, George Cheyne, and Newtonian physiology, 1690–1740". Journal of the History of Biology 18: 247–266.Guerrini, A. (1986). "The Tory Newtonians: Gregory, Pitcairne, and their circle". Journal of British Studies 25(3): 288–311.Guerrini, A. (1987). "Archibald Pitcairne and Newtonian medicine". Medical History 31:70–83Guerrini, A. (1989). "Isaac Newton, George Cheyne and the 'principia medicinae'". In: French, R., Wear, A. (eds) "The medical revolution of the seventeenth century". Cambridge: Cambridge University Press: 222–245.van den Berg, H. & Demarest, B. (2020). "Axiomatic Natural Philosophy and the Emergence of Biology as a Science". Journal of the History of Biology 53: 379–422.van den Berg, H. (2023). "The Essentialism of Early Modern Psychiatric Nosology". History and Philosophy of the Life Sciences 45(2).Zammito, J.

(2018). The Gestation of German Biology: Philosophy and Physiology from Stahl to Schelling. Chicago: University of Chicago Press.

Descarreaux, Samuel

A case study in Naturalized Epistemology: Helmholtz and Poincaré's Thoughts on Geometry.

Abstract: In this talk, I provide elements of definition for "Naturalized Epistemology" and evaluate if they apply to Helmholtz and Poincaré's thoughts on geometry and space. Contemporary commentators have frequently credited Helmholtz for putting together a naturalized epistemology Avant l'heure (Hatfield (1990), De Kock (2012), Patton (2018)); the same has not been attempted for Poincaré. I demonstrate in a two-part argument that Poincaré's thoughts on geometry and space are compatible with arguments found in Helmholtz's "Naturalized Epistemology". Historically, both thinkers have addressed the same problem: defining the geometrical relations in space and providing useful applications to physical objects. To this end, Helmholtz relied on the mobility of the rigid natural body and the notion of congruence to establish the real conditions for the measurement of physical space. In contrast, Poincaré opts for a classification of group displacement in geometrical space that is neither true nor false conventions indirectly guided by experience (or sensation in motion).Despite this debate, I suggest that both thinkers shared a common scientific unregenerated realism and epistemological holism, typically associated with "Naturalized Epistemology". Both do not look outside of the modes and methods of explanations of natural sciences to justify mathematical propositions; the methodological continuity between epistemology and natural sciences allows Helmholtz to construct spatial geometries out of a psychophysiological interpretation of spatial perception and Poincaré to support his reconstruction of spatial geometry with a psychophysiological genesis. Also, both fall back on holistic criteria (ex. commodity, simplicity) to overcome the empirical equivalence of different types of geometry. Helmholtz embeds the geometrical axioms in a system of actual physical measurement (as sufficient preconditions) while Poincaré's geometrical conventions take part in the generalization process along verifiable physical hypotheses which have observational (thus true or false) consequences. That said, I argue that Helmholtz and (more explicitly) Poincaré used methodological structuralism to account for the validity of geometrical relations. However, this interpretation could be at odd with the previous "Naturalized Epistemology" label. The structural relations (described by geometry) provide a norm of invariance that must be conventionally adapted and empirically exemplified (Heinzmann & Stump, 2021). Yet, it seems this structuralism allows the relations an epistemic apriority over the modes and methods of explanations of natural sciences which could be prohibited by "Naturalized Epistemology". In response, I conclude that the Helmholtz/Poincaré debate on geometry could be interpreted as a debate from within scientific practices (or more precisely from within mathematical practices). Hence, I suggest that Helmholtz and Poincaré's methodological structuralism is compatible with a moderate version of "Naturalized Epistemology".

Descarreaux, Samuel

Hintikka on Kant's Mathematical Intuition and its relevance for Diagrammatic Inferences in the 19th Century.

Abstract: In this talk, I show that Friedrich-Albert Lange's (1828-1875) use of intuitive diagrammatical demonstration to bridge Kant's distinction between the pure logical functions of judgment and mathematical principles corresponds to Jaako Hintikka's interpretation of intuition's role in Kant's mathematical demonstration. I contend that, despite the century-long gap, this alignment helps in understanding the logic implications underlying Kant's mathematical demonstration and the type of justification provided by such intuitive constructivism. Hintikka and Charles Parsons debated the features and use of non-conceptual intuition in Kant's mathematical demonstration throughout the 1960s. Because it was based on direct experience with immediately knowable mathematical objects, Hintikka rejected Parsons's phenomenal interpretation. He argued for a logical interpretation based on inferences drawn from the individually instantiated constructions of a general concept analogous to the application of the logical principle of existential instantiation. This analogy, traced back to Euclid's geometrical demonstrations and Aristotle's marginal ecthesis proof in Prior Analytics, allowed him to emphasize the purposeful creation of mathematical objects rather than postulating unexplained abstract mental beings. However, Kant's criticism against existential implication in syllogistic reasoning presents a challenge to Hintikka's perspective. However, it captures accurately Friedrich Albert Lange's ideographical attempt at conceptualizing and representing the inclusion and exclusion relations between classes of objects. To infer necessity from individual constructions, Lange uses Aristotle's ecthesis proof and logical diagrams that remain identical despite the infinite variations in shape, position, or size. Thus, this argument helps us understand the relationship between existential implication in logic and mathematical demonstration from a Kantian perspective.

Dewulf, Fons

Oral HOPOS: Project Overview and Initial Results

Abstract: In writing the history of philosophy of science, personal recollections (e.g. of Carl Hempel or Thomas Kuhn) have proven useful to ascertain information about an actor's intellectual development, their network and socio-political orientation. However, such recollections have so far not been gathered in a systematic way, and are often only available for high-status philosophers. In order to proliferate both the scope and quality of such recollections for the second half of the twentieth century, I have initiated an oral history project, interviewing philosophers of science from the English-speaking world, initially focusing on those trained in the 1950s and 1960s. In this paper, I first lay-out the methodology and future plans of the project. Next, I show the potential historiographical value of this project by discussing one insight that the initial round of interviews has already yielded. I will also present a list of prospective interviewees in order to solicit suggestions from the audience.The aim of the project is to create a catalogue of first-hand recollections of the intellectual and institutional climate in which post-war US philosophy of science developed out of the various available philosophical traditions in the 1950s and 1960s. In order to create a working template that can also be upscaled to adjacent disciplines and periods, the questionnaire for each interviewee was built up around the same eight themes. These focus on the social and intellectual developments spanning the career path shared by the interviewees in the second-half of the twentieth century. These themes probe the relationship between philosophy, science, and adjacent disciplines like history of science in the context of philosophy of science's increased specialization from the 1960s onwards. The awareness and struggle for diversity in academic philosophy is also thematized, alongside the debate whether philosophy/academia should play a public role.So far, I have interviewed Bas van Fraassen, Clark Glymour, Peter Achinstein, Peter Asquith, Noam Chomsky, Michael Scriven and Nicholas Rescher. Although the information contained in these initial interviews is too rich to give any overall interpretation, I will discuss one notable insight to be gained from them. Many of the interviewees saw Carnap, Reichenbach or Hempel as exemplars for their own work. However, the way in which these exemplars were understood and played a role in the meta-conception of philosophy heavily diverged. This was often related to the very specific intellectual context in which the interviewees initially engaged with logical empiricist texts. E.g., inspired by the same works of Hans Reichenbach, van Fraassen and Glymour initially evaluated the importance of a historical perspective on philosophy and science in opposite directions. This divergence is related to their earliest responses to the phenomenological tradition. For Achinstein, Rescher and Chomsky, who were taught in the high-status universities of Harvard and Princeton, ordinary language philosophy from Oxford was an important source of inspiration to expand philosophical interests beyond logical empiricism. In contrast, ordinary language philosophy was of little importance for those interviewees who came from lower status universities.

Dobre, Mihnea

Cartesian Democritus: the image of Democritus in the works of the first Cartesians

Abstract: It might seem surprising that three leading Cartesians devote chunks of their work to a discussion of Democritus. This paper will examine the Democritean image in the works of Jacques du Roure, François Bayle, and Nicholas Joseph Poisson. For du Roure, Democritus is an atomist philosopher whose theory is examined in a dedicated section of La Physique of 1653. François Bayle refers to Democritus in connection to experimental practice, and Poisson mentions him in the context of a broader argument against the accusation that Descartes has borrowed most of his views from the ancients. By looking at the various answers provided by the Cartesians while facing different philosophical challenges raised by their contemporaries, this paper aims to outline a variety of Cartesian solutions. I argue that examining this reception of Democritus among Cartesians provides a better understanding of the varieties of early modern forms of Cartesianism, shedding light on questions related to matter theory and methodology, but also about reading practices and the use of authorities in arguments developed by the new philosophy.

Dong, Hao

Christian Mysteries and Natural Philosophy in Leibniz

Abstract: As a devoted Christian, Leibniz deems it essential to defend the possibility of mysteries such as the Eucharist throughout his career; however, it proves to be a difficult task to do so while preserving the mysterious character of the mysteries. In this paper I argue that, first, the philosophical system of the mature Leibniz could accomplish this task better than that of the young Leibniz; second, although for Leibniz mysteries are strictly speaking outside of the natural order, they still exert considerable influence on Leibniz's natural philosophy, even in his mature system.I begin by describing the structure of the philosophical systems of the young and the mature Leibniz. On the one hand, these systems are similar in that both consist of an abstract part where necessary truths demonstrated from primitive definitions, and a concrete part that contains contingent propositions. On the other hand, they are different insofar as in the early system all contingent propositions cannot be demonstrated a priori, while in the mature system some of them can, thanks to the emergence of a group of contingent 'architectonic' principles that inform the optimal natural order of the world. This difference leads to the consequence that in the mature system there are propositions which, while implying no self-contradiction, violate the architectonic principles hence the natural order; by contrast, in the early system all propositions that cannot be demonstrated as necessary truths are of the same kind. Thus, the way in which the young Leibniz defends the possibility of mysteries is often modelled on the way in which he explains natural phenomena, that is, by proposing hypotheses constrained by the necessary laws of nature that could produce the mysteries/natural phenomena in question (A VI.1, 514-15). But in doing so the young Leibniz is essentially assimilating mysteries with natural phenomena. By contrast, the mature Leibniz has the intellectual resource to pinpoint the difference between mysteries and natural phenomena: the latter can be demonstrated a priori from metaphysically necessary truths plus architectonic principles that are morally necessary (Theodicy, Preliminary Dissertation, §§2-3), but the former cannot.Then I discuss the intricate relationship between mysteries and natural philosophy in these two systems. First, it should come as no surprise that in Leibniz's early system natural philosophical theories are often used to defend the possibility of mysteries, since the distinction between mysteries and natural phenomena is blurred. Turning to Leibniz's mature system, I show that Leibniz still has the explanation of mysteries in mind when he develops his natural philosophical theories (e.g., in his account of the substantial bond in the Des Bosses correspondence). I argue that this is mainly because it is unclear to the mature Leibniz which contingent truths can be demonstrated a priori. In other words, it is possible that a relatively complete theory of reality must include inexplicable mysteries, such as mind-body union. Thus, although in theory mysteries are excluded from natural philosophy, in practice they are still often treated as a whole.

Dougherty, John

Sellars and The Scientific Image

Abstract: The title of Bas C. van Fraassen's The Scientific Image is a nod to Wilfrid Sellars, who appears in the book as "one of the leaders of the return to realism in the philosophy of science" (p. 31). Van Fraassen's goal in the The Scientific Image is to resist this return-while acknowledging the criticisms of earlier antirealisms that motivated it-by developing a new, constructive form of empiricism. However, despite the significant impact of The Scientific Image on the realism debate and recent renewed interest in Sellars's scientific realism, little attention has been paid to Sellars's influence on van Fraassen's constructive empiricism and van Fraassen's criticisms of Sellarsian realism. My goal in this paper is to reconstruct van Fraassen's replies to Sellars's arguments for scientific realism and the relation of these to van Fraassen's positive position.This reconstruction is meant to address four questions. The first pair arises because Sellars is a notoriously systematic philosopher: his scientific realism is enmeshed with his views about language, modality, ethics, and mind. But in The Scientific Image, van Fraassen is concerned to defend constructive empiricism against scientific realism broadly, so engagement with the details of any one view are not on the itinerary. Is Sellars's view correctly captured by van Fraassen's broad characterization of realism? If so, does van Fraassen take issue with a specific piece of the Sellarsian system, or reject it wholesale?Two more questions arise when we look elsewhere for more detailed engagement between Sellars and van Fraassen. The most prominent such engagement was a symposium at the 1976 meeting of the Philosophy of Science Association (PSA) on the tenability of scientific realism, at which van Fraassen commented on Sellars's presentation of a reply to his critics. Here van Fraassen offers a detailed reconstruction and criticism of Sellars's argument for realism. But interpreting this exchange poses a problem opposite that of The Scientific Image: in his criticism, van Fraassen explicitly avoids setting out his positive view. Moreover, Sellars denies that van Fraassen's reconstruction is apt. Is he correct? Appealing to further sources also raises a diachronic question: do van Fraassen's criticisms of Sellarsian realism change between the PSA symposium and The Scientific Image?My reconstruction underwrites the following answers to these questions. Van Fraassen's characterization of scientific realism involves various placeholder terms, which his opponents will articulate in different ways. On a Sellarsian understanding of these placeholders, van Fraassen's framing of the realism debate becomes precisely Sellars's, with constructive empiricism a form of "sophisticated" instrumentalism. And the difference between the two becomes a disagreement over the nature of explanation-specifically, a disagreement over whether explanations supply modal information. Van Fraassen's early criticisms of Sellars, discussed at the 1976 PSA, misconstrued this disagreement. But by 1980, van Fraassen had adopted a theory of explanation that supplies grounds for a reply to Sellars's argument for realism.

Dresow, Max

The road not taken: reconstituting Stephen Toulmin's philosophy of science in practice

Abstract: In recent work, Paul Franco has begun to explore the intersection of mid-twentieth century ordinary language philosophy and philosophy of science. The intersection has been greatly neglected. Despite some remarks by Thomas Kuhn, philosophers have not typically regarded ordinary language philosophy as a contributor to philosophy of science. Instead, post-war philosophy of science is usually pictured as a battleground between logical empiricists and their historicist critics, including Kuhn, N.R. Hanson, and Paul Feyerabend. Franco argues that this picture has obscured the role that philosophers influenced by ordinary language analysis played in the field's "historical turn." As examples, he lists Stephen Toulmin, Michael Scriven, and Mary Hesse. In Franco's view, the focal problems for these philosophers "[were] part and parcel of the historical turn, as well as its reception." The implication is that, by attending to these neglected contributions, we can better understand why philosophy of science developed as it did in the decades following World War II. In this talk, I build on Franco's work by further articulating the methodology and orienting assumptions of Toulmin's philosophy of science. I do this through a close reading of two crucial texts, The Philosophy of Science: An Introduction (1953) and Foresight and Understanding (1961). I argue that while Toulmin was influenced by the ordinary language tradition, it is more useful to regard him as a specifically Wittgensteinian philosopher of science. (Franco's account suggests this characterization as well.) Several of Toulmin's contributions are fairly direct developments of claims put forward in the Tractatus (e.g., Proposition 6.342). This is most evident in Toulmin's unorthodox treatment of scientific laws and their inferential function. But the more pervasive influence comes from Wittgenstein's later philosophy, with its mistrust of idealization and its focus on "forms of life" as historically contingent "givens." It is these commitments that give Toulmin's philosophy its distinctive features. Yet the commitments do not steer him toward a preoccupation with scientific language and theories of meaning. Instead, his concern is to understand how scientific knowledge is possible, and his method is to explore scientific thought in action, and in relation to those "givens" that function as enabling conditions for patterns of activity.What is the value of reconstituting this (mostly) forgotten philosophy of science? I argue that, far from illuminating how philosophy of science actually developed during the twentieth century, Toulmin's philosophy is more useful for drawing attention to an unrealized possibility: a road not taken. Philosophy of science in the post-war period was decidedly not Wittgensteinian, at least in the sense exemplified by Toulmin's work. And while philosophers eventually found their way back to topics like representation and scientific understanding, very little of this work was influenced by Toulmin or his philosophical assumptions. The road actually taken may bear the stamp of ordinary language analysis; but of Toulmin's philosophy of science in practice, very little survived.

Du Crest, Agathe

Understanding behavior from an evolutionary perspective: a history of the diverse generalizations of Darwinism

Abstract: When he published The Descent of Man in 1871, Darwin proposed to extend his hypothesis of evolution by natural selection to the human species and its characteristic features, from the development of moral and intellectual faculties to language, and he even hinted at the historical change of « civilizations » (explaining the rise and fall of societies). According to his idea of continuity, i.e. of gradual and non-essential differences between human beings and other animal species, cultural characteristics could thus be explained in a similar way to biological traits.The next hundred and fifty years saw the emergence of numerous attempts to generalize Darwinism to the sociocultural behaviors of organisms, in particular for the human species. These attempts sometimes came from social sciences, particularly anthropology. Between the early 1970s and the first half of the 1980s, several competing visions of such a generalization took shape, this time from evolutionary sciences. In this presentation, I will focus on this period, in order to illustrate how, on the one hand, the systematic idea of a generalization of Darwinism emerged, and on the other, research programs that interpreted Darwinism very differently, and that were to encounter antagonistic fates.First, I will trace the birth and development of theories of cultural evolution, around the texts of Cavalli-Sforza and Feldman from the founding publications of 1973 to their 1981 monograph, showing how their thinking is based on an internal critique of population genetics, from which they came. This story continues with Boyd and Richerson, who developed these mathematical formalizations of cultural evolution. I will then show how this approach is based on an interpretation of Darwinism very different from that of sociobiology, which emerged at the same time under the pen of Wilson (1975). The consequences of this divergence remain profound today. Indeed, theories of cultural evolution have given rise to a range of approaches such as Dawkins' memetics (1976, 1982), Sperber's cultural epidemiology (Claidière, Scott-Phillips, Sperber 2014), Mesoudi's selectionist approach (2011) or Lewens' kinetic theory (2015). Evolutionary psychology took over from sociobiology in the 1990s (Barkow, Tooby, Cosmides, 1995). While theories of cultural evolution agree to reconfigure the Darwinian evolutionary theory by taking into account specifically cultural processes of variation, hereditary transmission and selection, sociobiology and subsequent evolutionary psychology are almost literal extensions of Darwinism, in which the change of explanandum does not imply any major modification of the explanans, which is both based on processes of evolution by natural selection, and gives major importance to the principle of selection over variation and heredity.Finally, I'll look at the idea of systematizing Darwinism, under a variety of names ranging from "Universal Darwinism" formulated by Dawkins in 1983, to the more recent "Generalized Darwinism" (Du Crest et al. 2023). It will be useful to clarify what we can expect from these attempts, in terms

of explanatory power of Darwinism, and to wonder whether a version of Generalized Darwinism would make possible a synthesis between the two approaches drawn.

Ducheyne, Steffen

Physica at the University of Leiden, 1675-c.1721: The Creation of an Autonomous Discipline

Abstract: In this essay, I trace some features of the development of physica at the University of Leiden between 1675-c.1721. I shall consecutively focus on the work of Burchard De Volder (1643-1705), Wolferd Sengerd(ius) (1646-1724), Herman Boerhaave (1668-1738), and Willem Jacob 's Gravesande (1688-1742). It will be shown that, although physica in the period under consideration needed to comply to a fixed number of desiderata that emerged in the wake of the Cartesianismusstreit, its meaning, methods, and aims were in constant flux. Experimental and mathematical physics—'physica' or 'natuurkunde' in the words of the protagonists of the present story—did not arrive overnight at the University of Leiden which was founded in 1575. The arrival of physica became highly visible with the opening of the Theatrum physicum, exactly a century after the foundation of the university. For its establishment, the theatrum was greatly indebted to Burchard de Volder (1643-1705).In the first part of my talk, I argue that, according to De Volder, the central aim of physica is to discover which material mechanisms, that are derivable from the a priori obtained clear and distinct notions of matter and motion, produce certain observed effects. In other words, these a priori obtained notions are taken for granted and cannot be challenged by experimental or observational data. For this reason, we might consider De Volder's experimentalism a limited form of experimentalism, clearly set within the bounds of certain a priori notions.In the second part of my talk, I document how experiments became gradually more important to De Volder's successor Senguerd. Furthermore, I argue that some of Senguerd's natural philosophical beliefs provided fertile ground for a favourable reception of Isaac Newton's natural philosophy. Senguerd showed that physica could be harmonized with religion, viz. Calvinist orthodoxy. Also, his conception of occult qualities as confessions of ignorance could facilitate acceptance of Newton's theory of universal gravitation, which notoriously remained silent on the cause of gravity. Furthermore, his criticism of 'feigning' how nature works could also be seen as compatible with Newton's famous attack on hypotheses. Finally, Senguerd's admiration of Bacon can be seen as a form of Anglophilia that promoted the work of English natural philosophers.In the third part of my talk, I argue that in a manuscript that have not received the attention it deserves Boerhaave for the first time came to believe that physica needs to incorporate features of Newton's natural philosophical methods, especially his epistemic modesty as exemplified by the latter's stance on the cause of gravity. Boerhaave thought that by following Newton's method physica could produce certain knowledge and free natural philosophers from natural philosophical sectarianism.In the final part of my talk, I show how 's Gravesande radicalized some features of Newton's methodology and that in doing so he made a relatively sharp distinction between physica and philosophy as a result of which physica became an

autonomous discipline at the University of Leiden. My talk will take into account recent work by for example Nyden, Strazzoni, and van Bunge.

Dunham, Jeremy

William James, François Pillon, and Charles Renouvier on the Uncritical Assumptions of the Natural Sciences

Abstract: The dedication of William James's 1890 Principles of Psychology reads: "to my dear friend François Pillon, as a token of affection, and as an acknowledgement of what I owe to the Critique philosophique".[1] Why did James dedicate his masterwork to this now largely forgotten French philosopher? During the 1870s, primarily in their own journal La Critique philosophique, François Pillon and Charles Renouvier argued that there is a sharp distinction between the sciences and philosophy.[2] They argued that although philosophy must investigate its own foundations, the sciences begin their development by assuming a set of foundational beliefs and logical procedures that they themselves do not examine. The scientific community accepts them as 'conventions' that must be accepted in order to proceed in their proper work.[3] In this paper, I argue that this understanding of the distinction between philosophy and the sciences motivated James to ground psychology as a natural science, and distinguish it from metaphysics, by means of the "uncritical assumption" of three postulates: the existence of (1) thoughts and feelings, (2) the physical world; and, (3) our ability to know (2) by means of (1). James, therefore, owed to Pillon, Renouvier, and La Critique philosophique the very understanding of the principles of science that allowed him to get his project of establishing psychology as a natural science off the ground. This context is important because it helps to elucidate some of the most confusing aspects of James's writings. As Wesley Cooper has argued, we can avoid attributing many paradoxes to James if we understand that his writings operate at two different levels: philosophical level and the empirical level.[4] He calls this the Two Levels View. In some of James's works he is writing from the position of the philosopher, in others, from the position of the natural scientist. In this paper, I argue that an examination of the source of the Two Levels View, i.e., Pillon's and Renouvier's writings on philosophy and the sciences, gives us a better understanding of the view itself and of the philosophical beliefs James held when the Principles was published. It also gives us good reason to be sceptical of Matthias Girel's recent claim that James had already captured all that he needed from Renouvier and La Critique philosophique by the early 1880s, as well as making it clearer where their views converged and diverged at this time[5]. [1] James, William (1890 [1981]) The Principles of Psychology. Cambridge, Mass: Harvard University Press[2] See, for example, Pillon, François (1873) "De la fondation de la philosophie comme science". La Critique philosophique, 1: 289-304 and Renouvier, Charles (1873) "Proposition d'un nouveau critère de certitude". La Critique philosophique. 2: 195-201)[3] See Schamus, Warren (2018) Liberty and the Pursuit of Knowledge. Pittsburgh: University of Pittsburgh Press [4] Cooper, Wesley (2002) The Unity of William James's Thought. Nashville: Vanderbilt University Press [5] Girel, Matthias (2022) "William James and Renouvier's Neo-Kantianism". Oxford: Oxford University Press [online first]

Dunlop, Katherine

Definitions and Constructive Procedures in Kant's Philosophy of Mathematics

Abstract: Kant's account of mathematical cognition has two main aims: to highlight the differences between mathematical and philosophical thought, and to explain mathematics' applicability to experience. Among the most important of the differences between mathematical and philosophical thought is that mathematical cognition is based on definitions. Kant claims that mathematical definitions are "real", meaning that they show the concepts' objective reality, specifically by "exhibit[ing] the object in accordance with the concept in intuition" (A242n.); in contrast, the so-called definitions of philosophical concepts are at best nominal. Kant's remarks in the Critique of Pure Reason's "Schematism" and "Discipline of Pure Reason" chapters drive home that mathematical concepts already incorporate the means to exhibit their objects in sensible intuition, whereas other a priori concepts must be provided with such means.As if to explain how definitions can serve to exhibit corresponding objects, Kant claims that definitions "come about as" (A730/B758), and even that they are (11:42-3), constructions of concepts. Interpreters seeking to explain the special features of mathematical definitions (Heis 2014, Nunez 2014) maintain that a mathematical definition includes or expresses a canonical procedure for constructing objects that fall under the defined concept. This view nicely explains how the definitions can prove the concepts' objective reality. But it is difficult to reconcile with remarks of Kant's which suggest that a given concept can be constructed by diverse procedures, and, more gravely, with the mathematical practice that informs these remarks. Kant adheres to a traditional distinction between "geometrical" constructions, by means of straight lines and circles, and "mechanical" constructions, by means of conic sections and other curves. Building on (Laywine 2014), I argue that Kant's view is influenced by developments in geometry, in particular the theory of conic sections. While Laywine brings out the salience of Apollonius's Conica and Cartesian analytic geometry, I argue that Newton's "organic construction" of the conics is another important point of reference. The breakthrough that Newton's novel procedure represents is impossible to explain on any view that ties concepts' definitions to particular constructive procedures.Severing the tie between definitions and specific constructive procedures requires us to provide a separate explanation of how the definitions can prove the concepts' objective reality. I argue that this explanation should be given in terms of the ascription of the "marks"-characteristic features-contained in mathematical concepts. While this account of the reality of mathematical definitions takes us some way from construction as it is usually understood, it has the virtue of accommodating shifts in mathematical practice, such as the introduction of novel constructive procedures. It thus conduces toward understanding Kant as a philosopher sensitive to mathematical advances.

Eckert, Daniel

Economics and „verstehendes Erklären“ in the social sciences: Hans Zeisel’s contribution to the working philosophy of the Marienthal study

Abstract: While Marienthal: the Sociography of an Unemployed Community (Die Arbeitslosen von Marienthal. Ein soziographischer Versuch über die Wirkungen langandauernder Arbeitslosigkeit, 1933) by Marie Jahoda, Paul F. Lazarsfeld and Hans Zeisel is acknowledged as a classic in sociology, its obvious connection to economics which is given by its very subject matter (which, according to an earlier paper by Zeisel, is supposed to determine the methodology), - unemployment, is still underresearched. This concerns as well the economic history, for which the decline of the textile factory in Marienthal is a case study for the Great Depression in Austria, as well as the history of economics. And it is allthemore astonishing as one of the co-authors, Hans Zeisel, was a trained economist and a protégé of Ludwig von Mises who not only acted as a board member of the Wirtschaftspsychologische Forschungsstelle but also invited him to the famous conference of the Verein für Sozialpolitik on „Probleme der Wertlehre" in 1931.In the light of Zeisel's very contribution to these conference proceedings „Marxismus und subjektive Theorie", it becomes clear that Mises's conception of economic value as both subjective and revealed by the act of choice („Wahlhandlung"), underpins the claim of the sociographic essay to make „complex psychological facts amenable to statistical methods", spelled out in Zeisel's Appendix on the history of sociography in the Marienthal study and resonant in Lazarsfeld 1960 foreword to its German reissue. Sociography then becomes the task of the elicitation of the preferences and attitudes of the agents in their socially stratified distribution, a task the applied form of which is precisely the consumer research done in the Wirtschaftspsychologische Forschungsstelle.Based on a reading of Zeisel's published and unpublished economic and applied economic work of the late 1920s and early 1930s we aim at at reconstruction of the working philosophy of science underpinning this sociological classic as an attempt to overcome the Erklären/Verstehen separation by a „verstehendes Erklären" based on the integration of economics in the scientific perspective on the overall picture of society.

Eder, Günther

Rigor and Proof in Frege

Abstract: The concept of proof is central to the epistemology of mathematics. Indeed, a mathematical truth becomes a piece of knowledge only if it has been proved. However, the concept of proof presents a number of difficulties. Several recent contributions to the philosophy of mathematical practice have been concerned with the concept of proof as it is understood in ordinary mathematics and how it is related to the concept of formal proof as understood in mathematical logic (e.g., Burgess and De Toffoli 2022, Hamami 2022, Tatton-Brown 2023). One of the central topics in this debate is the thesis that every rigorous proof must be convertible into a proof in some formal system, a thesis that is described by some as the "standard view of rigorous proof".In this paper I will discuss Gottlob Frege's views on the concept of proof and the relationship between formal and informal proofs, both from a historical and systematical perspective. Frege is widely considered to be one of the founding fathers of modern formal logic, but he was also a trained mathematician, working on topics in various branches of ordinary mathematics. Frege's views on formal and informal proof are therefore of intrinsic interest. I will start out by reviewing Frege's reasons for introducing his formal system, the "Begriffsschrift". Three things will become clear from this. First, according to Frege, there are different aims one might pursue with a proof. One is to convince oneself or others of the truth of a proposition; another is to reveal logical connections between truths to determine the epistemological status of a given proposition (purely logical or partly based on intuition?). Second, not every proof that serves the first aim is necessarily suitable to serve the second aim and vice versa. Third, rigor is a property that comes in degrees, and it is at least in part determined by how appropriate a proof is for a given aim.While Frege's views on proof and its aims are closely tied to his logicist project, I will argue that his conception highlights several ideas that are of independent interest, some of them potentially relevant to the current debate on rigorous proof in mathematics. I will particularly focus on the notion that rigor is a property that is not intrinsic to a proof, but relative to some purpose or aim. I will argue that this idea fits nicely into a contextualist conception of rigorous proof, where the degree of rigor attributed to a proof may depend on the context in which it is presented. I will conclude with a discussion of what this conception implies for the "standard view of rigorous proof".Burgess, John P. and Silvia De Toffoli. 2022. "What is Mathematical Rigour". APhEx 25: 1-17.Hamami, Yacin. 2022. "Mathematical Rigour and Proof". The Review of Symbolic Logic 15(2): 409-449.Tatton-Brown, Oliver. 2023. "Rigour and Proof". The Review of Symbolic Logic 16(2): 480-508.

Edgar, Scott

The origins of Hermann Cohen's Principle of the Infinitesimal Method and its History

Abstract: This paper argues for a novel account of the origins of Hermann Cohen's Principle of the Infinitesimal Method and its History (1883), a transitional work that introduces new concerns not contained in his earlier accounts of scientific knowledge but that still has only the seeds of his mature logic of pure knowledge. From one perspective, the origins of Cohen's book are well understood: he conceives of the project of the Infinitesimal Method when he revises his interpretation of Kant's Anticipations of Perception, trading his earlier psychophysical interpretation for one that sees it concerned primarily with our knowledge of physical bodies in motion. Cohen came to see the Anticipations as concerned primarily with the claim that intensive magnitudes play a privileged role in natural science's representation of reality, and the Infinitesimal Method is his attempt to articulate that idea systematically. But from another perspective, this account does not get us very far at all. After all, it only raises the question of why Cohen came to interpret the Anticipations the way he did when he did. Marco Giovanelli (2011, 2016) argues convincingly that Cohen must have come to his new interpretation of the Anticipations, and thus seen the need for the project of the Infinitesimal Method, in late 1880 or early 1881. At the same time, he argues that Cohen turned to his new interpretation of the Anticipations as a consequence of objections leveled at his earlier psychophysical interpretation. I argue that central and important features of Cohen's new interpretation can only be explained by appeal to a different context: namely, Cohen's attempt in the late 1870s and early 1880s to develop an idealist counternarrative to the history of science and philosophy that F.A. Lange had proposed in his History of Materialism. These efforts led Cohen, and his junior colleague Paul Natorp, to certain arguments of Leibniz's that, in Cohen's view, revealed a serious weakness in his own earlier accounts of scientific knowledge. But those same arguments of Leibniz's would also have hinted to Cohen that the Anticipations is where he would find the resources he needed to shore up the weakness in his views. I argue, finally, that the Infinitesimal Method is Cohen's first sustained attempt to shore up that weakness.

Eisenthal, Joshua

‘Are the Axioms of Mechanics Laws of Nature?’

Abstract: Wittgenstein writes "There is no compulsion making one thing happen because another has happened. The only necessity that exists is logical necessity" (6.37). This remark suggests that natural laws lack the kind of necessity that they are normally taken to have. On such a view, Newton's laws of motion would not really compel or constrain an object to move along a particular trajectory, and Coulomb's law would not really demand that the force between two charges takes a particular value. It would merely be a contingent fact that the universe has been sufficiently regular that events can be described by the (so-called) laws of nature, at least up until now. And indeed, Wittgenstein writes at 6.36311: "It is a hypothesis that the sun will rise tomorrow: and this means that we do not know whether it will rise." Such a view is familiar in the history of philosophy, most famously in the writings of David Hume. However, Wittgenstein also says a number of other things about laws of nature in the Tractatus which are not obviously suggested by a Humean account. In particular, Wittgenstein describes a number of law-like statements as "forms of laws", including minimum principles (such as the principle of least action) at 6.3211, conservation laws at 6.33, and laws "of continuity in nature, of least effort in nature, etc." at 6.34. Rather than downgrading such statements to mere contingencies, Wittgenstein claims that these convey "a priori insights about the forms in which the propositions of science can be cast" (6.34). This in turn is the remark that leads directly into Wittgenstein's net analogy at 6.341.In this paper I argue that, according to the Tractatus, there is indeed an important distinction among law-like statements. On the one hand, there are those that appear to be amenable to a Humean account, such as Coulomb's law or Newton's law of gravitational attraction. On the other hand, there are "forms of laws", such as the form of a conservation law or a minimum principle, which reflect certain "a priori insights". I argue further that, surprisingly, Newton's laws of motion (or, more generally, the axioms of mechanics however formulated) fall in the latter category rather than the former. Whereas Wittgenstein would regard the law of gravitational attraction as a substantive claim about the world which could prove to be false (hence the sun may not rise tomorrow), the same cannot be said of the axioms of mechanics. This is why Wittgenstein writes at 6.342: "the possibility of describing the world by means of Newtonian mechanics tells us nothing about the world."

Eisenthal, Joshua

On Constructive and Principle Theories

Abstract: One of the analytical tools that frames Brading and Stan's project is the distinction between constructive and principle approaches to philosophical mechanics. These terms, which were introduced by Einstein in 1919, are adapted by Brading and Stan for application to the context of the 18th century. In this talk, I will examine some of the similarities and differences between Einstein's distinction and the one that Brading and Stan employ, and consider how Brading and Stan's reformulation can be extended into the 19th century and beyond. As Einstein defines his terms, constructive theories "attempt to build a picture of complex phenomena out of some relatively simple underlying formalism," whereas the foundations of principle theories "are not hypothetical constituents, but empirically observed general properties of phenomena" (Einstein 1919). Einstein gives as examples the kinetic theory of gases and thermodynamics: kinetic theory derives the properties of gases from an underlying account of the motions of their constitutive molecules, whereas thermodynamics derives constraints on thermodynamic systems from the overarching principle of the impossibility of perpetual motion. Although both kinds of theory have their advantages, Einstein expresses a strong preference for constructive theories, writing: "When we say that we understand a group of natural phenomena, we mean that we have found a constructive theory which embraces them" (ibid).In Brading and Stan's hands, the distinction does not apply to theories themselves, but rather to different approaches to the development and interpretation of theories (p.265, note 5). Modifying Einstein's notion further, the distinction they have in view is whether the primary resources for formulating a philosophical mechanics are either the qualities and properties of matter (a constructive approach), or theoretical principles such as the laws of motion (a principle approach). Moreover, there is no definitive preference for one approach over the other; on Brading and Stan's view "it is the interplay between constructive and principle approaches to theorizing that turns out to be most fruitful" (p.295). Brading and Stan give many concrete examples of figures who took a constructive approach to philosophical mechanics, including Descartes, Leibniz, Du Châtelet, and Boscovich. The first figure who clearly brings a principle approach into view is d'Alembert, but here the case is not quite unambiguous - d'Alembert's Treatise can be understood from both a constructive and a principle perspective. With this in mind, I will suggest that one of the important shifts that took place in the 19th century was the emergence of a more definitively principle approach to mechanics. I will consider how a number of factors contributed to this possibility, including the emergence of positivism, the conception of energetics as an alternative foundation for mechanics, and the axiomatic work of particular figures such as Kirchhoff and Hertz. Furthermore, I will suggest that it was the rise of principle approaches in the 19th century which laid the groundwork for Einstein's appeal for a return to a more constructive approach later on.

El Nabolsy, Zeyad

Baconianism as an Anti-Racist Philosophy of Science in the Mid-Nineteenth Century

Abstract: Some scholars have argued that Francis Bacon's philosophy of science was in some fundamental sense deeply tied to the trans-Atlantic colonial projects launched in the fifteenth and sixteenth centuries (Scalercio 2018). Moreover, the period during which race science came into its own in Britain, i.e., the mid-nineteenth century, was also a period characterized by a revival of interest in Francis Bacon. This period witnessed the publication of the Spedding-Ellis-Heath edition of The Works of Francis Bacon in seven volumes from 1857 to 1859. Practicing Victorian scientists also generally tended to think of themselves as engaging in an empiricist inductivist Baconian form of inquiry (Van Riper 1993; Verburgt 2021). This also holds true for influential race scientists such as James Hunt, a key figure in the founding of The Anthropological Society of London, as well as Carl Vogt (primarily active in Germany but influential in British circles). James Hunt (1863) presents his claims about the anatomically grounded inferiority of Africans as a product of a Baconian form of inquiry. Vogt (1864) also makes claims about the inductivist Baconian nature of his anatomical inquiry into the differences between white Europeans and Africans. One might be tempted to think that there was some form of elective affinity between Baconian philosophy of science and race science. However, in this paper I attempt to show that such a claim would be too simplistic. Turning, to the writings of the Victorian Sierra Leonian natural philosopher, James Africanus Beale Horton, especially his West African Countries and Peoples (1868), I show how Horton sought to demonstrate that if one takes Bacon's philosophy of science seriously, one will be led to the conclusion that insofar as race scientists such as Hunt and Vogt engaged in "anticipations of nature" (to use Bacon's expression), their research programs were not an instance of science properly so-called. I thus show how Baconianism also came to be used as an anti-racist philosophy of science in the mid nineteenth century. To this extent, I argue that accounts that reduce Baconian philosophy of science to an ancillary of colonialism, imperialism, and race science are too simplistic, both historically and conceptually. References:Horton, James Africanus Beale. West African Countries and Peoples And A Vindication of the African Race. Cambridge: Cambridge University Press, 2011 [1868].Hunt, James. On the Negro's Place in Nature. London: Trübner & Co, 1863.Scalercio, Mauro. "Dominating Nature and Colonialism: Francis Bacon's View of Europe and the New World." History of European Ideas 44, no.8 (2018): 1076 – 1091. Van Riper, A. Bowdoin. Men Among the Mammoths: Victorian Science and the Discovery of Human Prehistory. Chicago: The University of Chicago Press, 1993.Verburgt, Lukas M. "The Works of Francis Bacon: A Victorian Classic in History of Science." Isis 112, no.4 (2021): 717-736.Vogt, Carl. Lectures on Man: His Place in Creation, and in the History of the Earth. Edited by James Hunt. London: Longman, Green, Longman, and Roberts, 1864.

Fabry, Lucie

The intertwinement of history and philosophy of science in Bachelard's epistemology

Abstract: As Georges Canguilhem and Dominique Lecourt discussed whether Bachelard's work should be referred to as historical epistemology or epistemological history, they both claimed that Bachelard had established a new kind of connexion between history and philosophy of science (Lecourt 2002). The aim of this paper is to identify the nature of this connexion and what it implies for Bachelard's practice of history and philosophy, following and discussing previous commentaries such as (Gayon 2003). The paper follows this relation back to Bachelard's early work: his two dissertation theses (Bachelard, 1928a, 1928b). His main dissertation, L'Essai sur la connaissance approchée, is rooted in the philosophical tradition of theories of knowledge; his complementary dissertation, L'Étude sur l'évolution d'un problème de physique, presents itself as an historical inquiry. In both works, however, philosophy and history of science permeate each other: L'Essai turns to the history of science in order to illustrate its thesis on the nature of knowledge, and L'Étude draws philosophical conclusions from the history of the physical laws of heat propagation. It appears, nonetheless, that the philosophical theses which stem from the historical study partly contradict the Essai's claims on the nature of knowledge. This is shown by comparing the way Bachelard considers the mathematisation of physics: in the philosophical Essai, deeply influenced by Bergsonism, mathematisation is presented as a structuration and simplification of experience, which should ultimately be overcome for a minute description of the purely given. In the historical Étude, however, Bachelard claims that mathematical physics is richer, not poorer, than direct experience, for mathematics alone open the path to progress of knowledge. This paper shows, paradoxically, that Bachelard's later works are closer, in methods and theses, to the historical study than the philosophical essay. Since the early 1930's, Bachelard has, indeed, regularly criticised the philosophical attitude which consists in turning to the history of science to illustrate a general thesis; he claimed that philosophers should, instead, derive their philosophy from the study of history of science (G. Bachelard 2013, 7; 2012, 2–5). To specify what that requirement meant to Bachelard, the author shows that Bachelard's project consists, more specifically, in identifying the conditions for and obstacles to scientific progress. It implies a specific way of writing the history of science, which Bachelard presented as retrospective, normative and recurring (G. Bachelard 1951; 1972). It also implies a specific use of philosophical concepts, which seeks to clarify the paths to scientific breakthroughs and promote further progress. As an example of Bachelard's conceptual activity, the paper discusses Bachelard's notion of non-, a concept which analyses the history of non-Euclidian geometries and provides a pattern for the progress of other sciences-Bachelard sketching, for instance, what a non-Lavoisian chemistry may look like (G. Bachelard 2012). Along with

the comment of Bachelard's texts, the consideration of their early reception by Suzanne Bachelard (1970) helps elucidate the assumptions which allow the intertwinement of history and philosophy of science in his epistemology.

Fasko, Manuel

Mary Shepherd's Influence on Mary Somerville on Induction

Abstract: Ever since Eileen O'Neill's groundbreaking paper ('Disappearing Ink') there have been increasing efforts to recover the works of past women philosophers and scientists, particularly in the modern period. One lurking danger, however, is treating these thinkers as 'handmaidens' of male philosophers (Witt, 'Feminist Interpretations of the Philosophical Canon'). This is partly because – Deborah Boyle noted in her keynote at TEMPO 2023 ('Sympathetic Curiosity: Joanna Baillie, Elizabeth Hamilton, and the Bechdel Test in the History of Philosophy') – we currently know few cases of pre-20th-century female philosophers engaging with each other about their own work. Here, I argue that Mary Somerville changed her views on induction – particularly on how we come to believe that 'like causes will produce like effects' – considering criticisms she received from Mary Shepherd. In 1831 Somerville writes in her Mechanism of the Heavens (p. v): "All the knowledge we possess of external objects is founded upon experience, which furnishes a knowledge of facts, and the comparison of these facts establishes relations, from which, induction, the intuitive belief that like causes will produce like effects, leads us to general laws".This claim is in the introduction of the first two editions of On the Connexion of the Physical Sciences(1834/1835). However, in 1858 Somerville publishes another edition and changes the introduction (p. 3): "Our knowledge of external objects is founded upon experience, which furnishes facts; the comparison of these facts establishes relations, from which the belief that like causes will produce like effects leads to general laws". Somerville ceases to characterize induction as an 'intuitive belief' and even forgoes mentioning 'induction' altogether. Instead, she describes a two-stage process that results in our belief that 'like causes will produce like effects'. I argue this is likely because of criticisms she received from Shepherd.Shepherd is concerned with induction (see Essay V of Essay on the Perception of an External Universe) and the notion that 'like causes will produce like effects'. She argues at length in EPEU that it's a conclusion of a 'latent reasoning' (e.g., EPEU 169–70) and not resulting from an "instinctive belief". As Shepherd writes, "that we are incapable of thinking otherwise than we do, can itself be no reason that we think rightly" (EPEU 223).Crucially, we know that Shepherd communicated with Sommerville – who wanted to hear Shepherd's opinion – on "that sentence on Induction [...] at the head of Mrs. Somervilles last book" as Shepherd writes to William Whewell (May 17th, 1837). In the same letter Shepherd notes that she "spoke to Mrs. Somerville as to the mistaken view I thought she had followed in so remarkable a sentence". (see Trinity College, Cambridge University Add Ms a.212.66). Thus, this change of expression is likely the consequence of the exchange between Somerville and Shepherd. While it's itself a minor change, it's nonetheless of great significance. For it illustrates that women's role in the development of philosophy and the sciences in the modern period went beyond offering criticism of their male contemporaries and predecessors.

Feldbacher-Escamilla, Christian J.

Rudolf Carnap's Approach to the Problem of Induction

Abstract: In his work on inductive logic, Rudolf Carnap (1950/1962, p.180) correctly emphasised that the reliance on an inductive methodology was traditionally seen as, ultimately, countering empiricism:EI1 The justification of induction hinges on the assumption about the uniformity of nature.EI2 The assumption about the uniformity of nature is synthetic because it cannot be justified deductively (in which case it would be analytic, but deduction is too weak for justifying this assumption).EI3 The assumption about the uniformity of nature is a priori because it cannot be justified inductively (in which case it would be a posteriori, but this would be circular, as we see with the help of premiss EI1).EI4 Hence, in order to justify the inductive methodology, one needs to make an assumption that is synthetic a priori, which counters logical empiricism.We see the Humean dilemma of induction operating here: the lack of strength of deduction for justifying induction is part of premiss EI2 and the circularity of an inductive justification is part of premiss EI3.Carnap countered this argument by putting forward a logical alternative to a (solely) frequentist conception of the inductive methodology and its notion of probability. He argued that very often and also in the case of justifying the inductive methodology, it is the logical conception that is relevant. If successful, then this approach counters premiss EI2 and makes the uniformity assumption an analytic a priori statement. The main twist in Carnap's reasoning consists in, first, arguing for the need of a probabilistic version of the uniformity assumption and, second, showing that his account of logical probability assesses all probabilistic statements as inductive logical statements-hence, also the needed uniformity assumption should turn out to be analytic (1950/1962, p.181).However, there are some gaps in Carnap's account that we want to address in our contribution. First, it needs to be shown that, in fact, a probabilistic statement about the uniformity of nature can be derived from Carnap's systems. Based on a reconstruction provided in (Leitgeb&Carus 2020), we will outline possible ways to close this derivation gap of Carnap's solution.Furthermore, Carnap distinguished between the traditional problem of induction (Hume's problem) and his approach of providing the degree of confirmation (i.e. the logical probability) of a hypothesis in the light of evidence (cf. his 1966, pp.317f). On some occasions, he states that being able to provide a logical probability is also instrumental for a solution to the traditional problem (cf. chpt.I in 1950/1962). In doing so, he seems to presuppose also a probability-based decision-theoretic framework for selecting a linguistic framework. We will outline the assumptions needed in order to close this gap between solutions of the two problems of induction, i.e. we reconstruct what is needed in order to transfer the Carnapian solution to Hume's problem.Finally, based on these assumptions, we will argue that meta-probabilistic reasoning is presupposed which falls prey to an infinite regress, for which reason the Carnapian solution can, in fact, not be transferred to Hume's problem.

Ferrari, Massimo

Hans Reichenbach and the Theory of Relativity in France

Abstract: The major event characterizing the French discussion about the theory of relativity is the famous debate with Albert Einstein promoted by the French Philosophical Society on April 6, 1922. Outstanding philosophers and scientists attended the meeting: among others, Paul Langevin, Émile Meyerson, Jean Becquerel, Jean Perrin, Léon Brunschvicg, and, last but not least, Henri Bergson. Needless to say, that this legendary discussion represents a crucial episode in the reception of Einstein's physical theory in the early 1920s, although recent scholarship has mainly investigated the discussion between Einstein and Bergson as classical topic of history of philosophy of science. However, there is also another episode on the sidelines of that debate that deserves to be adequately studied: namely, the publication, a few weeks after the meeting at the French Philosophical Society, of an article by Hans Reichenbach that appeared in the Revue Philosophique de la France et de l'Étranger under the title La signification philosophique de la théorie de la relativité. The article was committed to Reichenbach by Einstein himself, which had refused already at the end of 1921 some invitations to write for the French public a popular illustration of the general theory of relativity. Later on, precisely a day before the beginning of the debate at the French Philosophical Society, Lucien Lévy-Bruhl wrote to Reichenbach that he had just met Einstein, who had proposed to him a possible, excellent author able to carry out the writing of the paper. The author Einstein had in mind was just Reichenbach, who not only accepted Lévy-Bruhl's invitation, but he wrote in a very short time the article which appeared at the end of July 1922. The aim of my contribution is to analyse this quite forgotten article in the context both of Reichenbach's interpretation of Einstein's theory of relativity in the early 1920s and of reception of Einstein in French philosophy of science. Two main points, in particular, should be stressed. On the one hand, it is noteworthy that Reichenbach starkly underlines the philosophical background of the theory of relativity. Indeed, at the very beginning of his essay Reichenbach states that the wok of Einstein «always involves philosophical discovery». According to Reichenbach, it is in particular thanks to the philosophical analysis of the concepts of space and time that Einstein was able to build his physical theory. This remark was surely highlighting for a French reader, given also the fact that in that context the debate on Einstein was eminently a philosophical one (as is patently shown in the case of Bergson). On the other hand, it is of some interest to take into account the reaction to Reichenbach's article by some leading exponents of French epistemology, such as Brunschvicg and Meyerson. Tu sum up, this almost ignored chapter of the "Reign of Relativity" can enrich our knowledge of the fascinating story about the discussion on Einstein's theory of relativity and its philosophical significance.

Fève, Corentin

Hermann Cohen: from the psychological method to the transcendental method. The relationship between the history of philosophy and the history of science in the light of these two methods

Abstract: As a young man, Hermann Cohen first joined a research movement led by Moritz Lazarus, professor of psychology in Bern, and Heymann Steinthal, privatdozent of general linguistics in Berlin. Their main ideas were expressed in the introductory text published in 1860 in the Zeitschrift for Völkerpsychologie und Sprachwissenschaft. The new journal was aimed at all those "who study the historical life of peoples in one of its many aspects, in such a way that they endeavour to explain the facts found from the innermost level of the mind, and thus to their psychological reasons for being (Gründe)". It was in this journal that Cohen published his first articles, applying the method of psychological genesis to the study of Greek thought. In all his early essays, whether on philosophy, art, mythology or religion, the question posed is fundamentally psychological: How did such cultural phenomena come about? The answer is provided by collective psychology, that is, by the study of the general laws of the human mind and by the study of the characteristics of the mind of each people, according to the conditions in which they live. From the 1870s onwards, and in particular with the first edition of Kants Theorie der Erfahrung (1871), Cohen finally broke with this psychological method in favour of the transcendental method, which he considered to be the true method of philosophy. This method, introduced by Kant in the first Critique, consists of starting from the "fact of science" (Faktum der Wissenschaft) in order to examine its conditions of possibility. In this way, the psychological method, which consists in asking how we come to knowledge, is replaced by the transcendantal method, which consists in asking about the very validity of our knowledge. The transcendental method, according to Hermann Cohen and his school, consists mainly in discovering the principles or foundations on which scientific knowledge is built. In his view, this was already Kant's undertaking with Newton's mechanics. However, if the transcendental method characterises philosophical activity, what about the history of philosophy and the history of science, with which it is closely linked? Can the historian of philosophy really disregard a psychological method? If, in his early writings, the history of philosophy and the history of science depended on a psychological method, what happens after the emergence of the transcendental method? We examine Cohen's relationship between the history of philosophy and the history of science and these two methods.

Fletcher, Samuel

Spare the Rod: Relativity as Pure Chronometry

Abstract: At the dawn of general relativity's golden age in the 1950s, Irish mathematician and physicist John Lighton Synge proposed, both in his scientific and popular writings, to reduce spatial concepts to temporal concepts. He was responding to a twofold tradition in relativity. The first part of this tradition gave operational definitions of concepts: ideal clocks for times and rigid rods for lengths. But, Synge correctly pointed out, the latter cannot exist in relativity theory, and even approximations to them will fail for large enough distances. The second part of this tradition represented both temporal and spatial concepts in the models of relativity theory with the spacetime internal, according to its sign: for one (say, positive), it represents a duration, while for the other (say, negative, respectively), it represents a length. Synge argued that the structure of the spacetime metric allows one to define (e.g.) length in terms of time, a reduction of chronogeometry to what he calls pure chronometry.Synge's arresting proposal, aside from intrinsic interest for naturalistic philosophy of time, bears analogy with proposals a decade later for the "disappearance" of spacetime (or time) in theories of quantum gravity, starting with the Wheeler-DeWitt equation in 1967. Nevertheless, it has two problems. First, while Synge rejects the need for rigid rods as a primitive operationalization of length in favor of chronometry, he still retains an inadequate operationalist definition of the latter in terms of "standard clocks" and a dubious empirical assumption about the commensurability of all standard clock periods. In particular, he assumes that the "ticks" of all standard clocks that give quantitative meaning to a sequence of events with duration can be put into commensurable ratios, but this is not possible if durations can take on an irrational number of temporal units. Second, he shows how to define infinitesimal length in terms of infinitesimal durations in the context of relativity theory, but this does not extend beyond the infinitesimal nor does it affect a conceptual reduction as claimed. The mathematics of Synge's argument for reduction simply doesn't support his conclusion of conceptual reduction.In light of these criticisms, I defend a more moderate version of Synge's thesis and sketch a mathematical argument that avoids the aforementioned criticisms. I prove that finite lengths can be reduced to the times radar signals need to probe them. It is thus not spatial concepts which are reduced to temporal ones, but quantitative spatial properties to temporal properties. The qualitative spatial concept, familiar from standard relativity theory, of two events being spacelike related to one another is retained. I suggest nevertheless that this still vindicates the spirit of Synge's reduction using concepts and mathematical tools that would have been available to him.

Fontanive, Gabriele

Thought Experiments in Social Science: Do They Have a Life of their Own?

Abstract: In Representing and Intervening (1983), Ian Hacking famously states that "experimentation has a life of its own". His aim was to call attention to the fact that the philosophy of science of his time had been focusing almost exclusively on scientific theories, on the representational characteristics of scientific knowledge. He proposes that we pay attention to the importance of experiments and, more than that, to the fact that we use scientific knowledge to transform the world. Furthermore, Hacking emphasizes that we acquire scientific knowledge by means of experimentation, by intervening in the world. Hence, experimentation is not necessarily subservient to theoretical and representational scientific knowledge. Often, as Hacking presents in his plethora of examples in the second part of the book, scientists produce knowledge and technology without having precise representations of the world. A decade later, as the debate on thought experiments blooms in philosophy of science, Hacking asks in the title of his paper: "Do Thought Experiments Have a Life of Their Own?" (1992). The answer he gives, however, is negative.That answer can be reconsidered by discussing Otto Neurath's scientific utopianism. In Foundations of the Social Sciences (1944), Neurath critically notes that, in the social sciences, working only with given social orders is very limiting. The object of study of the social sciences must be understood as something complex and multifaceted, so that social scientists should not be limited to observing only existing and past societies. Seeking to expand the limits of our understanding, in addition to exploring the educational role of science, Neurath suggests that social scientists should be concerned with elaborating, developing and comparing imaginary social arrangements in search of improving social situations. In an approximation to the literary tradition, these imaginary social arrangements were called utopias. Neurath conceives utopias as schemas of social situations that take into account as many aspects as possible, in order to understand consequences of a given social plan or decision. This methodology can be characterized as dealing with thought experiments, since they are theoretical constructions and comparisons of imaginary societies from which important conclusions about our society can be obtained.This talk is going to argue that the role Neurath ascribes to thought experiments in the social sciences is similar to the role ascribed by Hacking to (non-thought) experiments in the natural sciences. Since Neurath's utopias can be used to expand scientific knowledge towards variations that are only potential in (non-imaginary) practice, the same thought experiment can point to different theories and different contexts of investigation. This suggests that, at least in Neurath's scientific utopianism, thought experiments do have a life of their own. References Hacking, I. 1983. Representing and Intervening. New York: Cambridge University Press.Hacking, I. 1992. Do Thought Experiments Have a Life of Their Own?. PSA: Proceedings of the Biennial Meeting

of the Philosophy of Science Association (1992), Vol. 2: 302-308. Neurath, Otto. Foundations of the Social Sciences. Chicago: University Of Chicago, 1947

Formica, Giambattista

Von Neumann's Lost Argument for the 2nd Incompleteness Theorem

Abstract: On September 7, 1930, Kurt Gödel announced an early version of what was to become the 1st incompleteness theorem (cf. Godel 1931a). The occasion was a roundtable on the foundations of mathematics that occurred on the last of a three-day International Conference on the Epistemology of the Exact Sciences, held in Königsberg from the 5th to the 7th of September (cf. Erkenntnis 1931). During the debate, this announcement was ignored by all the scholars present; however, at the end of the roundtable, John von Neumann approached Gödel in order to better understand his point (cf. Wang 1981). A few weeks later, on November 20, von Neumann wrote to Gödel that, using the latter's methods, he had arrived at the 2nd incompleteness theorem and would be able to send him the proof as soon as it was ready to be printed. In the letter, von Neumann also sketched an argument that is not conclusive and one may presume he had detailed elsewhere in order to support his result (cf. von Neumann 1930a). Today, Gödel's reply is considered lost (though two drafts of it are in von Plato 2020), but from a subsequent letter by von Neumann one can be sure about a few of its points: Gödel had told his colleague that he had reached the same result and that soon the article, i.e., the famous Gödel's 1931 paper, containing both the 1st and 2nd incompleteness theorems, would have appeared in the journal that eventually published it. In the second letter, written to Gödel on November 29, von Neumann put forward two items of interest: (i) reproducing Gödel's lines of thought, he was able to say that, in his own proof, he had employed a somewhat different method, and (ii) since he had reached the theorem using his colleague's techniques, he would have left to Gödel the paternity of the great discovery (cf. von Neumann 1930b). Furthermore, on January 12, 1931, after having read Gödel's proof sheets, in a third letter to Gödel, von Neumann outlined a way to shorten the proof of the 2nd incompleteness theorem (cf. von Neumann 1931). Since von Neumann did not publish anything on the subject, today his proof of the 2nd incompleteness theorem is commonly considered lost. However, one crucial question is still open: how did he reach the 2nd incompleteness theorem, knowing as little as he seemingly did about the 1st? In my talk I shall give an answer to the question by strictly linking two of the three letters written by von Neumann to Gödel (i.e., von Neumann 1930a and von Neumann 1931) and by presenting a conjectural argument to explain von Neumann's discovery. The argument is consistent with all the fundamental documents analyzed in Formica 2023 and is different from that presented in Hintikka 2000, Franzén 2005, Goldstein 2006 and von Plato 2017.

Franco, Paul

Mary Hesse on Open Texture and Interpretation

Abstract: In her review of Thomas Kuhn's The Structure of Scientific Revolutions, Mary Hesse says, "This is an important book. It is the kind of book one closes with the feeling that once it has been said, all that has been said is obvious, because the author has assembled from various quarters truisms which previously did not quite fit and exhibited them in a new pattern in terms of which our whole image of science is transformed" (1963, 286). In his 1990 Presidential address to the Philosophy of Science Association, Kuhn mentions Hesse as being at the start of the historical turn in philosophy of science, alongside Stephen Toulmin, Russ Hanson, Paul Feyerabend, and others. While all contributed some of the "truisms" Hesse alludes to, this talk is concerned with Hesse's novel contributions to the changing landscape in history and philosophy of science prior to the publication of Kuhn's Structure.In the talk, I pay particular attention to Hesse's treatment of scientific language. I trace two claims Hesse develops throughout her work in the 1950s, which she makes particularly clearly in her 1961 Forces and Fields. First, is her claim that the relationship between the theoretical and observational terms of our scientific theories is best understood along the lines of interpretation rather than translation. She says, "The correct analogy for the relation of theoretical and phenomenal languages is not the relation between a number-code and English, or even between simple sentences in English and in French, but the translation of poetry into prose" (1961/2005, 19). Second, is Hesse's related emphasis on the role of models "as devices...essential for rendering a theory intelligible and testable" (21). If models and analogies are essential in this way, we cannot dismiss them as mere aids in understanding unimportant to accounts of the logic of science.In developing these points about interpretation and the essential character of models, Hesse extends Friedrich Waismann's account of the open texture of empirical concepts to theories. According to her, "Theories must have 'open texture'...a fringe of meaning not defined by observation, otherwise the whole meaning of the theory would change whenever it was desired to incorporate into it observations of a novel kind, and it is precisely the function of theories to assimilate such new observations without the meaning of the theories being radically altered" (8). As I'll argue, Hesse's appeal to open texture, as well as the close attention she pays to scientific language, interpretation, and models, places her alongside a strand of 1950s British philosophy of science influenced by the later Wittgenstein and Oxford ordinary language philosophy. While this has not always been the case, standard narratives about the move towards more historically- and practice-based accounts of science in philosophy of science have tended to gloss over these contributions in favor of focusing on W.V. Quine and Kuhn. In highlighting Hesse's important contributions and placing them in their philosophical context, we get a fuller picture of the history of history and philosophy of science in the 1950s-1960s.

Franke-Reddig, Julia

Ilse Schneider's Understanding of Kant and Einstein as an Alternative to the Realist-Empiricist Interpretation

Abstract: At the beginning of the 20th century, the development of modern mathematics and physics led empiricist philosophers, such as Moritz Schlick, to the position that Kantian philosophy was no longer defensible. In Schlick's early writings we can read that all attempts to reconcile "apriorism" with the modern development of scientific thinking had failed. As a reason for this consideration can be cited, for example, the "discovery" of non-Euclidean geometry in the 19th century. But also the axiomatisation of geometry by David Hilbert, using so-called implicite definitions, through which any reference to intuition in geometry has been discarded. However, the most famous argument against transcendental philosophy is the empirically successful application of non-Euclidean geometry in relativistic physics. From Schlick's realist philosophical perspective, this may have appeared as the "final blow to the Kantian conception of geometry" (Neuber 2018).In contrast to Schlick, Ilse Schneider took the position that to make the assessment of Kantian philosophy dependent on the answer to the question, whether physical space is Euklidean or not is to completely misunderstand the meaning of the Kantian concept of space: it would mean to put something empirical into "pure intuition". She concluded: "As long as we do not interpret Kant at random and in an intentionally misleading manner, we can see the allegedly stark contradictions between his thought and modern relativity disappear".As a result of her early writing "On the Spatio-Temporal Foundations of Science" she formulates that the Kantian transcendental philosophy does not contradict the theory of relativity and that Einstein's physics even comes closer to the Kantian philosophy than any other physics before. Reading Schlick's comments on Ilse Schneider or more generally on Kantianism, Neokantianism and Apriorism, one gets the impression that the philosophical position of transcendentalism can only be represented by philosophers who have no background in physics and thus no deeper understanding of the implications and consequences of relativistic physics. But Schneider was a physicist herself. If one reads her book on Kant and Einstein unprejudiced, it turns out that she does not claim that the theory of relativity follows in any way from Kantian philosophy, as Schlick accuses to the Kantianist view. Nor does she claim that Newtonean physics and the Kantian Philosophy are in any in contradiction to each other, as one might assume from Schlick's account of apriorism. Her aim is to show that Kantian transcendental philosophy is consistent with both, Newtonean Physics and the theory of relativity as well. While arguing this position, she tries to show that this claim would probably be in Kant's sense as well.The talk is going to contrast Schlick's and Schneider's positions on Kantian transcendental philosophy and to elaborate Schneider's interpretation as an alternative to Schlick's analysis worth considering.

Frappier, Mélanie

Minding the Gap: Sophie Germain's mathematics, history, and epistemology

Abstract: Because of her contributions to number theory and mathematical physics, Sophie Germain (1776-1831) has been used-even in her lifetime-as an example of both the benefits and perils of women's education. Germain never openly entered the debate. Her only real contribution to epistemology was her unfinished Considérations générales sur l'état des sciences et des lettres, a two-part treatise offering what could easily be taken to be nothing more than a very naïve comparison between mathematical research and literary composition, followed by a poorly informed and rather speculative conjectural history of the arts and sciences. Such a reading, however, fails to explain why Comte and other 19th-century positivists lauded Germain as a philosopher and historian of the like of Ferguson, Hume, Kant, Fichte, or Hegel. I argue here that, read as a work informed by Condillac's sensationism and perhaps the work of Turgot, Condorcet, and d'Alembert, Germain's Considérations can be understood as an empiricist response to the idealists' "pragmatic histories of philosophy". From her acceptance of Condillac's proposal that not only our knowledge, but our faculties are shaped by nature, Germain argues that any discipline reaches maturity-and a subsequently accelerated development-once its practitioners figure out how to describe the relations existing between the objects they describe rather than these objects themselves. Knowledge, she maintains, is ultimately always about order and ratios. Of course, such a view precludes the type of a priori and atemporal history of knowledge offered by the idealists, but it does not prevent, the second part of Germain's book suggests, a genetic account of knowledge development that might allow us to both predict and guide the future development of any domain of human knowledge, be it physics, poetry, music, politics, or morality. Read in the light of contemporary philosophical debates, Germain's Considérations reveals itself as a sketch of an empiricist epistemology predicting that all knowledge would, in time, achieve the same self-evidence, precision, and universality as geometry.

Friedl, Johannes

What Schlick Might Have Said to Neurath

Abstract: What Schlick Might Have Said to NeurathSchlick's first contribution to the Vienna Circle's Protocol-Sentence Debate, his now classical paper "The Foundation of Knowledge" from 1934, marks the public visible split within the movement of Logical Empiricism. Naturally, the position developed here, departing from the criticism of the "aberrations" of the Circle's so-called "left wing" did not go unchallenged. The most explicit and direct reaction is Neurath's "Radical Physicalism and 'the Real World'" by the end of the year. Compared with other writings of Neurath, this paper consists of rather carefully fleshed-out arguments; unfortunately, it also contains polemic formulations which prompted Schlick's refusal to continue the discussion with Neurath in any form whatsoever.This breakdown of communication is regrettable not only from a historical point of view but also for those more interested in systematic considerations: the debate concerns issues of vital significance such as foundationalism vs. coherentism, intersubjectivity, the contribution of sensation to justification etc. (on both ends, the mixing up of epistemological questions with the issue of the nature of truth seems to me to be confused and will be neglected here). However, Schlick explicitly acknowledged that Neurath's paper contains substantive difficulties (see his letter to Carnap, November 14, 1935, ASP-RC 102-70-11). In short, the refusal even to mention the name of his opponent any longer does not mean that Schlick did not reply.The talk aims at the reconstruction of such a rejoinder. I will try to show that a good deal of Schlick's writings of his last period is to be understood as reaction to Neurath's objections. This concerns not only passages in his published writings (especially "Facts and Propositions", "On 'Affirmations'", "On the Relation between Psychologial and Physical Concepts", "Meaning and Verification"), but also remarks in his correspondence. Of special significance are his lectures in 1934/35 "Logik und Erkenntnistheorie" (recently published in: Schlick, Vorlesungen und Aufzeichnungen zur Logik und Philosophie der Mathematik, ed. by Martin Lemke and Anne-Sophie Naujoks, 2019). Taken together, this does not simply add up to a defence. At a central point, Schlick's revises his standpoint: The thesis of the ineffability of his basic statements („Konstatierungen", mostly translated as „affirmations") is no longer upheld. Taking up suggestions of Wittgenstein, he dismisses the claim of a principal privacy of sensations. All in all, a) the final position Schlick reached is far more sophisticated than usually presented, and b) has to be assessed as a further stage in a development still in full flow, interrupted by Schlick's premature death.

Friedman, Michael

Metaphorical investigations and mathematical practices: How did Hans Blumenberg read Wittgenstein’s "Remarks on the Foundations of Mathematics"?

Abstract: Hans Blumenberg's thought and the philosophy of mathematics are generally not associated with one another. Indeed, Blumenberg (1920-1996) is known for his work in the field of the history and philosophy of ideas, which focuses on the role played by metaphors in the formation of concepts and of images of knowledge. One of his early works is his 1958 text Paradigms for a Metaphorology (1958), which starts with a critique of Descartes, in which Blumenberg rejects the Cartesian project of developing a language which consists only of clear and distinct concepts,[1] and in which all figurative elements are eliminated. This rejection led Blumenberg to an inquiry into the various metaphors to be found in the history of European thought. If one turns to mathematics, which can be considered as reaching this ideal of presenting clear and distinct concepts, then one nevertheless hardly finds any reference to it in Blumenberg's writings. Indeed, his relation to mathematics, its history and its philosophy is rather difficult to reconstruct, since a detailed discussion on these subjects is not often found in his published writings. However, when Blumenberg discusses Wittgenstein's writings in his book Höhlenausgänge, in a chapter named "Im Fliegenhaus" (1989, 752-792), one does find reflections on, among other subjects, Wittgenstein's views on mathematics, mathematical proofs, as well as mathematical practices and riddles, mainly from Remarks on the Foundations of Mathematics. Rather than a study on concepts, there is here a focused discussion on how mathematics is practiced, being one of the main foci of later Wittgenstein's philosophy, which also challenges formalism. Here it should be noted that Blumenberg (2010, 43) himself criticizes axiomatization, noting that had one tried to reduce all of geometry to a set of axioms and postulates, then geometry itself might not have existed. Discussing Wittgenstein's Remarks, Blumenberg (1989, p. 758) emphasizes that with some solutions to mathematical riddles, "intricacies as well as licenses in thought experiments are created, especially imaginary spaces like caverns in the 'logical space' of the Tractatus." In a similar manner, he stresses Wittgenstein's conception of proof as an underground passage, to where the "light" of logic does not necessarily reach.The talk aims to analyze Blumenberg as a reader of Wittgenstein and how Blumenberg himself considered philosophy of mathematics and of mathematical practices. An examination of Blumenberg's annotated exemplars of Wittgenstein's writings as well as his notes on Wittgenstein, found at Deutsches Literaturarchiv Marbach, which will be also discussed, provides a more coherent understanding of Blumenberg's own conceptions of the philosophy of the practice of mathematics, rather than presenting Blumenberg's views of it as a simple critique on formalism.ReferencesBlumenberg, H. (1958). "Paradigmen zu einer Metaphorologie", Archiv für Begriffsgeschichte 6, 7-142.Blumenberg, H. (1989). Höhlenausgänge.

Suhrkamp.Blumenberg, H. (2007). Theorie der Unbegrifflichkeit. Suhrkamp.Blumenberg, H. (2010). "Theorie der Lebenswelt". In: Theorie der Lebenswelt (ed. Sommer, M.), Suhrkamp, 7-108. [1] The reference to the Cartesian 'clear' and 'distinct' concepts is explicitly expressed in: (Blumenberg, 2007, p. 11).

Frühstückl, Robert

Edgar Zilsel in the Vienna Circle’s protocol-sentence debate

Abstract: Edgar Zilsel's relationship to the Vienna Circle is in many respects ambivalent. Although he participated frequently in its meetings, he maintained a critical distance from the group. While he shared some central tenets of their philosophy, his views on epistemology, the role of logic in philosophical inquiry, and the commitments that he thought come with being an empiricist differed remarkably from those of other members of the Vienna Circle.This contribution tries to shed some light on this complicated relationship by examining in detail Zilsel's position within the famous "protocol-sentence debate" (Uebel 2007). It will become apparent not only that Zilsel has attempted to develop an original position (Zilsel 1932) on protocols, somewhere located between Carnap, Neurath, and Schlick, but also that this position is quite substantially dependent on his earlier philosophical work, particularly on Das Anwendungsproblem (Zilsel 1916). This not only reveals interesting continuities in the development of Zilsel's thinking but also outlines a broader and unfinished philosophical project.ReferencesUebel, Thomas. 2007. Empiricism at the Crossroads. The Vienna Circle's Protocol-Sentence Debate. Chicago: Open Court.Zilsel, Edgar. 1916. Das Anwendungsproblem. Leipzig: Barth.Zilsel, Edgar. 1932. "Bemerkungen Zur Wissenschaftslogik." Erkenntnis 3: 143–61.

Futch, Michael

Royce and Conventionalism

Abstract: This paper is an examination of the relation of Josiah Royce's late writings on the philosophy of science to the thought of Henri Poincaré. In his introduction to The Foundations of Science – a translated collection of three of Poincaré's writings – Royce endorses the thesis that some principles of mechanics are best understood as conventions. Being neither Kantian a priori synthetic propositions nor experimental truths, these conventions are "disguised definitions" that, while suggested by experience, are not the kinds of principles that will be refuted or called into question by subsequent experiments. Royce repeatedly likens Poincaré's views to his own notion of "leading ideas," a notion developed in earlier writings on probability theory. On Royce's telling, leading ideas are like conventions in that they are regulative principles used to organize scientific inquiry but that are inoculated against falsification. This makes conventions and leading ideas importantly different from experimental laws in that the latter are subject to disconfirmation. Explicating the nature of leading ideas by way of underscoring their similarity to Poincaré's conventions, Royce writes that "these hypotheses are not subject to direct confirmation or refutation by experience . . . . [They] stand in sharp contrast to the scientific hypotheses of the other, and more frequently recognized type. i.e. to hypotheses which can be tested by a definite appeal to experience." Following Poincaré, Royce emphasizes that, though not imposed with the predetermination of "Kant's rigid list of a priori forms," leading ideas are selected not arbitrarily but upon the basis of their ability to confer conceptual unity and connectedness to a wide range of phenomena.The focus of this paper will be on the nature of Royce's conventionalism, with the aim of establishing that it is in fact quite different from what one finds in Poincaré. First, I will exposit Royce's theory of leading ideas and how it is developed in opposition to Peirce's account of fair sampling. I then turn to the ostensible similarity between Royce's theory of leading ideas and Poincaré's conception(s) of conventions. Here, I try to show that, contrary to Royce's own assessments, leading ideas are importantly different from Poincaré's conventions, at least if conventions are understood as either disguised definitions or apparent hypotheses. Instead, to the extent that leading ideas find a counterpart in Poincaré's philosophy, they are better construed as analogous to so-called "indifferent hypotheses." I conclude by arguing that this divergence suggests that there are limitations on the extent to which Royce's philosophy of science can be considered as a kind of conventionalism.

Gandon, Sébastien

Depsychologizing without desubjectivizing? Logic and philosophy in Royce, C. I. Lewis and Quine.

Abstract: The separation between logical and psychological considerations is one of the pillars of Frege's and Russell's philosophy of logic. Around the same time, the American philosopher Josiah Royce developed an anti-psychological view of logic as well, but one which was grounded in the concept of the Self. The Self in question was not the finite psychological self, but the absolutely rational and infinite Ego - the divine Self. From his perspective, the basic logical laws were the truths that must be presupposed to make the activity of this absolute Self possible - or put another way, the basic logical propositions were such that "the very act of getting rid of them, or thinking them away, logically implies their presence".The goal of the paper is to describe Royce's anti-psychological yet subjectivist view of logic, and to assess his impact on the work of one of his students, C. I. Lewis. In Mind and the World Order, C. I. Lewis takes up Royce's reflexive method, and links a priori truths (of which logical laws are a subset) to the activity of the Self. But he rejects Royce's "speculative" view, which involves positing the existence of an absolute Self. For C. I. Lewis, there is only finite minds; but then, how to prevent Royce's reflexive method from collapsing into psychological analysis?C. I. Lewis' finite minds are finite but "intelligent". The intelligence is defined as "the capacity [...] of transcending our individual limitations of discrimination by indirect methods" (MWO, p. 113). An individual being whose behavior and habits differs from ours has the power to attain our concepts, if he is intelligent, that is, if he is able to enter into a process of back-and-forth communication with the other intelligent finite beings that we are: "A Martian might be likeminded with ourselves in spite of quite different immediate experience. But, if so, he must be very intelligent" (MWO, p. 114).Is intelligence a psychological property? In one sense, yes: it is a property of several finite selves that interact one with the other. But in another sense, it is not. Indeed, intelligence is the capacity for a finite mind to transcend its psychological limitations to attain the common level of the concepts and the a priori. The mind considered by C. I. Lewis is not the absolute rational mind as it was in Royce. But it is not the psychological mind either, since Lewis' mind has the capacity to overcome any psychological restriction. C. I. Lewis' intelligent mind and his emphasis on the communication process can thus be seen as kind of secularization of Royce's view.In a last moment, I will argue that, even if Quine's behavioristic view is completely foreign to C. I. Lewis' outlook, we find trace, especially in the first chapter of Word and Object, of the idea that Selves can mutually adjust to each other by communicating (that minds are intelligent in the sense of C. I. Lewis).

Garancini, Daniele

Looking at the Painting, Looking at the Brushstroke - A Change in Focus in the Study of Scientific Progress

Abstract: This paper argues that there has been a change in focus in the recent history of philosophy of science. I compare the debate about scientific progress between Popper (1934), Kuhn (1962), Feyerabend (1970), and Lakatos (1978) with the debate on the same subject between Bird (2007), Rowbottom (2008), Niililuoto (2014), and Dellsén (2016). I argue that the former debate focused on major turning points in the history of science, whereas the latter focuses on minor scientific achievements which collectively may or may not constitute major turning points. In a manner of speaking, Popper, Kuhn, Feyerabend, and Lakatos are interested in the painting, they look at the big picture. Bird, Rowbottom, Niililuoto, and Dellsén, focus on the brushstroke, they look at the smaller units of scientific progress. Let us consider Bird's (2007) criticism of Kuhn (1962) as a case in point. Bird argues that Kuhn's account of progress is inadequate because it counts as progressive a case that clearly is not. He points out that Nicole d'Oresme believed that goat's blood could split diamonds. Kuhn, says Bird, held that any paradigmatic solution to a puzzle amounts to progress. Thus, had Oresme produced a theory of goat blood splitting diamonds, that would have been progress by Kuhn's standards. This is absurd, continues Bird, because in this case progress would occur once the solution to Oresme's puzzle, and indeed the puzzle itself, is revealed to be spurious. One can argue that Bird's reconstruction of Kuhn's account is uncharitable (Rowbottom 2023), but never mind that. What is interesting, here, is that Bird is focusing on a rather narrow issue. That is, he is discussing progress in the context of the discovery that goat blood does not possess diamond splitting properties. By contrast, Kuhn tends to focus on major scientific turning points, which often occurred over several decades. For instance, Kuhn discusses the Copernican revolution and the discovery of oxygen (Kuhn 1957; 1962). In the process of analysing major scientific turning points, Kuhn describes in detail the work of individual scientists. However, his focus is not on describing what constituted progress in each of the minor scientific findings that collectively constituted a major turning point, but rather on what constituted progress on a big-picture level. Bird, on the other hand, seems interested in finding out what constitutes progress in every individual scientific finding.It is unclear whether the business of finding the fundamental feature of punctual progressive events coincides with the business of finding the fundamental feature of major scientific turning points, even if the former events constitute the latter. A painting may be more than a sum of brushstrokes. If these two endeavours do not coincide, then the change in focus that has occurred in the recent history of philosophy of science might not mark a disagreement. A theory about what constitutes minor progressive events and a different theory about what constitutes major turning points might both be true. Indeed, it is often profitable to look both at the painting and the brushstrokes.

Garau, Rodolfo

Horkheimer’s and Borkenau’s Histories of Philosophies of Science (1930-1934)

Abstract: In its early years, the Frankfurt Institute for Social Research produced two notable accounts of the emergence of early modern philosophy of science: "Der Übergang vom feudalen zum bürgerlichen Weltbild" ("The Transition from the Feudal to Bourgeois Interpretation of the World," Paris 1934) by Franz Borkenau and "Anfänge der bürgerlichen Geschichtsphilosophie" ("The Beginning of the Bourgeois Philosophy of History," Stuttgart 1930) by Max Horkheimer. Such background also sheds light on the critical positioning of Henryk Grossmann against Bokenhau concerning the technological roots of early-modern mechanism (scientific and philosophical) in his classic Die gesellschaftlichen Grundlagen der mechanistischen Philosophie und die Manufaktur (1935). Notably, Borkenau and Horkheimer held distinct philosophical and political positions, but their analysis of the rise of early modern reflections on science, I contend, exhibits parallels that are as remarkable as the differences. Both Borkenau and Horkheimer identified the emergence of individual subjectivity and its belief in comprehending nature and its laws as central to the new bourgeois sensibility. However, Horkheimer's essay aimed to demonstrate that the notion that psychological individuality was rooted in behavioral egoism – a trait he associated with the capitalistic homo economicus, which he saw reflected in Freudian psychoanalysis – was fundamentally a product of the early modern mechanization of physiology and psychology. Consequently, he argued that it was not an inherent aspect of human nature. This historical analysis held political implications, as it was integral to the endeavor of melding Marxism and psychoanalysis to develop novel research and analytical perspectives. In contrast, Borkenau perceived early modern intellectualism and mechanism as a reaction to the crisis experienced by the Western world during the transition from feudal to bourgeois societies. This presentation aims to reconstruct and compare Horkheimer's and Borkenau's histories of the philosophy of science, placing them within the contexts of their respective political agendas in the early 1930s.

GIOVANNINI, EDUARDO

Carnap on Logicism, Geometry, and Logical Interpretation

Abstract: Logicism was a central theme in Carnap's philosophical reflections on the foundations of mathematics. Throughout his intellectual career, he developed original and detailed proposals to carry out a logicist reduction of pure mathematics to (higher-order) logic. Perhaps the most systematic version of Carnap's logicist thesis, defended in several works in the late 1920s and the early 1930s, was a form of conditional logicism. According to this position, the content of any mathematical statement can be recast as a conditional statement, where the antecedent is a conjunction of the mathematical axioms and the consequent a theorem. Moreover, reformulated as universally quantified conditionals, all mathematical statements can be derived from logical axioms. As recently revealed by Schiemer (2022), Carnap combined this form of conditional logicism with the practice of (universal) ramsification, that is, the systematic substitution of all mathematical primitives by logical variables (of the adequate type), and the formulation of all mathematical statements as universal conditional statements consisting only of logical terms. The aim of this talk is to critically examine a variant of the logicist thesis outlined by Carnap in his work Foundations of Logic and Mathematics of 1939, and particularly to assess its application to geometry. This version of the logicist reduction of mathematics is based explicitly on the idea of a translation, which closely resembles the modern notion of interpretation in mathematical logic. Carnap's fundamental claim was that pure mathematics (including arithmetic, analysis, and also geometry) could be reduced to higher-order logic by constructing purely logical interpretations based on the synthetic translation of these mathematical theories into the higher logical calculus. Roughly, a theory T1 is interpretable in a theory T2 if the primitives of the interpreted theory T1 can be translated into formulas of the interpreting theory T2 so that the translation function preserves theoremhood and logical structure. Interestingly, Carnap added a semantic component to his notion of translation of theories by claiming that, given the normal interpretation of the higher-order logical calculus (i.e., simple type theory), one can construct purely logical models of the mathematical calculi. In the case of geometry, a logical interpretation could be obtained by reducing it to a mathematical calculus (i.e., the theory of real numbers) via the customary method of coordinatization.Carnap's schematic description of the logicization of geometry in 1939 raises significant interpretative issues. Firstly, a precise understanding of this alternative proposal of the logicist reduction of mathematics, and particularly of geometry, requires a more exact logical explanation of the notion of translation. Secondly, the claim that the 'normal interpretation' of the higher-order logical calculus is purely logical is problematic for well-known reasons (e.g., the assumption of the axiom of infinity). Thirdly, the historical and conceptual connections between this variant of logicism and other versions advocated by Carnap also demand further clarification. In this talk, I aim to provide a detailed reconstruction of Carnap's geometrical logicism in 1939 by paying particular attention to these three interpretative issues.

Gorham, Geoffrey

Radical Themes in Early American Natural Philosophy

Abstract: The insights of HOPOS have not been extended sufficiently to philosophy of science in European Colonies, including pre-revolutionary America.1 This paper examines radical tendencies in several early American natural philosophers, such as Cadwallader Colden and Benjamin Franklin. Their philosophies of science relied upon a minimalist, deist theology, more familiar from political actors of the time. Scientific deism repudiates the Newtonian dictum "to treat of God from phenomena is certainly a part of natural philosophy." (Principia, General Scholium) If historians of science like Cunningham are right that "natural philosophy was about God and about God's Universe" (1991, 281), then Colonial natural philosophy was superseded by an austere, empiricist natural science which bracketed the God of traditional theology. Colden himself was a Newtonian; but he declares: "The principles of religion are so much distinct from the principles of philosophy that they have nothing in common." (On the Eye, 149) I briefly review the theological natural philosophy of early 18th c. America (e,g. Mather, Edwards), and then discuss Colden's extended correspondence with Yale philosopher Samuel Johnson. Although Colden and Johnson are influenced by British philosophy (Newton and Berkeley respectively; Locke jointly), they develop divergent conceptions of God's place in the natural world. In particular: (i) Idealism vs. Realism. Johnson defends idealism, following Berkeley (with whom he corresponded while the latter was at Whitehall): "Ideas of sense are not pictures but the Real things". Colden rejects Berkeley's skepticism about infinitesimals, and defends a Lockean causal-representative model of perception: "simple ideas arise from the simple actions of simple powers and complex ideas from the complicated actions of several simple powers". When it comes to science, Johnson espouses pious instrumentalism, and Colden inferential realism. (ii) Activity of Matter. Colden invokes Newton, Leibniz, and reason, in attributing inherent activity or agency to matter: "There is no great difficulty of forming a conception of this agent, as a species of matter." Johnson believes he is following Berkeley in holding, "the words Material Agents are really a contradiction in terms and that we cannot use the term 'agent' when we speak of material things." The dispute comes down to whether activity requires conscious volition. (iii) Natural Theology. Johnson aims to preserve a traditional role for God in natural theology and natural philosophy: "all motions and consequently actions in nature are conformable to the wisest rules and laws . . .and must therefore be under the active management of a most wise and designing principle or cause." In contrast, Colden articulates a surprisingly pantheistic God: "the intelligent being is universally diffused in the same manner as space is", i.e. even through all bodies. (They naturally accuse one another of Spinozism.) With appeal to contemporaneous natural philosophers, such as Franklin and botanist Jane Colden, I argue that radical metaphysical disputes such as (i)-(iii) were key to the separation of theology from philosophy, and hence the emergence of science from natural philosophy, in the American colonies._____ 1. Though see Irving, "Rethinking Instrumentality", HOPOS, 2 (2012): 55-76.

Gori, Pietro

Expanding the epistemological framework of natural science. Mary Hesse (and Thomas Kuhn) on hermeneutics, translation, and interpretation

Abstract: In her Revolutions and Reconstructions in the Philosophy of Science (1980), Mary Hesse argues that natural science should be integrated into a wider epistemological framework embracing the philosophy of social science, hermeneutics, and the sociology of knowledge, in order to counter the traditional deductivist account of scientific explanation. In fact, she defends a post-empiricist view of natural science that closely resembles the hermeneutic analysis of the human sciences as expressed e.g. by Jürgen Habermas. That account is based on an instrumental rather than theoretical account of scientific knowledge, which Hesse conceives of as continuous with the hermeneutical model of understanding that, for Habermas, is employed in the cultural studies. According to that model, the structure of an object of study can never be analyzed to the point of eliminating all contingency (Habermas 1971: 161). Similarly, Hesse defends that in the natural sciences theoretical facts are only imperfect translations or interpretations of practical facts. Furthermore, she maintains that the deductivist account of scientific explanation should be replaced by the network model developed by Duhem and, later, by Quine, that is, the view that what is primarily significant in science is the interpretive expression we gave to what is observed.In presenting these views, Hesse elaborates on an issue that has also been raised by Thomas Kuhn in a series of papers published after The Structure of Scientific Revolutions. In these papers, Kuhn deals with translation and interpretation in the history and philosophy of science, and argues that "no more in the natural than in the human sciences is there some neutral, culture-independent, set of categories within which the population – whether of objects or of actions – can be described. ... Discovery is required in both cases, and hermeneutic interpretation is how discovery is done" (Kuhn 1989: 220). A similar conception (with the due differences, of course) can be found in Hesse. For her, interpretation of a theory should be understood as involving holistic stories about the concepts one aims to interpret. Moreover, Hesse famously maintains that to interpret foreign concepts implies the introduction of these concepts in terms of analogies and metaphors and, consequently, she argues that natural science cannot dispense with linguistic techniques of metaphors and modelling (cf. Hesse 1982: 707 and 709).The proposed paper aims to deal with the aformentioned issues and to address Hesse's account of scientific explanation from a less explored perspective. Special emphasis will be given to Hesse's view that "society interprets itself to itself partly by means of its view of nature. But nature is informed by human meanings and is subject, in its theoretical aspects, to hermeneutic methodology" (Hesse 1980: 186). ReferencesHesse 1980, Revolutions and Reconstructions in the Philosophy of Science, Indiana University PressHesse 1982, "Comment on Kuhn's 'Commensurability, Comparability, Communicability'," Philosophy of Science 2, pp. 704-711.Kuhn 1989, "The

Natural and the Human Sciences", in T. Kuhn, The Road Since Structure, University of Chicago.Kuhn 1982, "Commensurability, Comparability, Communicability", Philosophy of Science 2, pp. 669-688.Habermas 1971, Knowledge and Human Interests.

Green, Christopher
Jennifer, Bazar

Moritz Schlick's Student and the Popularization of Philosophy in the US: Dagobert Runes

Abstract: For nearly half a century, Dagobert David Runes was a key editor and publisher of philosophical literature for the masses (and sometimes for the experts) in the United States. His ambit included philosophy of science, but extended well beyond those boundaries to general philosophy, psychology (especially of the neo-psychoanalytic variety) and natural science. In addition to editing and publishing a number of magazines - Modern Thinker, Modern Psychologist, Current Digest, among others - he organized a lecture series in New York City called The Institute for Advanced Education where prominent intellectuals presented their ideas to a general audience. By selling Current Digest to the larger Reader's Digest in 1940, he was able to found a publishing house of his own: The Philosophical Library and, through it, to produce reference books such as the Dictionary of Philosophy, Who's Who in Philosophy, ATreasury of Philosophy, and APictorial History of Philosophy. He did not restrict himself to popular material, though. He was also the founding publisher of Philosophic Abstracts and of The Journal of Aesthetics and Art Criticism.Runes' specific relevance to the HOPOS community stems from his having earned his PhD at the University of Vienna under the supervision of Moritz Schlick in the early 1920s. Runes' dissertation was on the ethics of Plato and Spinoza. It was completed in 1924, precisely the time that Schlick was first convening the core members of what would become the Vienna Circle: Hans Hahn, Philipp Frank, Otto Neurath, and others.Runes was born in 1902 to a Jewish family in East Galicia, on the northeastern edge of the Austro-Hungarian Empire. The region became a war zone during the Great War and many residents, including Runes' family, fled to Vienna. The post-war Austrian Republic was dominated politically by the anti-Semitic Christian Social Party. Runes, who was 18 in 1920, entered Law School at the University of Vienna. He also led an "Ethical Seminar" that studied the religious thought of Constantin Brunner, a German-Jewish theologian who promoted the philosophy of Spinoza. Runes also took up the politics of "Red Vienna," a stronghold of the Social Democratic opposition to the conservative government of the day: he authored a book on the intensification of anti-Semitism, so irritating to government authorities that his publisher was jailed and he felt forced to flee the country. Runes landed in New York City in 1928. He worked in the magazine industry for a few years before the bankruptcy settlement of his employer resulted in his acquisition of two struggling magazines. Drawing upon his impressive network of European acquaintances - Alfred Adler, Albert Einstein, Emil Ludwig, Havelock Ellis, and Bertrand Russell, among others - he gradually leveraged these connections into a self-sustaining publishing business that focused on making cutting-edge intellectual pursuits accessible to an educated public. He also continued to publish his own writings on the topics of Jewish culture and anti-Semitism. He retired from his publishing company in 1976, and he died in 1982.

Gronda, Roberto

Dewey on Induction

Abstract: Although a rather unexplored topic in recent scholarship, Dewey's account of induction is a fairly distinctive feature of his philosophy of science. His account is so idiosyncratic that, as May Brodbeck has pointed out, not only does Dewey's theory of induction seem at odds with the received views; it also seems incompatible with the common understanding of the practice of science (Brodbeck 1947, 1949). And yet Dewey's theory of induction, as formulated in Chapter XXI of his Logic, is presented as the result of a much-needed revision of the outmoded logical conceptions "formed prior to the development of scientific method" (LW12: 415).The goal of this talk is to reconstruct in broad strokes Dewey's theory of induction. A genealogical approach will be adopted to show how Dewey's views on the subject emerged and developed over the course of his career. There are three main steps in such a process of development. The first extensive analysis of induction traced back to his Psychology (1888), where induction and deduction (the two are almost invariably conceived of, and discussed, as a dyad in all Dewey's works) are presented as "different aspects of the same self-developing activity of mind" (EW2: 197). While deduction is described as a synthetic activity, induction is defined as an analytic activity that "ends in rendering its object more unified, more identified with other objects, i.e., more connected with them" (EW2: 197). The characterization of the logical properties of induction and deduction is odd, both conceptually and terminologically: it is rooted in 19th century idealistic logic, in which Dewey was very well versed.I will then focus on the two editions of How We Think (1910 and 1933). The two editions differ in many respects: for my purposes, it is worth noting that Dewey substantially revised Chapter VII of the 1910 version, entitled "Systematic Inference". In the first version, the subtitle of the chapter was "Induction and Deduction" (once again, they were coupled together to form a single subject), while in the 1933 version the subtitle was changed to "Control of Data and Evidence", with the title of the chapter slightly changed to "Systematic Method" (and relocated as Chapter XI). I will show that the change in the subtitle reflects some deeper terminological and conceptual changes: the notion of "act of thought" – of which the inductive and deductive movement are the two constitutive moments, in close continuity with the thesis formulated in the Psychology – is dropped, and the importance of the notion of induction is significantly reduced.Finally, as hinted at above, in Logic a chapter is once again devoted to the analysis of induction. Moreover, Dewey discussed such a topic at length with Sidney Hook and Joseph Ratner in a number of letters written during the writing of this book. I will examine all this material in order to provide a more in-depth view of Dewey's mature account of induction, with an eye to identifying the sources (Mill, Stebbing) on which he relied in his work.

Guidi, Simone

Immaterial Extension, Light and Geometry: Marin Cureau de La Chambre against the Cartesians

Abstract: Marin Cureau de La Chambre (1594–1669)'s stands as one of the last attempts to champion a qualitative understanding of nature in the seventeenth century. Pivotal to his argument is the role attached to light, which constitutes a critical point of contention in contrast to the prevailing mechanical views of his contemporaries. This perspective was first put forth in 1634, when Cureau published his first work, the Nouvelles pensées. This work features a Discours sur les causes de la lumiére where he sets out a theory of light that is strongly committed to Neo-platonic themes and to the Italian Renaissance tradition (Ficino, Scaligero). According to Cureau, light can be explained within his theory of immaterial extension, for which a body is extended by virtue of its form, and its coarseness and subtility depend on being its form deprived of or filled with matter. However, La Chambre's more impactful effort to resist the rise of mechanism started just in 1659, with the reissue of his Discours within a more comprehensive work, the influential and extensive treatise on light La Lumiére. Subsequent to this pivotal publication, he continued to release other substantial works such as the Système de l'ame (1664) and the third part of L'art de connoistre les hommes (1666), where he staunchly advocated for his theory of immaterial extension. Such a relaunch of his philosophy of light and extension may be interpreted as an attempt to specifically counter the spread of Cartesian mechanical philosophy in France after the death of Descartes in 1650. Effectively, it intertwines with two elements. On the one hand, with the publication of the Latin (1662) and then the French (1664) posthumous editions of the Homme by Schuyl, Clerselier, and La Forge. On the other hand, with the circulation of Henry More (1614–1687)'s ideas in France through his epistolary exchange with Descartes published in the Clerselier edition in 1657. In this paper, I outline the view of Cureau, with particular attention to his theory of immaterial extension, the nature of light and images, and his view about the relationship between geometry and optics. Then, I discuss the role Cureau played in prompting the formulation of Pierre Fermat's 'principle of least time', suggesting that it was part of his overall strategy against the Cartesians.

Gutiérrez García, José Antonio
Daniel, Nieto Camesella

Du Châtelet on the construction of a new methodology: from Leibnizian-Wolffian principles to the concept of science

Abstract: Émilie du Châtelet's contributions to the philosophy and methodology of science are characterized to a large extent by her reception of the thought of traditional authors. A paradigmatic example is that of Wolff, from whom she incorporates fundamental elements. This is the case with her conception of the principles of rationality (especially the principle of non-contradiction and the principle of sufficient reason). This conception is essentially related to the delimitation of the field of study of disciplines such as philosophy and the various sciences. In this work, we propose to analyze the concept of science in Châtelet; precisely, how it is articulated through the incorporation and modification of the principles of Wolffianism. In a second section, we will study the status of Wolff's principles with special attention to some problematic aspects of their interpretation in the context of the reception of Leibnizianism, which will allow us to study Châtelet and her contributions from a broader historical perspective.Châtelet understood physical science as an investigation that should consist solely in the study of the empirical, without attending to the first causes of the phenomena that are given to us. In this respect, Châtelet's contribution is fundamentally methodological. Thanks to her conception of the PNC and PSR, Châtelet develops a concept of method that is especially novel in the physical sciences (even anticipating ideas such as the "degree of confirmation" of the Vienna Circle or Popper's "falsificationism"). In her Institutions de Physique, the author takes up the Wolffian approach to philosophical methodology, namely that philosophy should follow the model of mathematics. While Châtelet makes use of mathematical resources through geometry, Wolff simply posed this desire. Wolff wanted to incorporate mathematical principles, especially algebra, to ground his philosophical method. In Châtelet, it is clear that the mathematical method plays an important role, since the truths of physics or natural philosophy must be susceptible to mathematical description. In addition, while Wolff gave priority to algebra in his Psychologia empirica to discover hidden truths, Châtelet gives it to geometry..With regard to the incorporation of the principles of Wolffianism, it is important to analyze the historical context of Châtelet's contributions. A common misunderstanding in the study of Wolff is to identify the main points of his philosophy with those of Leibniz's. If this were the case, the attributions about the novelty of Châtelet with respect to Wolffianism should also be valid with respect to Leibniz's philosophy. However, there are some inconsistencies that are important to highlight here. The Wolffian notion of "metaphysics" establishes a field of application governed by a hierarchical conception of the principles of rationality. This conception extends the domain of metaphysics to the realm of the logically possible and relegates some principles such as the PSR to a subsidiary position. However, in this talk, we will analyze how there is a certain indeterminacy in Leibnizianism as to how this relation

between principles is understood. This will help us to specifically contextualize Châtelet's contributions and the historical framework of her reception of Wolffian thought.

Hamid, Nabeel

Christian Wolff as a theorist of what would later be called the 'Scientific Revolution'

Abstract: This paper examines Christian Wolff's views on the significance of the new sciences of the seventeenth century. It draws attention to his various narrative accounts of developments in natural philosophy, from which he extracts certain philosophical lessons. These narratives concern, for instance, the theories of corporeal substance and force (in Ontologia §761; §773; and Cosmologia §§359-61), empirical method (in prefaces to the second edition of Johann Christoph Sturm's Physica electiva, 1722, and to a German edition of Steven Hales's Vegetable Staticks, 1748), and the rise of Newtonianism (in his correspondence with Count von Manteuffel from the 1740s). The paper argues that Wolff's narratives represent an early case of the historicization of seventeenth-century natural philosophy as constituting a singular episode of dramatic intellectual consequence, or what later generations would dub 'revolution in science' and, eventually, 'the Scientific Revolution.' Wolff's historical-philosophical reflections lead him to the following positions. First, he rejects instrumentalist appraisals of the new sciences, that their value consists chiefly in their potential, e.g., to improve the material conditions of humanity, or to buttress religion, in favor of a robust semantic realism about their theoretical claims. Second, he sees certain developments in metaphysics, notably in the doctrine of substance, as equally as important as experimentation and measurement in producing the new state of natural science (in this regard, his opinion agrees with those of Hatfield 1996 and, to a lesser extent, Floris Cohen 2010). These positions gradually lead him to rethink the relationship between metaphysics and empirical science. Briefly, for the later Wolff, metaphysics acquires a new, propaedeutic role of guiding empirical inquiry by, for instance, sifting out illegitimate speculations, or identifying implausible hypotheses. In doing so, Wolff accords an unusual degree of autonomy to the special sciences to conceptualize their domains. At the same time, he insists that metaphysical reflection on the results of these sciences remains indispensable. From a historiographical interest, reading Wolff as a historian and philosopher of science suggests that, even though 'Scientific Revolution' did not become an actor's category until the turn of the nineteenth century, as I. Bernard Cohen (1976) showed, the historical conceptions which that term was devised to capture bear strong resemblance to ones already theorized by early observers of the period between Copernicus and Newton. While the label 'Scientific Revolution' has been the target of various criticisms in recent decades (e.g. Shapin 1996; Poovey 1998; Garber 2016), this paper concludes that it still remains valuable for understanding the earliest reception of seventeenth-century science. Wolff's case shows that the label remains useful as an analytical tool for historians, inasmuch as it picks out the judgment of at least some influential early modern observers concerning their own most recent past: that it could be understood as a single, extended intellectual event which resulted in a profound remaking of the study of nature.

Hanley, Brian

Three Problems of Cause-Condition Reasoning in Mill's System of Logic

Abstract: In System of Logic, Mill describes the distinction between "causes" and "conditions" as "capricious." Many interpret this as a claim that the distinction is illogical. Thus, most try to refute Mill by identifying a logical principle he must have overlooked that guides cause-condition reasoning. Here, I argue this common understanding of Mill's thought is a misinterpretation. I provide a new interpretation by situating Mill's discussion within his broader naturalistic philosophy of science in Logic.I begin by showing that Mill offers an analysis of the logic behind cause-condition reasoning. Then, I explain three problems Mill thinks this logic raises for scientists and philosophers. The first problem has to do with how cause-condition reasoning is apt to work against the precise and careful logic needed for scientific induction. The second problem is what Mill calls the threat of "logical disguise." The third problem has to do with how cause-condition reasoning adapts to different purposes and contexts.I close by arguing that despite his apparent pessimism, Mill's deeper thought offers a positive path forward. Based on his analysis, I lay out a pragmatic approach to analyzing cause-condition reasoning that retains Mill's naturalistic spirit in Logic. I conclude by explaining why this Millian approach can organize a more fruitful philosophical research program into this important kind of reasoning.

Harkema, Scott

Berkeley on the Metaphysical Foundations of the Calculus

Abstract: According to a common story in the history of mathematics, although Newton made great strides in applications of the calculus, his methods were not set upon a sufficiently rigorous foundation. Although Newton attempted to make his version of the calculus rigorous by banishing infinitesimals and introducing fluxions, he failed to give adequate definitions of fundamental concepts of his fluxional calculus. It is therefore thought to be unrigorous. As part of this story, George Berkeley's critique of the fluxional calculus in the Analyst is often depicted in a positive light, as an effective critique of its lack of rigor. Those who praise Berkeley's critique typically focus on his logical critique of the calculus, which argues the methods of Newton's calculus are logically inconsistent, and therefore unrigorous. This is to be distinguished from the often dismissed metaphysical critique, where Berkeley argues that such entities as "fluxions" and "nascent and evanescent increments" are metaphysically disreputable, and that therefore the calculus is unrigorous. The challenge facing interpretations that privilege Berkeley's logical critique is that if Berkeley intended it as a stand-alone argument against the fluxional calculus, it appears to be directed at a straw man, and thus is not as effective as typically supposed. Phillip Kitcher and David Sherry have both argued that the alleged inconsistency in method that Berkeley's logical critique identifies is not only anticipated by Newton, but furthermore sufficiently addressed, so that Newton's methods are in fact not contradictory. This is because the entire procedure of supposing an increment of a positive magnitude and then allowing it to vanish is rationalized by Newton's theory of prime and ultimate ratios, and the concept of limits contained therein. While not rigorous by today's standards, Newton's methods do not appear to contain the obvious inconsistency that Berkeley alleges. In this paper, I take Berkeley's argument against Newton's infinitary methods in the Principia as a case study, showing that his arguments against the methods of the Principia are deeply metaphysical, rather than merely logical, as has previously been assumed. If I am correct, the argument against the calculus of the Principia serves as evidence that we have misunderstood the structure of Berkeley's critique of the calculus and mistaken the importance of his metaphysical critique of the calculus. At stake is the question of whether Berkeley's critique of the calculus can withstand the objections from Kitcher and Sherry, who claim that Berkeley's critique relies on a misunderstanding of Newton's methods. If I am correct, Berkeley properly understood Newton's methods, and offered a more penetrating metaphysical criticism of these methods than a mere appeal to a logical contradiction in the method.

Harmon, David

Spinoza and the Vacuum: Plenum Physics as a Motivator for Necessitarianism

Abstract: Most scholars take Spinoza's relationship to the physics of the seventeenth century to be tenuous. Though there is good evidence that Spinoza had an interest in physics, he wrote very little about his own views. That said, close attention reveals that some metaphysical issues in Spinoza are deeply connected to patently physical concepts, like motion, inertia, collision, etc. This paper explores one such relationship. Spinoza denies the possibility of a vacuum in physical space several times throughout his works, but the arguments he offers against this possibility do not address physics or its laws. Instead, his arguments generally follow from deeper metaphysical principles, like God's simplicity. However, there are obvious ways in which the non-existence of a vacuum in extension is relevant for understanding the ways that bodies interact with each other according to a mechanistic physics. In this paper, I explore some of these connections in order to make one of Spinoza's most controversial doctrines more clear: his necessitarianism. There is a strong case to be made that Spinoza is committed to the view that all truths are necessary truths, or rather that things could not have been any other way than the way that they are. This has historically been a point of contention among Spinoza scholars, as necessitarianism is a difficult doctrine to accept on its face. Some recent work has been done to try and save Spinoza from this commitment, despite his ostensible endorsement of the view. Rather than try to help Spinoza escape from the view, I think Spinoza's denial of the vacuum can help his interpreters to better understand his motivation for accepting necessitarianism. Given a plenum physics (entailed by denial of the vacuum), a commitment to deterministic laws of motion, and a commitment to the view that all variety in the physical world can be explained only by motion, a necessitarian picture of reality might start to look more appealing. Since Spinoza is committed to all three of these (some of which he inherits from Descartes), his necessitarianism is perhaps not so surprising. I argue that Spinoza's acceptance of a plenum physics may have motivated him to accept a strict causal-physical necessitarianism. Though the denial of the vacuum does not strictly compel a commitment to necessitarianism, it highlights the conceptual difficulty in changing the arrangement of bodies in the world without causing downline contradictions. If I am right, then there is a close connection between one of Spinoza's physical doctrines (that there is no vacuum) and one of Spinoza's metaphysical doctrines (necessitarianism) that is not explicit in the text.

Harrop, Stephen

Reframing the Substantivalist/Relationalist Debate

Abstract: The substantivalism/relationalism debate concerning the nature of space (and in later accounts space-time) continues to occupy the attention of both philosophers of physics, science, and metaphysicians. It is also bound up with particular historical events and circumstances like the formulation of classical mechanics. Figures like Newton are taken to be substantivalists, who believe space is a sort of substance or quasi-substance, and others like Descartes and Leibniz are taken to be relationalists, who believe that space consists in the relations between material objects alone.I suggest that, while this debate is not exactly outmoded, it suffers from lack of conceptual accuracy and clarity. It lacks accuracy since, as some scholars (and I will) argue, the prototypical substantivalist Isaac Newton plausibly did not hold a view on which absolute space is anything like a substance. This complicates the view of substantivalism as being the avowal of something like absolute space (or maybe space-time structure). Further, if Newton is not a substantivalist, it is not clear what the debate is about since the main reason to believe in absolute or substantival space post-Newton was to give a proper analysis of true motion. Moreover, the prototypical relationalists Rene Descartes and GW Leibniz hold views that are so different in content and ontology that it seems both inaccurate and imprecise to call them both relationalists.I propose a new way to carve up the debate, as between reductionists and separatists. Reductionists about space think that there is nothing to spatial structure over and above the relations and structure that obtain(s) between material objects. Separatists about space deny this and postulate spatial relations and structure over and above the one(s) which obtain(s) merely between material objects. To use a figure of speech: The reductionist believes that all God had to create the physical world was make material objects, whereas separatists think that an additional creative act was needed. Importantly, the precise details of each ontology are left open: No assumptions are made regarding what space is on the separatist view, only that spatial relations and structure is not reducible to the relations and structure of material objects.Put this way, the substantivalism/relationalism debate emerges as a debate between one version of separatism and one version of relationalism, neither of which capture the history with strict accuracy. I claim that this division allows for a broader, more accurate analytical framework with which to look at key figures, one which lets us see crucial distinctions between them. I do this by examining two separatists (Newton and Isaac Barrow) and two reductionists (Descartes and Leibniz).

Hattab, Helen

Descartes on the Methods of Analysis and Synthesis

Abstract: Descartes' most detailed claims about the methods of analysis and synthesis are found in his Replies to the Second Objections of the Meditations. Secondary literature often connects these pronouncements about his philosophical method to the procedures Descartes employs in his scientific writings, e.g., as showcased in his explanations of the anaclastic line and rainbow. Underwriting such connections, one often finds the assumption that Descartes' philosophical uses of the terms 'analysis' and 'synthesis' derive respectively from the resolutive/analytic and compositive/synthetic phases of an Aristotelian demonstrative proof known as the regressus. This assumption leads to the search for a regressus style resolutive phase, followed by a compositive phase in Descartes' scientific explanations, as a means to articulating a consistent, unified method across his corpus. Interpreting the texts through the lens of regressus style procedures makes Descartes' method consistent over time. However, it comes at the cost of terminological confusion, since Descartes then appears to mean by 'synthesis' what he should call 'analysis' and vice versa.This paper shows that prior philosophical senses of 'analysis' and 'synthesis' circulating in Descartes' intellectual environment in the Netherlands include late Scholastic Aristotelian views of these methods that fit the claims in the Second Replies and related passages much better than the senses derived from the regressus. Specifically, the highly influential logician Zabarella (who also developed the regressus) associates analysis and synthesis not with demonstrative method but with a branch of method he calls 'order'. For Zabarella, analysis and synthesis are didactic orderings rather than methods of discovery. However, Protestant logicians who were influential in the Netherlands in the first half of the 17th century took up Zabarella's view of method as order and modified it into a natural method. By interpreting the Second Replies and related passages in the light of these Protestant logic texts found in Descartes' intellectual environment, one arrives at a far more coherent interpretation of how he characterizes these methods in the context of the Meditations.The question remains whether this more coherent interpretation of how Descartes characterizes analysis and synthesis in the context of the Meditations allows us to map his philosophical method on to a general method also at work in his early scientific writings. Answering this question would require more extensive investigation. As a preliminary step, I argue that, notwithstanding Descartes' early claims to have found a unified method, one must exercise caution to avoid the presupposition that methods on display in the explanations Descartes gives of natural phenomena must conform to what he later says about analysis and synthesis in the context of the Meditations. There are different ways to understand Descartes' claim to have one unified method as well as debate about the extent to which the early method laid out in the Rules for the Direction of the Mind and Preface to the Discourse on the Method survives intact in his mature philosophy. Moreover, Descartes admits that how he presents his scientific explanations in his published works need not faithfully follow his method of discovery.

Heidelberger, Michael

“Leibnizian Thoughts”: How Emil du Bois-Reymond dissolved the empiricism-nativism controversy against (but also with) Helmholtz

Abstract: In volume III of his Handbook of Physiological Optics, Helmholtz introduced the dichotomy between "nativism" and "empiricism" into the theory of human perception. On one hand this move helped him to summarize, reorganize, and to some extent simplify, the earlier and ongoing discussion. On the other hand by identifying his own point of view with empiricism and by rejecting nativism as incorrect, superfluous or unscientific he made his theory stand out clearly against rivals and provided it with an ideological dimension that was in accordance with the programme of the organic physics of 1847. Helmholtz singled out the physiologist Ewald Hering as the arch-nativist – a choice that led to one of the most famous scientific controversies of the 19th century. The story is well known. So it is all the more surprising that nobody seems to have noticed so far that Emil du Bois-Reymond, the closest companion of Helmholtz, also in matters scientific, argued for the dissolution of the dichotomy and even for a reconciliation of the two conflicting positions. He did so in his address of 1870 on Leibnizian Thoughts in Modern Science (to which Helmholtz never reacted) and in his obituary for Helmholtz from 1895. In the first part of this presentation, I shall give an account of du Bois-Reymond's argument: He claims that empiricism is improbable in view of the quick control animals gain of their limbs and senses in their development. Pre-established harmony, as advocated by nativism is ruled out but Darwin's theory can "cast off from pre-established harmony its supernatural robe that it has worn since the times of Leibniz". It enables to understand innate ideas as natural traits inherited from generation to generation, and to allocate the individual learning process postulated by empiricism to an "immeasurable series of genera" that preceded the individual. Empiricism thus becomes an empiricism of the ancestors, and nativism concerns the inherited traits of the individual. "In this way", du Bois-Reymond adds, "pre-established harmony is, as it were, incorporated into the mechanical world process." Next, I shall investigate the role that pre-established harmony played in the thought of Johannes Müller, Helmholtz and du Bois-Reymond. I shall introduce some distinctions that allow more determinate judgments on the views at stake, including those of Leibniz himself.In the third part, I shall defend the view that du Bois-Reymond's argument against Helmholtz's rigid notion of empiricism has only been made possible in the first place by Helmholtz interpretation of Darwinism (1869): "Darwin's theory contains an essentially new creative thought. It shows how adaptability of structure in organisms [Zweckmäßigkeit der Bildung in den Organismen] can result from a blind rule of a law of nature without any intervention of intelligence." The fourth part shortly deals with Ewald Hering's speech on Memory as a General Function of Organised Matter of 1870. This address, I claim, fits squarely into the preceding discussion, and its treatment is suitable to shed additional light on the covered topics.

Helbig, Daniela

Philosophical-Scientific Borderlands: Kantian Legacies in Ilse Rosenthal-Schneider's Work in Australia after the Second World War

Abstract: An emigrant to Australia from Nazi Germany in 1938, Ilse Rosenthal-Schneider brought with her a distinctive interest in the continuities between scientific and philosophical questions. The rhetorical insistence on the relevance of the empirical sciences for philosophical inquiry had been at the core of the Neo-Kantian construction of a disciplinary genealogy beginning with Hermann Helmholtz, put forward among others by Rosenthal-Schneider's supervisor Alois Riehl. She reverses the relation in her doctoral dissertation published in 1921, articulating an understanding of the relationship between philosophy and the empirical sciences that would continue to inform her work after her emigration, as I argue in this paper.In Rosenthal-Schneider's reading, Einstein's theory of relativity illustrates Kant's argument for the possibility of scientific knowledge, and it points to the need to clarify the epistemological implications of scientific theories. She carried her view of the inevitably philosophical character of proper scientific inquiry into her work in Australia, where she argued for forms of scientific practice that were grounded in explicit epistemological presuppositions. Her main site for this argument was a series of articles published in the Australian Journal of Science in the late 1940s and early 1950s, engaging among others with astronomer Arthur S. Eddington, biologist Erwin Schrödinger, and Marxist historian of science Boris Hessen. This aspiration for philosophically informed scientific practice went hand in hand with Rosenthal-Schneider's extensive public engagement in Australia. In a series of lectures held over two decades, she travelled across rural New South Wales and Victoria to spell out the wider societal implications of modern science-work that served to carve out a career as a woman philosopher in post-war Australia, but that is also a legacy of the systematic Kantian ambition to reflect on the connections between science and moral practice. However, Rosenthal-Schneider's emphasis on social and moral concerns to do with the sciences, and her view of philosophy of science as ideally embedded in scientific practice put her at odds with contemporaneous efforts to institutionalize the philosophy of science as a discipline linked to and reliant upon, yet separate from science itself, and as discipline that largely avoids engagement with matters of social and moral concern; a prominent example of an advocate of this ultimately successful professional positioning is Rosenthal-Schneider's former fellow Einstein student Hans Reichenbach.

Henne, Céline

John Dewey on Generality

Abstract: In his 1929 paper "General propositions and causality", Frank Ramsey diagnosed a problem in the early Wittgenstein's views of open generalizations (such as "Arsenic is poisonous" or "All men are mortal") as infinite conjunctions. Against the Tractarian view, he argued that open generalizations should be understood as "rules for judging" rather than as statements of fact. Ramsey's idea quickly became influential among the logical positivists, who adopted an account of general propositions (including laws of nature) as hypotheses or instruments. While Dewey is not usually included in the history of the debate on the nature of general propositions, he made an important contribution to this issue, although one that was largely overlooked. In this paper, I show the superiority of Dewey's view of general propositions over that of Ramsey and the logical positivists, and defend its importance for an account of the practice of science. The paper is structured as follows: I begin by presenting Ramsey's account of general propositions as rules with which we "meet the future." I identify two competing developments of his view, both of which can be found in its reception by the logical positivists. The "dichotomy interpretation" (e.g. early Carnap) tends to emphasize the distinction between genuine propositions having existential reference and instruments or rules for forming propositions (causal laws, scientific laws). The "global interpretation" (e.g. later Wittgenstein, Waismann, later Carnap) interprets all propositions as hypotheses or instruments of prediction. Both have problems: while the former maintains the Tractarian picture for some propositions, the latter tends to erase the important functional differences that motivated Ramsey's picture in the first place. I then turn to Dewey's (1938, LW 12) functionalist theory of propositions, focusing in particular on his distinction between existential and universal propositions. I argue that it avoids the problems of the dichotomy and the global interpretations of Ramsey's view: it takes into account the different functional roles of propositions, including propositions that state matters of fact and propositions that function as rules, without falling back into the representationalist picture of the Tractatus. What is more, Dewey's account distinguishes between additional kinds of generality that are neglected in the Ramseyan and logical positivists' accounts. Dewey's distinction between existential and universal cuts across the class of general propositions and can explain why general propositions having the same logical form (e.g. open generalizations) "meet the future" differently in several respects: (1) the role of empirical evidence in their (dis)confirmation; (2) their existential or abstract reference; (3) their role in warranting judgment. Dewey's suggestion resurfaced 25 years later in Wilfrid Sellars' "Abstract Entities" (1963)-although no credit is given to Dewey.

Hofer, Veronika

Philipp Frank and Moritz Schlick on the disciplinary integration of biology

Abstract: In this paper I provide a deeper look at the discourse in biology at the time, when Philipp Frank was in charge of organizing the talks and essays for the conferences and volumes for the Vienna Circle concerning biology. I will also discuss the philosophical background and historical circumstances why Moritz Schlick enthusiastically supported Ludwig von Bertalanffy's attempt to establish his newly developed Theoretical Biology, which had emerged from the earlier Systems Theory, by an extra ordinariate at the Philosophical Faculty of the University of Vienna. I will give an account of the epistemological situation in Vienna and in Prague (where Frank taught since 1912), of the local contexts of research institutions, of the main research problems they were dealing with, and of the public and political figures that dominated the field of biology in the period from 1904 to1938. My focus will be the question whether Schlick and Frank developed conceptions of knowledge in biology different from physics, when they analyzed theories, concept formation and methodologies in biology, given that both physicist-philosophers had their scholarly background in the problems and challenges prompted by the new physics. One of the main topics of the 1930s was the concordance among different accounts of biological phenomena and the integration of the multiplicity of biological disciplines by way of sound and clearly formulated questions. What were the epistemological standards of progress that Frank and Schlick brought to these questions, what were the obstacles they saw to the objective of a disciplinary integration of biology, what outlook did they have on specific concepts, such as system, entelechy, and Ganzheit. Did they, and if so, what unifying steps did they welcome, what kind of biological concept formation did they oppose and on what grounds? Addressing these questions will add an important aspect to the already very complex historical picture of Vienna Circle.

Holcblat, Alain

Helmholtz’s and Cassirer’s structuralisms

Abstract: To what extent can we find similarities between Hermann von Helmholtz's and Ernst Cassirer's philosophy of sciences facing the scientific revolutions in logic, mathematics and physics that started in the late 19th century? In spite of his rather empirist epistemology, calling on physiology and psychology, Helmholtz was an inspiring source for the latter because of what could be called his protostructuralism. Indeed, both found in structuralism resources to solve the problems raised by their common rejection of the given of intuition and the copy theory ("Abbildtheorie").Cassirer is indebted to Helmholtz, mainly for the sign theory and their convergences about geometry. In sciences of nature, Cassirer opposes the copy theory and presents in a favourable light the sign theory, which holds that we cannot know the things in themselves but only the causal relations between them. Concerning the new geometries of space that challenged the Euclidian geometry in the mid-19th century, Helmholtz eventually rallied to the transcendental aspect of space and introduced the distinction between pure geometry and geometry of the physical space in "Ueber den Ursprung und die Bedeutung der geometrischen Axiome". Finally, Helmholtz claims to be Kantian, but, contrary to Cassirer, granted a very limited room to a priori faculties and knowledge, which are basic constituents of a Kantian philosophy.Cassirer distinguishes itself from other neo-Kantians by abstaining from a complete rejection of intuition, whereas the other ones integrate it into understanding. Following Helmholtz, he keeps space as a pure form of sensibility in « Zur Einsteinschen Relativitätstheorie ». Like the other neo-Kantians of the Marburg school, Cassirer postulates the existence of a priori categories, and, among them, gives the first place to the category of relation, which a mark of structuralism. His structuralism is more systematic than Helmholtz's. Contrary to Helmholtz's structuralism, sometimes bordering on analogy, Cassirer's is a mathematical structuralism, close to the structuralism that emerged later in human sciences. Finally, unlike Helmholtz, who had no interest in history of sciences, Cassirer shared with the other neo-Kantians from Marburg a deep interest for this history, which they judged inseparable from philosophy of sciences. In "Substanz und Funktion", he presents this history as an evolution from the conceptualisation by substance to conceptualisation by function. In summary, Cassirer's structuralism presents analogies with Helmholtz's protostructuralism, but brings also significant new points when compared to the latter. This can be explained by other important influences from the other neo-Kantians (Hermann Cohen and Paul Natorp), the founders of modern logic, in particular Bertrand Russell, as well as from the physicists-philosophers, Pierre Duhem and Max Planck.

Holvoet, Marjolein

French Mathematical Philosophy: Albert Lautman's Platonic Dialectic of the Concept

Abstract: Recent years have witnessed a resurgence of interest in French philosophy of science. Yet, within this intellectual renaissance, one intriguing area still deserves further in-depth exploration: French mathematical philosophy. Emerging from Léon Brunschvicg's seminal 1912 work 'Les étapes de la philosophie mathématique,' this particular strand of philosophy of science formed the epistemological landscape for the ideas of the mathematician-philosophers such as Jean Cavaillès, Gilles Gaston Granger, Jean-Toussaint Desanti, Jules Vuillemin and Albert Lautman. Albert Lautman, although a direct disciple of Brunschvicg and a friend of Cavaillès, occupies a unique position within this group. His philosophical pursuits extended beyond the dynamics of mathematical reason and encompassed also the ontological implications of mathematics. Drawing inspiration from antique and classical conceptions of rationality, Lautman firmly rejects the reduction of mathematics to a purely logical enterprise. Instead, he grants mathematics its autonomous development through creative progressive conceptualization. However, being a close friend of the mathematician Herbrand and an interested reader of both Russell and Hilbert, Lautman does not dismiss logic. His interaction with logic leads him to reinterpret Hilbert's metamathematics and axiomatic structuralism – a formal system with its structural properties and its material realizations or models – in terms of an idiosyncratic Platonic dialectic of the concept. Challenging foundationalist approaches in the philosophy of mathematics and rejecting the strict separation between form and content, Lautman's mathematical Platonism revolves around movement and theory dynamics in mathematics rather than conceiving it as a realm of immutable forms. At the heart of Lautman's philosophy lies the idea that mathematical reason provides a unique gateway to understanding reality and existence. As such, his epistemological characterization of mathematics as conceptual becoming extends into the realm of metaphysics. This bringing together of mathematics and metaphysics is, in his view, necessary and not contingent, and it is the task of mathematical philosophy to 'remake the Timaeus' and 'make metaphysics depend on the mathematic.' Unfortunately, Lautman's philosophical endeavours were brutally cut short by World War II, leaving his body of work constrained.This paper proposes to engage with Lautman's thinking, introducing a Platonic dialectic of the concept that goes all the way down to the surface of the sensible and inverses the Aristotelian logic of substance. Firstly, I provide an interpretation of Lautman's dynamic perspective on mathematics, intricately connecting it to Hilbert's axiomatics and the mathematical philosophy of Léon Brunschvicg. Secondly, in accordance with the critical and historical method of Léon Robin, Lautman's Professor of Ancient Philosophy at the Sorbonne, I situate Lautman's dynamic perspective on mathematics within a Platonic framework that can be traced back to Plato's innermost teachings, known as the unwritten doctrines or 'agrafa

dogmata'. In conclusion, I suggest, following Lautman's lead, a fresh perspective on Plato's dialectics and the interplay between metaphysics, mathematics, and reality.

Hommen, David

Wittgenstein and Hesse on Metaphors and Family Resemblance

Abstract: As part of her seminal work on models and analogies in science, Mary Hesse develops a general 'network theory of meaning' in which she posits that all language, including scientific language, is fundamentally metaphorical. For this, she takes her cue from Ludwig Wittgenstein's theory of family resemblance (cf. PI, § 67), according to which general terms are applied to their diverse instances not on the basis of essential features common to them all, but rather on the basis of complex similarity relations (cf. PI, § 66). In this view, Hesse claims, any application of a general term to a novel case amounts to a metaphorical transfer of that term from its familiar domain of application (the source, as it were) to the new case (the target) – a process which always alters, however slightly, the meaning of the term (cf. Hesse 1988, 2). Whether the reapplication continues to be judged metaphorical or whether the new case is ultimately incorporated into the literal meaning of the term is more a matter of pragmatics than semantics, according to Hesse (cf. ibid., 3). Thus, in a sense, "all language is metaphorical" (ibid., 1). As Hesse explains, the metaphorical transfer of terms is based on the recognition of antecedent similarities between their old and the new cases of application. But these similarities are not, and cannot be, stated in literal (stable, univocal) language; for, there is, in Hesse's view, ultimately no such language. As she says, "[...] similarity and difference are irreducible primary relations, prior even to application of the simplest predicate: they are shown and not said" (Hesse 1988, 7). In this way, the network theory of meaning manages to preserve a nonparaphrasability and hence indispensability of metaphors while providing objective criteria of adequacy for them and hence a sense in which metaphors can be true or false. In this talk, however, it will be argued that Hesse's cognitive theory of metaphor relapses into a constructivist antirealism. For Hesse ultimately regards the similarities based on which all descriptive concepts are applied not as objective features of the world, but as a matter of psychological or social agreement (cf. Hesse 1985, 40). Thus, the truth values of the resulting classifications remain culturally relative and mind-dependent. In contrast, it will be shown that Wittgenstein's original theory of meaning already contains a theory of perception, developed from his private language argument and the famous rule-following paradox, that guarantees genuinely epistemic access to objective resemblances in the world. This access is established through a direct acquaintance – what Wittgenstein calls "familiarity" (PG, § 37) – of the conceptual agent with such resemblances, which is manifested in their competent (also linguistic) interaction with the objects of the corresponding resemblance classes and is neither conceptually nor phenomenally mediated. Wittgenstein's overall theory thus proves to be more realist than Hesse appreciates in her own interpretation and development of it, and can indeed help to redeem the realist program of Hesse's approach.

Howard, Stephen

Kant on the Regulative Use of the Cosmological Ideas for the Investigation of Nature

Abstract: This paper examines how, on Kant's account, the cosmological ideas have regulative utility for the theoretical investigation of nature. Recent scholarship has valuably attended to the regulative use of the transcendental ideas (Massimi 2017, Zuckert 2017, Kraus 2020, forthcoming). However, these accounts aim to reconstruct the regulative utility shared by all three transcendental (or speculative) ideas: the soul, world-whole and God. Few commentators have considered the regulative use of the cosmological ideas in detail, or noted how this differs from the regulative use of the other two classes of transcendental ideas (although cf. Pissis 2012, 170-1, Fœssel 2008, 133-8, Rauscher 2010, 299).In sections 8 and 9 of the Antinomy chapter, Kant describes a regulative principle that results from the cosmological conflicts. This principle instructs reason not to posit an absolutely unconditioned member of any series of conditions for given appearances; we should expect always to find further empirical conditions (A508-9/B536-7, A561/B589). Reason should proceed indefinitely when investigating the various series of conditions for any given conditioned thing (A510-11/B538-9).This regulative principle directs us to avoid "lazy reason" (ignava ratio), which is "any principle that makes one regard their investigation into nature, whatever it may be, as absolutely complete, so that reason can take a rest, as though it had fully accomplished its business" (A689-90/B717-18). It is striking that, with regard to theoretical explanation of nature, one side of each antinomial conflict seems to succumb to lazy reason, while the other side guards against it by affirming the regulative cosmological principle. As Kant writes, the 'dogmatist' or 'Platonist' thesis position in each conflict allows reason to "indulge in ideal explanations of natural appearances, and to neglect the physical investigation of them". The opposed 'empiricist' or 'Epicurean' antithesis position, by contrast, "encourages and furthers knowledge" of nature (although "to the disadvantage of the practical") (A472/B500).We might thus consider the regulative use of the cosmological ideas to stem only from the antithesis position of each antinomy, spurring indefinite scientific investigation into each series of conditions. However, in this paper I will argue that the picture is more complicated. Even if we consider only reason's theoretical interests, we can conceive of a regulative use of the thesis position of each antinomy. Positing a temporal beginning of the world motivates research in cosmogony. Positing a spatial boundary of the world spurs scientists to explore whether this exists and how it might be determined. Positing atoms can be helpful (even if not necessary) for mathematical explanations of matter, as Kant suggests in the Metaphysical Foundations (4:532-3). And positing free causality and a necessary being grounding the world seems to have regulative utility similar to the psychological and theological ideas, regardless of whether our theoretical curiosity about the soul and God can ultimately be satisfied. I aim to show that the regulative use of

the cosmological ideas, because connected to the opposed claims of the antinomies, is significantly more complicated than that of the psychological and theological ideas.

IJdens, Nina

Serving Life in the USA: Socio-Political Representation of Logical Empiricism in the Early American Context

Abstract: In the last three decades, historians and philosophers have rediscovered how the logical empiricist movement was driven by modernist motives to transform the social and political forms of life of the interbellum period. This can be seen in its relation to architectural modernism, socialist-inspired adult education or the transmission of scientific knowledge to a wide audience through new media such as radio. These modernist motives have been well documented in the German-speaking countries of origin of the movement in the 1920s and 1930s. However, through the work of George Reisch, the literature on logical empiricism's trajectory in the USA shows a different image of the interaction between the movement and its modernist ambitions. In an intellectual climate heavily influenced by Cold War dichotomies between individualist and collectivist, liberal and totalitarian, open and closed, logical empiricists had to redefine their self-understanding to avoid suspicions of leaning towards the wrong end of these poles. Rather than the movement actively intervening in the political discussions of the day, then, political moods intervened into the movement, putting pressure on members to focus solely on technical philosophy in order to protect their livelihood and their academic reputation. While the logical empiricists opposed totalitarian forces in Vienna in the 1930s, in the USA of the 1950s they ran the risk of being perceived as such themselves.Less attention has been paid to the movement's political representation in its early years of exile. While Reisch describes the logical empiricists' relations with socialist intellectual circles in New York in these years, little is known about their adaptation to and reception in the wider political arena. In this paper, I will investigate the socio-political representation of logical empiricism in the US public sphere between 1935 and 1945, beyond the narrow confines of their reception in socialist/communist New York circles. The logical empiricists' manifesto strongly emphasizes the spirit animating their philosophy - what Helmuth Plessner referred to as the political a priori of a philosophy. Did the logical empiricists negotiate about their raison d'être in their new context, and how so? Did they reformulate their project such as to 'adapt' to the web of socio-political associations dominating the American context, when cold war dichotomies were not as prevalent? And how were they received beyond academia?To this end, I will first systematically investigate all American correspondents in Otto Neurath's network to see how Neurath conveyed his movement both to American academics, social organizations, and news outlets. I will also inquire how Neurath, Carnap and Morris negotiated about the public representation of their movement in the USA through a discussion of correspondence. Next, I will look into the articles about logical empiricist philosophers in American news outlets and discuss transcripts from radio talks of logical empiricists, like Reichenbach and Hempel. Finally, I will investigate the relation between the Institute of Design in Chicago and the Chicago logical empiricists, Rudolf Carnap and Charles Morris.

Ioannidis, Stavros

Reconsidering the Structure of Darwin's 'Long Argument'

Abstract: Darwin describes the Origin of Species as 'one long argument'. The exact structure of this argument has been the subject of controversy among historians and philosophers of biology. For some, Darwin's method is hypothetico-deduction; for others, inference to the best explanation, Herschel's method, or Whewell's consilience of inductions. In this paper, after briefly reviewing the main features of two accounts of the methodological structure of the Origin, the traditional account as developed by Jonathan Hodge (1977 and in a series of publications) and Elliott Sober's (2011) more recent account, I will propose a new analysis that differs from both.I will argue that a satisfactory account of Darwin's argument must explain the following two prima facie strange features of the structure of the Origin. First, the fact that the hypothesis of common descent, as well as the evidence that supports it, are presented in the last part of the book; while the theory of natural selection, which is the cause of evolution, is presented at the beginning. Second, the fact that in the last part of the book Darwin seems to compare the theory of special creation with the theory of common descent, and not with the theory of evolution by natural selection. I will claim that Hodge's and Sober's analyses cannot satisfactorily explain these features of Darwin's argument, and that an alternative analysis is possible which retains some of the positive features of the two previous accounts, but at the same time explains these two enigmatic features of the Origin and highlights the unity behind the structure of Darwin's argument (contra Waters 2003).According to the account I propose, the evidence that supports the theory of common descent, presented in the third part of the book, can only satisfactorily support the theory only after the theory of natural selection has been regarded as an in principle possibility. In other words, in order for Darwin to be able to successfully support the hypothesis of common descent, he must first propose a plausible theory of organismal adaptations. Such a plausible theory is the theory of natural selection.Moreover, I will argue that the proposed analysis has consequences for both evidentialism and Bayesianism as applied to Darwin's argument. Concerning evidentialism: the introduction of the hypothesis of natural selection increases the degree to which the (previously known) evidence supports the hypothesis of common descent. Given evidentialism, then, the hypothesis of natural selection as an in principle possibility must be regarded as part of the evidence. Concerning Bayesianism: the posterior probability of the hypothesis of common descent after the introduction of the theory of natural selection is higher than its posterior probability before the introduction of the theory; but since the difference does not involve the discovery of a new fact, a simple bayesian analysis fails. The central importance of the theory of natural selection for the full support of the hypothesis of common descent, then, explains the structure of the Origin and sheds new light on Darwin's 'long argument'.

Jalobeanu, Dana

Bacon’s Democritus: fictional history of philosophy in service of the scientific method

Abstract: This paper aims to discuss Francis Bacon's strategy to create a philosophical persona of the experimental philosopher using ancient and modern sources relating to Democritus, philosopher, magus and "inquisitor of truth." To date, Bacon's positive assessments of Democritus have been attributed to an interest in atomism, or to an attempt to appropriate the persona of the moral philosopher for the purposes of natural philosophy. By contrast, I show that Bacon's project was quite different: it was a historical and philosophical reconstruction of a Democritian tradition intended to ground the experimental method. This is why, I claim, quite a number of technical terms of Bacon's methodology of experimental investigation are attributed, in one way or another, to Democritus: from the experiments of "compulsion" to the "Democritean instances," and from the "subtlety of nature," to the discovery of "transcendentals." Meanwhile, unlike other contemporaries attempts to establish distinguished philosophical genealogies, Bacon's Democritus is not so much a predecessor, as a (fictional) colleague, whose achievements must be continued, and whose errors can help experimenters to detect their own errors and amend their own judgments. In other words, Bacon's Democritus is an interesting case of (fictional) history of philosophy put in service of the scientific method.

Janiak, Andrew

A tale of two forces: gravity and vis viva in Du Châtelet's Institutions.

Abstract: Contemporary scholarship (Bour & Zinsser 2008) typically translates Émilie Du Châtelet's Institutions physiques, first published in Paris in 1740, as Foundations of Physics. A now standard narrative explains that the text specifically provides Leibnizian metaphysical foundations for Newtonian physics. The narrative is tempting, enabling readers to see the early chapters of the Institutions concerning principles like the PSR as grounding later chapters concerning forces like gravity. However, Du Châtelet also expended considerable energy in defending Leibniz's idea of vis viva, especially in the second edition of her text in 1742. Since one can hardly call vis viva an aspect of Newton's physics, this fact complicates the standard narrative. Instead, a more complex narrative is required if we are to understand the complexities of Du Châtelet's magnum opus.In the case of universal gravity, Du Châtelet argued that physics requires a foray into metaphysics. Newton's view that gravity is a universal force suggested to many that it was fundamental to matter. In 1713, Roger Cotes famously claimed that gravity is a primary quality, but Newton explicitly avoided that idea in the very same edition of the Principia. Pace Cotes, Du Châtelet noted that if gravity was found to depend on some medium like the ether, it might not be a property of bodies after all. Pace Newton, she argued that one must clarify what one means by the essence of matter, a task Newton had assiduously avoided, and only then can one clarify whether gravity is essential to matter. So the idea of universal gravity requires a foray into the very metaphysics that Newton eschewed. But crucially, Du Châtelet surveyed the metaphysics of essences, rather than any of Leibniz's ideas, so she was not exploring a Leibnizian foundation in this case.The case of vis viva was fundamentally different. Far from eschewing metaphysical questions as Newton had, Leibniz emphasized metaphysical aspects of forces whenever defending vis viva. Unlike universal gravity, whose metaphysics was not clarified, living force was described as a function of a body's size and its speed2. Since these were perfectly clear concepts, the physics of vis viva did not require any further foray into metaphysics. And crucially, the fact that Newton's physics ignored vis viva was irrelevant to Du Châtelet: it deserved attention just like universal gravity.Du Châtelet's approach to analyzing these two forces helps us to develop a new narrative of her Institutions. She presented the best physics of her day, rather than the elements of Newton's system alone. She also presented the best metaphysics, such as a discussion of essences, rather than merely the original ideas of Leibniz's system. And she clearly rejected the notion that physics per se stands in need of any foundation. Instead, she argues that physics and metaphysics are "interconnected" in complex ways that defy easy categorization, and one must employ the best ideas in physics and in metaphysics regardless of their origin.

Jareonsettasin, Preedanant

How to decompose an essence: Kilwardbian genus-differentia definition in context.

Abstract: Many medieval Aristotelians were engaged in the essentialist project of reducing modal, classificatory, and scientific truths to true claims about the essence of objects. The success of this project requires a semantically precise account of what one is committed to in making these essence-claims. Many also treated the attribution of a genus-differentia definition (GDD) to an object as a paradigmatic type of essence-claim. What is the scope of GDD-attribution? When is a GDD-attribution true? In this paper, I examine Robert Kilwardby's answers to these questions in the context of his metaphysics and semantics. Though Kilwardby defends the traditional position that only substances have a definition, he seemingly inconsistently applies GDD to characterise the essences of many non-substantial objects, including the syllogism. I vindicate Kilwardby's consistent and broad employment of GDD by showing that and how successful GDD-attribution reveals the metaphysical constitution of the object defined. Kilwardby argues that by considering non-substantial objects, e.g. triangle, apart from their modifying a substance, we may consider them to have essences and essential forms. I then show how successful GDD signifies the constitution and ordering of these forms within any object's complete essential form. I illustrate Kilwardby's analysis by showing how he understands the GDD-attribution: 'man is a rational sensible mortal body'. I thus advance our understanding of how a foundational component of the medieval essentialist project works.

Josten, Dries

Plessner’s material a priori categories of life

Abstract: In his Die Stufen des Organischen und der Mensch (1928), Helmuth Plessner proposes to analyze what he calls "vital categories", those laws that serve to individuate an organic whole from its surroundings. In calling these laws "categories", Plessner is clearly drawing on concepts from the idealist tradition, especially from Kant; but in qualifying these categories as "vital", he distances himself from idealism, as is also clear from his characterizing them as a "material a priori". In this paper, I argue that we should understand these vital categories in light of a central problem in the philosophy of science of that time: that of the relationship between scientific disciplines and the relationship between philosophy and science. Where recent scholarship on Plessner has sought to understand him through a parallel with Max Scheler and the influence of Nicolai Hartmann on philosophical anthropology (Fischer 2021; Wunsch 2016), I propose we find the core of his philosophy in his conception of what role remains for philosophy in the 20th century. With the philosophical project of Die Stufen, Plessner clearly situates himself within the tradition of Lebensphilosophie insofar as he rejects reductionism. At the same time, Plessner explicitly elaborates 'philosophical anthropology' as a new approach to philosophy and distinguishes it from Lebensphilosophie. I show that Plessner criticized Lebensphilosophie for engaging with the sciences and what they can teach us in a too limited manner and thereby failing to integrate the results of philosophy and of science with each other. Plessner finds the basis for a more integral picture, I argue, not in thought, but in the "structure of life" (1928, 109). He holds, that categories can be found in the corporeality of the organic, which it shares with the inorganic, but also that the organic's specific relation to its corporeality distinguishes it from the inorganic. This relation Plessner calls positionality, referring to those categories which have to be presupposed in order for life to occur. I argue on the basis of this that Plessner's material a priori laws attest to his commitment both to a mitigated form of naturalism and to antireductionism. I also show that this sheds light on his general conception of philosophy, his stance in the debate on the unity of science, and his place within the traditions of Lebensphilosophie and Philosophical Anthropology.ReferencesFischer, Joachim. "Philosophische Anthropologie und Neue Ontologie: Die ›Kölner Konstellation‹ zwischen Max Scheler, Nicolai Hartmann und Helmuth Plessner als philosophiegeschichtliches Ereignis im 20. Jahrhundert. Philosophische Anthropologie als interdisziplinäre Praxis. Leiden: Brill mentis, 2021. 3-23.Plessner, Helmuth. Gesammelte Schriften/4 Die Stufen des Organischen und der Mensch. Frankfurt am Main: Suhrkamp, 1928/1981.Wunsch, Matthias. "Anthropologie des geistigen Seins und Ontologie des Menschen bei Helmuth Plessner und Nicolai Hartmann." Zwischen den Kulturen. Freiburg/München: Alber. 2016.

Klein, Alexander

Introspection: From Jamesean to Russellian Monism

Abstract: The first known instance of Bertrand Russell positively espousing neutral monism appears in notes he composed in prison in 1918 (CPBR 8, 252; "neutral monism" is Russell's name for a position William James had advocated, according to which reality is composed of one kind of stuff that is fundamentally neutral between the physical and the mental). Despite longstanding criticism of James, one thing is immediately obvious in the notes: Dewey, the American Realists, and especially James himself frame the initial discussion. Another thing stands out: Russell's opening argument for neutral monism rests on an appeal to introspection (CPBR 8, 256 – 257). Russell proposes a version of neutral monism on grounds that it could furnish what behaviourism (he cites Watson) evidently cannot-an adequate account of introspective knowledge.In this paper I ask whether these themes-introspection and James-are related in Russell's early thinking about neutral monism. The question is interesting because James's psychological conception of introspection gave both metaphysical and epistemological shape to his own neutral monism (I will argue). So when he provisionally "adopt[s] William James's view"-neutral monism-does Russell also adopt James's account of introspection? I think the answer is a qualified "yes," but I will show that it is (intriguingly) difficult to pin Russell down on this, and that clarifying the intended model of introspection highlights philosophical ambiguities concerning the precise version of neutral monism Russell actually meant to articulate. Auguste Comte once argued that true introspection is impossible because "the organ observed and the organ observing" would have to be "identical" (Comte 1830, quoted in translation at James 1890, 188). Following Mill, James responded to this problem by portraying introspection as retrospection (James 1890, 189). Some of James's earliest hints of neutral monism exploit two epistemic principles that follow from this model of introspection. James held that A) we are not immediately aware of awareness itself, and B) we are aware of awareness only by using short-term memory (James 1890, 290 – 291). James eventually concluded that awareness-consciousness itself-is constructed in retrospect out a neutral stuff he came to call "pure experience" (James 1904/1912, 13). This is a constructivist version of neutral monism, since neither physicality nor mentality is "realized" until pulses of pure experience are "grouped" (1904/1912, 12) with other associates in appropriate ways. The evidence is ambivalent concerning whether Russell understood introspection to involve a single mental state observing itself, or a present mental state observing a past state (the latter view is suggested by 1921, 248; the former view is suggested by some archival materials I will present). The preponderance of evidence suggests that Russell did adopt a James-style, retrospective account of introspection. But this raises a deeper question: did Russell accept the constructivist version of neutral monism that James took to flow from the retrospective account of introspection? It seems Russell wanted to avoid this constructivism, but it is not clear whether he had a model of introspection that could support such a view.

Koenig, Daniel

Number as Scheme of Order. Ernst Cassirer's Philosophy of Mathematics in the context of his Philosophy of Culture

Abstract: In recent years, both Ernst Cassirer's (1874-1945) philosophy of culture and his philosophy of mathematics were rediscovered, but until now there is little interaction between the two research strands. In my talk I want to show that it is necessary to consider both to fully appreciate Cassirer's view on mathematics and his broad epistemology; there is a correlative relation between his philosophy of culture and his philosophy of mathematics. Therefore, my talk underlies a twofold thesis: On one side, Cassirer's philosophy of mathematics, which he develops in initial stages in his early writings, gains increasingly in contour in the context of his later Philosophy of Symbolic Forms, which implies an extension of his epistemology to a philosophy of culture. At the same time, Cassirer's preoccupation with mathematics has a paradigmatic significance for the symbol-theoretical approach of his philosophy of culture, which can be considered as his epistemology in the broadest sense. To illustrate this interdependence, I want to show that number has a double function in Cassirer's philosophy of culture. On the one hand, number – understood as the scheme of order par excellence – is of determining meaning for mathematics as a general or pure theory of relations, as a universal symbolic language. On the other hand, in the philosophy of symbolic forms, number is presented, along with space and time, as a form of intuition that takes different forms in language, myth, and science. If the goal of science is the construction of a strictly ordered reality, number – understood as the scheme of order par excellence – is the means for the formation of this reality. In this respect, number is also the determining principle not only for mathematics but also of the symbolic form of science in general. According to Cassirer, science accepts the principle of numerical determinism as a regulative idea that is responsible for its logical coherence and its systematic unity. Accordingly, by considering the different specializations of number, a process of differentiation of symbolization from the symbolic form of the language, over that of the myth up to the science can be clarified, whereby in science number no longer appears as a form of intuition, but as the determining principle. If mathematics has to do with all kinds of strict relations and their symbolization, the relations found here can serve not only for the construction of a scientific reality. Rather, Cassirer also uses them, as I would like to indicate in the form of a short outlook, to express general relations within his comprehensive philosophy of symbolic forms itself. In this sense, Cassirer's characterization of mathematics as a universal symbolic language can be taken seriously: For Cassirer, mathematics is not only a form of theoretical cognition, but also to be understood as a form of metasymbolic cognition.

Körber, Silke

L. Susan Stebbing: Humanism in Practice

Abstract: In the first half of the twentieth century new approaches emerged to the transfer of knowledge in the sciences and to popular education that were intended to promote the formation of modern democracies and an egalitarian world society. The British philosopher Susan Stebbing, the first female president of the Union of Ethical Societies (1941) and first women to hold a philosophy chair in the UK (1933), incorporated such developments in her theoretical work as well as in her public activities, particularly in conjunction with the Austrian émigré and member of the Vienna Circle Otto Neurath. Stebbings works and collaborations have received only limited attention so far and will be evaluated in this talk. The collaboration with Otto Neurath took place against the backdrop of a convergence of the Vienna Circle in the 1930s and in exile, as well as the emerging analytic tradition in England (later known as the Cambridge School), a development that was significantly fostered by Stebbing. Moreover, with her critique of the Vienna Circle's concept of analysis, the British philosopher helped sharpen and clarify the disputes between the Cambridge School and logical positivism. Neurath and Stebbing collaborated primarily with the aim of realising and disseminating a "scientific attitude" in conjunction with a principled humanistic understanding of science and philosophy as "scientific humanism" – as well as reflecting on the role of science and the scientific expert in times of crisis.For all their differences in interpreting philosophical principles, they were united by fundamental approaches and principles: their common denominator was, and increasingly became, the consistent linking of socio-political engagement in the public sphere with a more holistic and contextual concept of analytical thought and action, mediated by criticism of the common language and a more pluralistic methodology. They wanted to improve the lives of as many people as possible by promoting their ability to observe and judge the world around them. This was seen as part of the education and upbringing in democratic societies and as a prerequisite for a fulfilled and happy life in a peaceful global community.

Koterski, Artur

The Zeitgeist as a Paradigm: Sociology of Scientific Knowledge in Tadeusz Bilikiewicz's "Die Embryologie"

Abstract: The (re)discovery of Ludwik Fleck's book in 1979 made it sufficiently clear that some ideas and insights commonly linked with T. S. Kuhn's Structure of Scientific Revolutions (1962) had been formulated as early as the 1930s. It appears that there is more yet to uncover: In the autumn of 1930, Tadeusz Bilikiewicz (1901-1980), a junior assistant in the Department of History and Philosophy of Medicine at the Jagiellonian University in Cracow (Poland), completed a volume on a rather underdeveloped part of the history of medicine, namely the beginnings of modern embryology. While his Die Embryologie im Zeitalter des Barock und des Rokoko (1932) received over two dozen positive reviews in multiple languages across Europe and the USA–with prominent German historians of medicine such as Paul Diepgen, Edith Heischkel, or Walter Pagel among the reviewers–today it remains virtually unknown. Despite this acclaim, Bilikiewicz is mostly remembered for his post-war contribution to psychiatry and, occasionally, for his polemical exchange with Fleck initiated by the latter regarding Bilikiewicz's sociology of science (1939).In his book, Bilikiewicz presented and discussed the history of embryology from William Harvey to Caspar Wolff, explaining its meandrous development by environmental conditioning. Drawing from the historiographies of Heinrich Wöfflin, Karl Joël, and particularly Henry Sigerist, Bilikiewicz aimed to provide a reconstruction of the intellectual ideals, or 'the spirit of the time (Zeitgeist),' during the Baroque and Rococo periods. The Zeitgeist influences all domains of culture in a given era, including science, determining scientific objectives, problem scopes, research methods, empirical data choices, and their interpretations. With the concept of the spirit of the time, Bilikiewicz sought to rationalize some perplexing decisions made in the République des Lettres, primarily the rejection of the theory of epigenesis in favor of variants of preformationism and the concept of preexistence, as well as the emergence of vitalism from mechanicism.According to Bilikiewicz, the Zeitgeist is a set of patterns, and in this sense, it functions as a paradigm–quite unexpectedly resembling the Kuhnian paradigm in many ways. Thus, I will attempt to present Bilikiewicz's idea of the spirit of the time and his scientific constructivism in comparison with the key concept of Kuhn's Structure.

Kürbis, Nils

The Ancient Problem of False Judgement, its Appearance in Early Intuitionist Philosophy of Mathematics, and its Solution in the Brouwer-Heyting-Kolmogorov Interpretation of the Logical Constants

Abstract: The ancient problem of false judgement is the following. To judge truly is to judge what is, to judge falsely is to judge what is not. But to judge what is not is to judge nothing, and to judge nothing is not to judge at all. So how is false judgement possible? The problem is rooted in the poem of Parmenides. It plays a prominent role in Plato's Theaetetus. A solution is presented in the Sophist. Sentences differ from names. They are composed, in the simplest cases, from names and verbs that are 'woven together'. An act of naming succeeds or it doesn't. Sentences are true or false. Early followers of Brouwer faced a similar problem with his account of mathematics. Mathematical activity consists in the mind effecting certain constructions. But if a mathematical sentence is false, then there cannot be any mathematical construction to be effected, so there is nothing for the mind to do, and hence no mathematical activity. So how can false mathematical propositions be meaningful? Brouwer claimed that mathematics does not appeal to proof by contradiction: `At the point where you enounce the contradiction, I simply perceive that the construction no longer \emph{goes}' (Brouwer (1975): 72f). Brouwer's assistant Freudenthal writes: `jeder Satz, wenn man ihn erst einmal intuitionistisch einwandfrei formuliert, [enthält] automatisch seinen ganzen Beweis.' (Freudenthal (1937): 112f) False mathematical sentences have no proof, and hence they cannot be `formulated unobjectionably', so they are defective, and that is not far from saying that they are meaningless. It was this difficulty that lead Griss to develop his negationless mathematics. Griss comments on Brouwer's account of mathematical activity: `Ici la négation n'entre pas. [...] Faire la supposition qu'une preuve soit donnée, tandis que cette preuve paraît être impossible, est incompatible avec le point de départ constructif et évident, car l'existence d'une preuve est identique au fait qu'elle a été donnée.' (Griss (1948: 71) Heyting reports that for Griss there are no false mathematical propositions, as false mathematical propositions are meaningless. I shall argue that the Brouwer-Heyting-Kolmogorov interpretation of the meanings of the logical constants provides a contribution to a solution that is along the ancient lines. Heyting explains the meanings of of logical constants in terms of how complex mathematical propositions are proved. Thus there is a distinction between two essentially different kinds of constructions, those of the mathematical objects that mathematical propositions are about, e.g. the numbers, and those that are mathematical proofs. The former constructions are the objects named by mathematical expressions. The latter are what mathematical truth and falsehood consist in: if a proof exists, the proposition is true, if it is shown that it cannot exists, it is false. Brouwer, L. E. J. (1975). On the foundations of mathematics. In A. Heying (Ed.), Collected works, vol. 1: Philosophy and foundations of mathematics, pp.478–479.

Amsterdam: North Holland.Freudenthal, H. (1937). Zur intuitionistischen Deutung logischer Formeln. Compositio Mathematica 4, 112–116.Griss, G. (1948). Sur la négation (dans les mathématiques et la logique). Synthèse 7(1/2), 71–74.

Kvasz, Ladislav

Koyré's thesis of mathematization of nature

Abstract: Alexandre Koyré formulated the thesis, that the central event of the Scientific revolution was the mathematization of nature. According to Koyré, this mathematization consisted of three points: 1. geometrisation of space, 2. the replacement of hte hierarchical cosmos by the infinite universe of modern science, and 3. the replacement of a world of more or less by a universe of precision. This account attracts even today considerable attention (see Hypotheses and Perspectives in the History and Philosophy of Science: Homage to Alexandre Koyré 1892-1964, Raffaele Pisano, Joseph Agassi and Daria Drozdova eds., Springer 2017). Nevertheless, there are also more critical accounts of Koyré's thesis (see The Language of Nature: Reassessing the Mathematization of Natural Philosophy in the Seventeenth Century, Geoffrey Gorham, Benjamin Hill, Edward Slowik and Kenneth Waters eds., University of Minnesota Press 2016) and Sophie Roux has called the mathematization thesis a grand narrative (see Roux, Forms of mathematization, Early Science and Medicine 15 (2010), pp. 319-337). In the paper I will try to offer an exposition of the historical development of Koyré's views, give an overview of the different points of its criticism and then attempt to argue for a reformulation of the thesis that could withstand the criticism.In a nutshell, the main point is that in the 17th century mathematization did occur, but it was not a mathematization of nature but rather of motion. Therefore all critics, who argue against Koyré, that there was no mathematization in natural history, biology, or medicine, are correct. The thesis should be scaled down. The second point is, that the driving force behind this mathematization was not geometry but rather differential and integral calculus. Thirdly the transition from the cosmos to an infinite universe did not affect all sciences, as several critics have pointed out, thu rather than the general cosmological backgroud, what was for the Scientific revolution of importance, was the understanding of matter. And finally, also the introduction of precision was not the only methodological change, and it seems that the introduction of the experimental method (which Koyré attempted to downplay) was more important. It is possible to apply the experimental attitude also in biology or medicine, even if the phenomena are far from precise.I hope that after such scaling down and shifting the focusses, the mathematization thesis can be defended against the criticism, that it is only a grand narrative. It is a philosophical account of the birth of modern science, having its roots in the philosophy of Ernest Cassirer, and it offers an alternative to the analysis of the same historical event by Edmund Husserl in his Die Krisis der europäischen Wissenschaften und die transzendentale Phänomenologie, that appeared in 1936, the same time when Koyré worked on gis Etudes Galileenes which appeared in 1939.

L. Condé, Mauro

The reception of the Zilsel thesis in the context of the internalism versus externalism debate

Abstract: This contribution addresses the critical reception of Edgar Zilsel's ideas in the context of the internalism versus externalism debate, a discourse in which the Austrian thinker was one of the poles of the dispute. It maps the main approaches and comments on Zilsel's work by influential scholars (e.g. Durand 1943; Crombie 1953; Koyré 1965, 1966/1973; Kuhn, 1977/1961, 1977/1968; Shapin 1992, 1996; Shapin & Schaffer 1985) concerning this controversy. This approach provides a good idea of the reception and the influence of Zilsel's work on the Anglophone debate on the origins of the scientific revolution. Furthermore, this mapping aims to show that the main criticisms leveled against the so-called Zilsel thesis were not tenable, even though they have not been directly refuted by the Austrian philosopher and historian of science, due to his untimely death. Despite the criticism, Zilsel's ideas linger on. His interpretation of the rise of modern science, continually present in the historiography of science for several decades after his death, demonstrates the strength and robustness of his ideas for this debate around the scientific revolution.ReferencesCrombie, Alistair C. 1953. Augustine to Galileo. The History of Science – 400-1650. Harvard University Press. Durand, Dana B. 1943. "Tradition and Innovation in Fifteenth Century Italy: 'Il Primato Dell' Italia' in the Field of Science". Journal of the History of Ideas 4 (1): 1-20.Koyré, Alexandre. 1965. Newtonian Studies. Cambridge: Harvard University Press.Koyré, Alexandre. 1966/1973. Étude d'Histoire de la Pensée Scientifique. Paris: Gallimard.Kuhn, Thomas. 1977/1961. "The Function of Measurement in Modern Physical Science". In: Kuhn, T., The Essential Tension. University of Chicago Press. Kuhn, Thomas. 1977/1968. "The History of Science". In: Kuhn, T., The Essential Tension. University of Chicago Press.Shapin, Steven. 1992. "Discipline and Bounding: The History and Sociology of Science as seen through the Externalism-Internalism Debate". History of Science (30): 333-369.Shapin, Steven. 1996. The Scientific Revolution. University of Chicago Press.Shapin, Steven. Schaffer, Simon. 1985. Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life. Princeton: Princeton University Press.

Laino, Luigi

Natorp on Minkowski Spacetime. How an Incorrect Interpretation May Work Well

Abstract: Hentschel 1990 distinguished revisionists from immunizers within the history of neo-Kantianism by noticing that "immunizers" such as Natorp were recalcitrant to accept relativity and its philosophical consequences. Although Hentschel's book is still seminal, I aim to show that incorrect interpretations may work relatively well if one holds to historical criteria that put things in perspective. While taking an "immunizing" stand against the empiristic and holistic trends in the epistemology of relativity, Natorp indeed defended a Kantian view on Minkowski spacetime based on a reinterpretation of Kant's concept of construction. In particular, he argued that Euclidean geometry is a priori also in this case, given that Minkowski structures can be developed from a 3D Euclidean manifold. Therefore, I will show that aprioricity is limited to mathematical diagrams and does neither concern the categories of the understandig nor the nature of geometrical axioms.Nevertheless, I do not give up the attempt to focus on the flaws of Natorp's interpretation. First, I will show that by altering the relationship between the group transformations Gc and G∞ of Minkowski (1909), Natorp misconceived the non-classical character of Minkowski transformations (Hentschel 1990; Torretti 1996). Second, I will show that although Natorp's reading is consistent with the state of physical research, to the point that his ideas square with Einstein's explanation that Minkowski spacetime is a 3D continuum plus the rotation that obtains through i (the imaginary unit), it is nonetheless weak. An epistemology that produces statements not passing the test of scientific evolution may be conceived of as dogmatically in contrast with the idea that reason is in fieri.As to the structure of the talk, I will present Hentschel's frame for categorizing the interpretations of relativity. In the second section, I will address Minkowski's paper and dwell upon how subtly Minkowski forges his "Weltpostulat" in contraposition to Kant and how it is difficult to couple it with neo-Kantian trends. Finally, I will elaborate on Natorp's interpretation and specifically deal with the last two sections of Die logischen Grundlagen der exakten Wissenschaften, devoted to relativity theory.Reference list (limited to the abstract)Hentschel, K. (1990). Interpretationen und Fehlinterpretationen der speziellen und der allgemeinen Relativitätstheorie durch Zeitgenossen Albert Einstein. Basel et al.: Birkhäuser.Minkowski, H. (1909). Raum und Zeit. Jahresbericht der Deutschen Mathematiker-Vereinigung, 18, 75-88.Natorp, P. (1910). Die logischen Grundlagen der exakten Wissenschaften. Leipzig und Berlin: Teubner.Torretti, R. (1996). Relativity and Geometry. Oxford et al.: Pergamon.

Landry, Elaine

Breaking Benacerraf

Abstract: In this paper I will use my as-ifist account of Plato to break Benacerraf's [1965] dilemma. Recently, I argued that, against typical realist interpretations, Plato's view of mathematics is best understood as offering an as-ifist account of mathematics. An as-ifist takes mathematical hypotheses as if they were true for the purpose of solving a mathematical problem. Once the problem is so solved, we are then committed to the existence of those objects that are needed for the solution. For example, as outlined in the Meno, if we begin with the problem of finding the length of the side that will double the area of a square having sides of length two and we set-up the hypothesis that the length of the side is the length of the diagonal of the square of length two, then we can solve the problem. In so doing, however, we are committed to the existence of irrational numbers, that is, to the existence of 22. So, for Plato, and for any as-ifist, existence is a consequence of truth. Let's now recall Benacerraf's dilemma: we must either choose a shared semantics with mathematics and ordinary discourse (and, in so doing, adopt a realist view of mathematical objects and thereby forego a reasonable epistemology) or we must choose a reasonable epistemology (and, so doing, give up mathematical realism and thereby forego a shared semantics, because we don't have reference to mathematical objects). I will argue that we do have reference to mathematical objects, but that this reference is a consequence of our taking mathematical axioms as-if they were true. So, we do have a shared semantics, but it is one where the order of existence and truth is reversed. That is, for ordinary discourse, truth is taken as a consequence of existence, whereas for mathematics existence is taken as a consequence of truth. By way of arguing for this claim, I refer to the work of Hilbert and his famous debate with Frege. That is, I will show that as we shift to modern axiomatic mathematics, where we replace talk of as-if true hypotheses with as-if true axioms, we arrive at Hilbert's view that consistency implies existence. I turn next to consider Tarski's [1944] formal definition of truth and argue that truth in both mathematical discourse and ordinary discourse can analyzed using the same Tarskian schema. Thus, I will have shown that, as a Plato inspired as-ifist, one can have a shared semantics with mathematics and ordinary language without adopting mathematical realism, and thereby I will have broken Benacerraf's dilemma.

Lawrence, Richard

Content, applications, and arithmetic as science: Frege vs. the formalists

Abstract: As formalism rose to prominence in German mathematics, it acquired one of its harshest critics: Gottlob Frege. Frege argued against "formal arithmetic" throughout his career One of the reasons these arguments are interesting is that they reveal a connection between Frege's views about content and his understanding of what makes arithmetic a science. Frege often connects his general views about content with his understanding of science, particularly after distinguishing Sinn and Bedeutung as aspects of judgeable content. In science, "it is the striving for truth that drives us to advance from the Sinn to the Bedeutung" ("On Sinn and Bedeutung" 33); and "the Bedeutung is thus shown at every point to be the essential thing for science" ("Function and Concept", 134).One of the enduring themes in Frege's criticism of formalism is that it does not respect this relationship between content and science. In formal arithmetic, signs are conceived as not having any content, and this robs arithmetic of its status as a science: since content is the essential thing for science, if arithmetical signs have no content, "we should have neither truths, nor a science, of arithmetic" (Frege 1885, 114). As Frege sees it, formalism shifts focus away from contents to the signs themselves, and thereby deprives arithmetic of its proper subject matter. Signs themselves are "merely ink or print on paper", which have physical and chemical properties, but not arithmetical ones (Grundlagen §95). A whole battery of other objections flow from this: that formalism threatens to make arithmetic empirical (Grundlagen §109), that it forces us to distinguish e.g. (1+1) and 2 as different numbers ("Function and Concept", 4), that it cannot account for the infinitude of numbers (Grundgesetze Vol II §123ff.), and so on. But recent work has revealed Frege's central charge to be inaccurate. Frege's formalist interlocutors do claim that signs in formal arithmetic have content, both expressly and by implication. A careful examination of their views shows that Frege's disagreement with them is not about whether signs have content, but about how they conceive of this content, and thus, what it means for arithmetic to be a science. How then does Frege's conception of content differ from theirs? The answer I would like to argue for here is that Frege and the formalists differ on the issue of how the content of signs are related to applications of arithmetic. The formalists think it is important to isolate arithmetic from its applications; for them, this is part of what makes arithmetic a rigorous and a priori discipline. Frege on the other hand insists that applications are what make arithmetic a science rather than a mere game, and that the possibility of such applications must be built into arithmetical concepts. This fleshes out both our understanding of Frege's theory of content, and our understanding of what science is from a formalist point of view.

Leon Bauer, Philipp

Ernst Mach's Principle of Economy

Abstract: Ernst Mach (1838-1916) was one of the most influential natural scientists and philosophers of the late 19th century. He was as well a pioneer for meta-theoretical investigation in the history and philosophy of science. Mach's work has had a significant impact in the methodological development of various sciences. His principle of economy (economy of thought, thought economy) played an extremely important role in this regard. Present throughout his work, from his early writings (e.g., Die Geschichte und die Wurzel des Satzes der Erhaltung der Arbeit (1872)) to his later contributions (e.g., Erkenntnis und Irrtum (1905)), the principle of economy was central for his anti-metaphysical stance, vindicating the elimination of the concept of a substance and of existential claims.Coupled with his Pragmatism and Historicism, Mach made use of the economy principle to advance a view of scientific research as a process, in which theoretical terms and scientific theories are seen as auxiliary tools for acquiring relevant insights and predictive power, but not as expressing any kind of fundamental ontological unities of reality. Therefore, the principle should be taken as an instrument for assessing the practical value of concepts and theories for orienting our expectations in the environment. The general scientific and anti-metaphysical nature of his meta-theoretical reflexions and the instrumentalist view of concepts and theories entailed by the principle of economy, had a big impact in the development of the early 20th century theory of science. Mach should be seen as an important predecessor and pathfinder for Logical Empiricism and as a major influence source on the Vienna Circle. In my talk, I want to present Mach's principle of economy and how it was taken up by the Vienna Circle.

Liciu, Alexandru

Thinking from analogy and the genealogy of optics in the early Royal Society. Robert Hooke and Henry Power

Abstract: In this paper, I will explore the hypothesis that, at the early Royal Society, theorization of analogical thinking went hand in hand with a discourse on the development of optics. This could be seen, for instance, in Henry Power, who claimed that Adam's knowledge of nature after the Fall was entirely open to the dangers of analogical thinking precisely because he lacked the science of dioptrics (Power 1664, Sig. a7r). This error, in Power's view, was to permeate the whole natural philosophical thinking up to his times. Indeed, had Aristotle been alive today, he would have re-written his natural history of animals to start from microscopic creatures. (Ibid., Sig b1v) It was because of the lack of progress in optics that, Power thought, much of the true natural philosophy could not have been articulated up to him. This is at least partly because of the notion of observation that optics entailed, which was probably an elaboration on Francis Bacon's notion that microscopy could reveal the 'schematisms' (inner structures) of bodies (and that it is a pity Democritus himself did not have a microscope, which would have transformed his speculative notion of atom into one confirmable by observation).A somewhat more refined view on the relationship between optics and analogy was developed by Robert Hooke. He agreed with Power (whose book he certainly read as a preparative to his own Micrographia) (Hooke 1665, Preface [28]) that the natural powers of the mind needed to be held in line by optics, but he was more skeptical as to what one might observe through optical instruments to begin with. Hooke did not believe that microscopic observation could at this time be used to carve nature at its joints, but he did hope that one day that would be possible. (Ibid., Preface [9], 114) He thus, from early on, conceived optics as part of a corrective epistemology: the purpose of optics was to restrict the mind's propensity towards using illegitimate analogies, although in the end some degree of thinking from 'similitudes and comparisons' was still present. But to complicate things further, in his later years Hooke started to consider that by using telescopes one could observe 'non-Pareils of nature', i.e. things so different from our usual expectations that they could not be conceptualized by mere analogical thinking (an example are the rings of Saturn). (Derham 2005 [1726], 257-273) In other words, towards the end of his career, Hooke gained more trust in the capacity of optical devices to reveal natural truth, while the role of analogy was to be diminished. However, in Hooke's fine-grained genealogy of microscopy (comprising Roger Bacon, Giambattista Della Porta, and the Accademia dei Lincei more broadly), the discipline of optics was declining, to the point that it presently became mere 'diversion and pastime'. Reasons for such conceptual shifts will be discussed in this presentation, in the broader context of how the relation between thinking from analogy and the development of optics was conceptualized at the Royal Society.

Limbeck-Lilienau, Christoph

Success Semantics in the 1920s

Abstract: Since the 1990s, success semantics, originally formulated by Ramsey (1927), has had an important impact on naturalistic theories of mental content and on teleosemantics (Whyte 1990; Papineau 1993). I will reconstruct here how Ramsey's original formulation of success semantics emerged in the context of a debate between Bertrand Russell and C. K. Ogden. In their widely read book The Meaning of Meaning, C. K. Ogden and I. A. Richards spoke about the emergence of a new kind of naturalistic theories of meaning, theories they called "causal theories of reference" (1923/1949: 54), among which they included Russell (The Analysis of Mind), the American neo-realist E. B. Holt and their own position. Ramsey as well as Russell had reviewed Ogden and Richards' book. In his review, Russell insisted on the importance of a causal account of meaning, discussed different versions of such a theory and sketched the one he thought the most promising. While Ogden and Richards offered only a theory looking at the cause of expressions (a "causal theory"), so Russell, he claimed that he himself offered a theory explaining meaning by taking into account both the causes and the effects of expressions (combining therefore, as Russell says, a "causal theory" with an "effect theory"). Frank Ramsey, at times a close collaborator of Ogden, also followed Russell and advocated the position that "the meaning of a sentence is to be defined (...) by its possible causes and effects" (Ramsey 1927: 170).Based on these developments, Russell and Ramsey almost simultaneously proposed a theory which explained the meaning of a belief by the success conditions of the actions triggered by that belief. This theory, which was later called "success semantics" (Whyte 1990), was formulated by Ramsey in his paper "Facts and Propositions". In this paper, Ramsey credited also the "pragmatism" "derived from Russell" for his view "that the meaning of a sentence is to be defined by reference to the action to which asserting it would lead" (Ramsey 1927: 170). We will show how Ramsey's success semantics is based on the causal theory and effect theory developed by Russell, but we will also show that almost at the same time Russell developed a success semantics very similar to Ramsey's. This finds expression in Russell's book An Outline of Philosophy (1927) in which he had improved his account of meaning based on a causal theory and an effect theory. In the chapter on "Truth and Falsehood" from that book, Russell states: "A man 'believes' a certain proposition p if, whenever he is aiming at any result to which p is relevant, he acts in a manner calculated to achieve the result if p is true, but not otherwise." (Russell 1927: 272). We will compare the two versions of success semantics in Ramsey and Russell and show how they emerged from a common source in the debate about the "causal theories of reference" in the 1920s.

Lin, Qiu

The Prehistory of Du Châtelet's Chapter on Space

Abstract: Ten years ago, Andrew Janiak (Duke) and Karen Detlefsen (Penn) visited the Bibliothèque Nationale de France (BnF) to study the 1738 manuscript of Du Châtelet's Foundations of Physics. Also known as 'the Paris manuscript', this document is key to understanding the evolution of Du Châtelet's natural philosophy for the following reasons: (1) it was withdrawn from publication by the author after receiving an approbation in 1738, (2) it was finished before Samuel König started to tutor Du Châtelet, so it could tell us how much she knew about Leibniz and Newton independently of König, who repeatedly accused her of plagiarism, and most importantly (3) there appears to be a remarkably major shift of thinking from the Paris manuscript to the first edition of the Foundations, which was published two years later in 1740. As Janiak and Detlefsen noted, Du Châtelet had significantly re-worked her chapters on space, time, matter, and body, among others. Following their footsteps, the present paper results from a comprehensive study of Du Châtelet's space chapter in the Paris manuscript. The aim is to unveil the subtlety of her thinking about space by way of discussing various deletions, revisions and margin notes found in the original document. In addition to showing the philosopher's intellectual development in the late 1730s, this paper argues that contrary to the view expressed in Janik (1982), according to which König's plagiarism charge was well and justified, we now have evidence to dismiss it conclusively.

Lindholm, Juho

A Comparison of John Dewey and Joseph Rouse

Abstract: @page { size: 8.5in 11in; margin: 0.79in } p { color: #000000; line-height: 115%; text-align: justify; orphans: 2; widows: 2; margin-bottom: 0.1in; direction: ltr; background: transparent } p.western { font-family: "Liberation Serif", serif; font-size: 12pt; so-language: en-US } p.cjk { font-family: "NSimSun"; font-size: 12pt; so-language: zh-CN } p.ctl { font-family: "Arial"; font-size: 12pt; so-language: hi-IN } a:visited { color: #800000; so-language: zxx; text-decoration: underline } a:link { color: #000080; so-language: zxx; text-decoration: underline } Classical pragmatists anticipated much of the practical turn that took place in the philosophy of science in the 1970s and 80s. In this presentation, I will argue that there are striking similarities between John Dewey's and Joseph Rouse's philosophies of science.The most obvious similarity obtains between Dewey's concept of habit and Rouse's concept of practice. Another significant similarity obtains between Dewey's concept of situation and Rouse's concept of phenomenon. Both pairs of concepts seem to cut across the celebrated subject-object dichotomy that early modern philosophers established and bequeathed to us and that nowadays goes largely unchallenged. In fact, on the one hand, habit and practice, on the other, situation and phenomenon seem to be both "subjective" and "objective" at once.Both Dewey and Rouse understand the concept of knowledge non-representationally. In effect, they suspend judgment about the traditional definition of knowledge: justified true (un-Gettiered) belief (see Plato 1952: 97d–98a; 1977: 201c–d; Gettier 1963). For Dewey (1929b), knowledge is a "kind of action" – a term which I have explained as habit or practice (undisclosed reference 1). For Rouse (1996), knowledge consists in "epistemic alignments." Dewey and Rouse do not seem to reject representation altogether; rather, they seem to allow knowledge to involve the use of representations; but they deny that knowledge could be defined as representation.Both Dewey ([1925] 1929a: ch. V; 1938: ch. III) and Rouse (1996; 2002) understand the concept of language in a very broad sense that includes everything that has meaning – even non-conventional things. This notion is somewhat surprising, given Dewey's account on the emergence of meaning from the discovery of cause-effect relations (1929b: 81–84) and Rouse's preference of practical hermeneutics over theoretical hermeneutics (1987: ch. 3). Again, both deny that language function representationally but seem to grant that it may involve the use of representations. Especially for Dewey, language is communicative practice, the purpose of which is agreement in action.To my knowledge, Rouse never cites Dewey. Thus it is unclear, whether the latter has directly influenced the former. One possible explanation is that Rouse has adopted some positions from Richard Rorty, who has adopted them from Dewey.

Lower, Kevin

Margaret Cavendish's Materialist Conception of Sympathy

Abstract: Natural sympathy was a widely-contested concept in the early modern period. Renaissance Platonists, such as Marsilio Ficino (1433-1499), understood sympathy as an immaterial force of attraction that produces life, order, and unity in a world of brute matter. Suspicion toward this concept grew alongside the rise of mechanical philosophy, proponents of which held that all natural phenomena are explicable in terms of matter in motion. The content of mechanical philosophy is highly variable since there are several incompatible expressions of its grounding principles. For the purposes of this investigation, we will center some of the principles held by Rene Descartes (1596-1650), Thomas Hobbes (1588-1679), and Robert Boyle (1627-1691). These include: (1) that properties of bodies are intelligible only insofar as they are reducible to geometrical properties, (2) that all forms of natural chance can be explained by the transfer of motions between bodies through physical contact, and (3) that all motion is fundamentally local motion (that is, change of place). From the vantage point of these principles, sympathy could only appear as an occult force that should play no role in natural philosophical explanations.Mechanical philosophy was not without its critics. For instance, Henry More (1614-1687) argues that it cannot explain phenomena such as the resonance of strings, the elasticity of the air, and the unity of the mind. In his view, these deficits showcase the limits of materialism, which led him to draw upon so-called occult concepts such as immaterial spirit and sympathy. Kenelm Digby (1603-1665) agrees with More about the limits of materialism but remains impressed by the explanatory power of mechanical philosophy. He develops a mechanical conception of sympathy grounded in the resemblance of motions exhibited by bodies of similar weights, quantities, and figures. This context provides essential background for understanding how Margaret Cavendish (1623-1673) conceptualizes sympathy. Cavendish is perhaps best known for developing a unique framework of materialism. She holds that the natural world is intelligible in terms of matter in motion, though she disagrees with the principles of mechanical philosophy outlined above. Her conception of sympathy is distinct from contemporary accounts since it is simultaneously materialist and non-mechanical. This paper investigates the main features of Cavendish's materialist conception of sympathy to motivate its relevance to historical investigations of natural sympathy.I argue that Cavendish materializes the concept of sympathy by connecting it with her theory of corporeal motion. Rather than describing sympathy as an abstract force, she conceives it as particular sympathetic and antipathetical motions. These motions manifest among the constitutive and effective parts that compose individual bodies, as well as between several bodies considered as wholes. Her conception of sympathy thus explains the persistence and disruption of material unities and the broader presence of order and regularity within material nature. Cavendish develops a dynamic conception of natural sympathy rooted in the corporeality of motion. Her account demonstrates the multifaceted nature of early

modern conceptions of natural sympathy and demonstrates the value of ameliorative historical research.

Lu, Yan

Mathematical Incommensurability and Power in Plato

Abstract: In exploring the question of "what is knowledge" in the Theaetetus, Socrates clarifies that, in response to Theaetetus' answer with a list of various sciences and expertise, the question is not what one may have knowledge of, or how many branches of knowledge there are, but how to know and define what knowledge itself is. This reminds Theaetetus of his discussion about the definition of mathematical power (dynamis) with the younger Socrates' namesake a little while ago. Theaetetus then describes the development of the definition of mathematical power as an example of knowledge.The puzzle is whether Theaetetus and Theodorus are successful in defining dynamis, and how this definition process relates to the definition of knowledge discussed in the dialogue. There are positive and negative replies to this puzzle. As far as positive replies are concerned, historians of mathematics rarely doubt that Theodorus and Theaetetus finally succeeded in achieving definition to some extent. They understand the definition of power here as the process of discovering or constructing irrationals and then proving they are irrational through geometric methods. More recently, however, Brisson and Ofman have provided negative replies to this puzzle in three consecutive papers (2020a, 2020b, 2018), arguing for the view that both Theodorus and Theaetetus fail to define dynamis, and their failures correspond to the subsequent three attempts by Theaetetus to define knowledge.This paper aims to attempt to provide correlated replies to the puzzle. On the one hand, by carefully re-examining the four occurrences of dynamis in this passage and the three occurrences of its associated adjective and verb, I defend a more traditional interpretation of dynamis, arguing that its fundamental meaning is square and that its use as a side of a square in the definition is derived from its fundamental meaning as a square by the process of construction and proof. On the other hand, I will argue that Theodorus and Theaetetus successfully achieve the definition of power through a process of construction and proof, and this definition process is positively applied in the subsequent discussion of the definition of knowledge. This application is both methodological and substantive. Methodologically, the methods of construction, demonstration, and classification used by Theodorus and Theaetetus are also used in the subsequent discussion of the definition of knowledge. Substantively, a side as irrational needs to be constructed from a square of which it is a side and can only be understood with the help of this square.One result of my interpretation is to argue that Plato goes beyond mathematical Platonism, i.e., mathematical objects such as irrational numbers, and objects of knowledge in general, although existing both outside the physical world and independently of the mind, their manifestation is the result of the active construction of the mind. We cannot grasp mathematical knowledge by simply abstracting the physical world or accepting mathematical propositions, and each grasp of mathematical knowledge requires the construction of definitions and proofs by the individual mind.

Lucken, Collin

On Carnap's Language Engineering

Abstract: The growing prominence of conceptual engineering within contemporary philosophy raises intriguing questions about its scope and significance. Conceptual engineering, as elucidated in Cappelen's seminal work (2018), revolves around the process of evaluating and enhancing our representational devices (3). While the landscape of conceptual engineering encompasses diverse approaches, two figures loom large: Sally Haslanger's ameliorative analysis and Rudolf Carnap's method of explication (Dutilh Novaes 2020). This essay focuses on Carnap's approach, aiming to clarify the breadth of his conceptual engineering. The central argument contends that Carnap's notion of conceptual engineering includes more than mere explication, and this expansion is a crucial facet of his philosophy. Carnap, in fact, had a more comprehensive vision of conceptual engineering, and this broader perspective is a crucial feature of his philosophical project. Furthermore, it must be so in order to defend itself against Quienean criticisms of Carnap's conventionalism. Conventionalism, as I will use the term, is the view that there are two processes by which theories change: by changing the rules of language, and by making corrections in the field of factive knowledge. Quine, however, famously claimed that changes in the rules of our language are not in principle different from "corrections to the field of factive knowledge" (Carnap 1950, 3). In order to meet this criticism, Carnap developed a position I will call framework conventionalism that is broader and more systematic than merely the process of explication. To convey this argument, the essay unfolds in three sections. In the first section, I scrutinize the contemporary landscape of conceptual engineering, highlighting its reliance on Carnap's method of explication. I explore the motivations behind this reliance and consider why explication is regarded as a central process for revising concepts in both philosophy and science. In the second section, I delve into Carnap's intellectual development. By understanding the problems Carnap was attempting to solve, it becomes clearer how emphasizing his neo-Kantian roots and his assimilation of Poincaré's geometrical conventionalism led him to a more systematic view of conceptual engineering than is suggested by accounts only concerned with his discussions of explication. This is achieved by illuminating how Carnap's unique philosophical stance emerged as a response to Pierre Duhem's holistic thesis and as an integral component of his endeavor to provide a philosophical analysis of Albert Einstein's General Theory of Relativity. In the third and final section, I argue that Rudolf Carnap's mature philosophy, characterized by the linguistic framework approach, combined both a conventionalist and holistic perspective. For instance, Carnap represents this feature in his response to Strawson's critique of the conventionalist project: "exactness and clarity are best achieved by a certain degree of systemization. Therefore, the explicatum usually belongs to a systematic conceptual framework" (Carnap 1963, 936). I argue that it is this broader, conventionalist-holistic view that shields the conceptual engineering project from

Quinean criticism. This reassessment not only highlights the enduring relevance of Carnap's philosophy but also underscores its pivotal role in contemporary philosophical debates.

Lund, Matthew

N.R. Hanson's Peculiar Legacy

Abstract: Norwood Russell Hanson is now remembered as a firebrand, as a great critic of logical empiricism, and is routinely grouped with Toulmin, Kuhn, and Feyerabend as an enfant terrible. Against this popular characterization of Hanson, this paper argues that Hanson is best viewed as – to use his own favored expression – a via media between different forms of analysis and distinct theoretical persuasions. Hanson eschewed the parlor games that gave ordinary language philosophy a bad name, and turned the tools of linguistic analysis to science. Hanson's science, understood as an epistemological system to be worked out through inquiry, was adventurous and lively. Hanson learned about science from some of the premier scientists of the day: Werner Heisenberg, J.A. Ratcliffe, Fred Hoyle, Paul Dirac, and J. Robert Oppenheimer were among his eminent colleagues. From Herbert Butterfield, A. Rupert Hall, and Marie Boas Hall, Hanson acquired the skills of a historian of science. Hanson owed much to Gilbert Ryle (the originator of the term 'theory-laden'), Rudolf Carnap, and Karl Popper – Hanson never mounted explicit philosophical critiques of ordinary language philosophy, logical positivism, or falsificationism. Hanson saw himself as performing philosophical analyses of key concepts in science rather than putting forward a distinctive model or theory of how science does, or ought to, function. He championed concepts as indispensable, and irreducibly philosophical, elements of all science. Hanson saw himself as extending conceptual analysis into domains that his predecessors had avoided. For instance, his famous claim that observation is theory-laden resulted from applying philosophical and empirical analysis to a subject that had been previously defined as off-limits. In addition, Hanson argued for the continuity of scientific inquiry since the Scientific Revolution – despite the dramatic innovations in scientific theory in the 20th century, Hanson saw science as still unified by the basic quest for intelligibility that originated in the 17th century. Hanson also argued for a logic of discovery. Hanson viewed discovery as yet another area where philosophers, historians, and practicing scientists could share their perspectives to make better sense of science. Viewing Hanson's philosophy as he viewed it, not as his critics and borrowers did, reveals that it is focused on connecting and synthesizing intuitions from different theoretical and disciplinary perspectives.

Mahant, Nikhil

Strawson and Russell on Descriptions

Abstract: Strawson and Russell on DescriptionsStrawson's paper ['On Referring', 1950] is a wonderful paper marred only by a quite unnecessary discussion of Russell's theory of descriptions.Paul Grice, as told to Stephen Neale (Carlini, 2020 28:44)The current philosophical orthodoxy has it that in opposition to Russell (1905), who gave a quantificational analysis of definite descriptions, Strawson (1950) took definite descriptions to be referential expressions: like proper names, definite descriptions refer to individuals but unlike proper names, definite descriptions also introduce a presupposition concerning the existence of an object satisfying the description. Russell and Strawson are therefore thought to represent opposing sides in the debate about the correct analysis of English definite descriptions. This article makes a minor but controversial point. I will argue that the orthodoxy has been wrong in its Strawson exegesis-§2 argues that Strawson's position on definite descriptions is not inconsistent with that of Russell. Apart from what he calls 'uniquely referential uses' (which are his exclusive focus in 'On Referring') Strawson acknowledges that definite descriptions admit of a variety of other uses: for instance, generic uses (e.g., 'The whale is a mammal') and predicative uses (e.g., 'Napoleon was the greatest French soldier'). Strawson's view about uniquely referential uses therefore does not rule out uses in which a definite description is used not to refer to an object, but-as Russell thought-to assert the existence and uniqueness of an object satisfying the description.Beyond this significant yet purely interpretative point, this article has a larger objective: it sketches a position that remains unoccupied in the broader debate concerning the semantics of singular terms. Analogous to the view that Strawson adumbrates for definite descriptions, §3 motivates the view that all sorts of singular terms-proper names, deictic pronouns, demonstratives-admit of a variety of different uses. In other words, it motivates the view that all singular terms are, to use Russell's (1957) phrase, 'egocentric' expressions. I will also argue that this theoretical stance raises the bar for what would be needed for analyses that seek to provide a unified semantic account for definite descriptions or other singular terms (e.g., Elbourne, 2005, 2013; Neale, 1990) to be successful.References:Carlini, C. (2020, November 20). Analyzing Language: Stephen Neale on Bertrand Russell's Philosophy of Language (Part 1). https://www.youtube.com/watch?v=sQ4bZbGxIEYElbourne, P. (2005). Situations and Individuals. MIT Press.Elbourne, P. (2013). Definite Descriptions. Oxford University Press.Neale, S. (1990). Descriptions (Vol. 43). MIT Press.Russell, B. (1905). On Denoting. Mind, XIV(4), Article 4. https://doi.org/10.1093/mind/XIV.4.479Russell, B. (1957). Mr. Strawson on Referring. Mind, 66(263), 385–389.Strawson, P. F. (1950). On Referring. Mind, LIX(235), Article 235. https://doi.org/10.1093/mind/LIX.235.320

Mantovani, Mattia

The Iconography of the Animal Machine. A Cartesian Atlas

Abstract: The doctrine of bêtes machines has long been considered a hallmark of René Descartes' philosophy. As of late, however, a growing number of scholars is claiming that this (ill-)famed thesis was never defended by Descartes himself, but has been projected onto his works due to the illustrations accompanying the posthumous publications of his Treatise on Man (Leiden 1662 and Paris 1664). In my talk, I put to test this reading by considering the iconographical legacy of these editions and, more specially, two series of documents: the 1670s engravings produced by Louvain university printers, which students pasted in their notebooks, and the engravings of a 1680 article published in one of the earliest scientific journals of the time, the Miscellanea curiosa medico-physica, in which a Stuttgart physician described a "circulatory statue" of his making. These documents show how the "Cartesian" images of the animal machine adapted to the most disparate genres, audiences and interests: the notebook and the newspaper article; the students of a renowned Catholic university and the members of a young German academy; the needs of teaching and those of diagnostics. As I demonstrate in my talk, the history of bêtes machines is indeed also a history of images.

Marasoiu, Andrei

Positivistic induction: the Mill-Whewell debate revisited

Abstract: What is induction, so that it may subtend scientific method regardless of the specific scientific inquiry being pursued? On the heels of empiricist and positivistic outgrowths of 19th century scientific methodology, Mill and Whewell both critically - but constructively - position themselves with respect to Auguste Comte's philosophy and previous empiricist, naive realist and sensationalist approaches (Hume, Reid, Condorcet). The fundamental question both Mill and Whewell seem to pose is how to move from particular observed instances to an inductive generalization that captures both the specificity of what was observed and the nomic, or law-like, character of the proposition advanced as inductive generalization.Current scholars of the Mill-Whewell debate (from Larry Laudan to Laura Snyder and A. Cobb) emphasize the differences between the two in how they construe induction, and attempt to unify the conceptions of induction produced by each, in an effort to provide a coherent guide to author-based views. In contrast, we aim to thematize the key notion of "inductive practices" so as to capture both the diversity and the unity of purpose different aspects of the scientific method which have at times passed for "induction" for either Mill or Whewell (or both). Inductive practices, as we conceive and articulate them, consist in a plurality of methods designed to produce law-like statements from experimental data. This description is intended to be neutral between alternative formulations of what induction consists in by both Mill and Whewell, and can capture, we argue, the key insight of both.Our title is "Positivistic induction" precisely to emphasize the common approach Mill and Whewell exhibit in identifying what the positivity of inductive practices may consist in. We argue that the realism-positivism debate is a red herring when it comes to reconceiving the role of induction as part of the scientific method in both the natural and the moral sciences. Rather, the issue is whether, purely positively, a concept which both Mill and Whewell can use, there a general-purpose inductive procedure that explains domain-specific generalizations carried out in fields as different as mechanics, the theory of (card) games, medical chances, moral propensions, and so on. This neutralist stance, which distinguishes a metaphilosophical debate (realism vs. positivism) from a philosophy-of-scientific-practice one (explanatory unity vs. particularism - whether induction is best construed as a unified concept or best seen as material and domain-specific), has, we argue, eschewed scholars of this debate even though it is in sync with more recent approaches in formal (mathematical) theories of inductive generalization (or projection) rules.

Marschall, Benjamin

Herbert G. Bohnert: The Last Carnapian

Abstract: Rudolf Carnap and Herbert G. Bohnert met in 1942, when Carnap was teaching and Bohnert studying at the University of Chicago. They became lifelong friends and philosophical allies. In his last letter to Carnap, Bohnert expresses regret that he did not manage to play "a Bernays to your Hilbert". He had hoped to be the one who summarises, refines, and popularises Carnap's philosophy, thus bringing it to a wider audience than Carnap's own technical work reached. But things did not pan out that way. Bohnert was not influential during his lifetime, and nowadays he is a completely forgotten figure. I will argue that Bohnert's writings merit renewed attention. While being no Bernays, he played a role akin to Goethe's Eckermann or Johnson's Boswell. His papers contain records of his many conversations and other personal interactions with Carnap, and those illuminate aspects of Carnap's philosophy that are missing from the published work. I will illustrate the value of this material by drawing on Bohnert's detailed accounts of Carnap's teaching. Bohnert observes that Carnap sometimes conveys the impression that the "acknowledgement of a realm of 'pragmatics' was only to dispose of matters [Carnap] thought little about". Against this Bohnert notes that the "interplay between the literal-cognitive and the inexplicit-pragmatic" actually plays a central role for Carnap's philosophy (1970: XXXVII). The importance of pragmatics – the study of language understood as an activity of people – has likewise been emphasised in more recent scholarship (Stein 1992, Ebbs 1997, Ricketts 2003, Carus 2007). Some fundamental questions about pragmatics remain contentious, however, such as whether Carnap's constructed languages need to be usable by speakers, and, if so, what usability amounts to (Marschall 2022). Carnap says little about this in his publications. But, as Bohnert reports, Carnap explicitly addressed the learnability of formal languages – including Logical Syntax's Language II – in his seminars (1975). This discussion, so I will argue, supports those who think that there must be a behavioural criterion for whether a speaker has adopted a formal language (e.g. Creath 2006). In the late 1980s a Carnap-revival began. The 1970s are a lost decade, however, during which interest in Carnap's philosophy was at an all-time low. Arguably this explains why Bohnert's writings originally fell on deaf ears. After several decades of switching back and forth between academia and industry, Bohnert became a professor at Michigan State University in 1968. He started to publish regularly on Carnapian themes, but the timing was not good. When the much-delayed Schilpp-volume on Carnap finally came out in 1964, a reviewer already noted that "philosophical fashions have shifted away from Carnap's preoccupations" (Wilson 1965). This became even more pronounced after Carnap died in 1970. Times have changed for the better, but even today scholars rarely study Bohnert in depth. He deserves a second hearing.

Masi, Felice

In Search of (Lost) Extension. Realism, Intentionality, and Accessibility in Husserl

Abstract: Except for Schröder, the exclusively extensional interpretation of predicative logic was poorly received by late 19th-century German logicians. Both Frege and Husserl wrote reviews of the Vorlesungen. Both agreed on the distinction between calculus and logic. While we know what Frege's position was in this regard, we do not know Husserl's equally well. After years of intensive work on these topics (1891-94), Husserl never discussed them again. In 1894, he engaged with Twardowski, conceived the idea of phenomenology, and developed the notion of intentionality.This talk argues that, at that point, Husserl needed to justify extensionality and its reality. Intentionality and its subsequent reformulation as a principle of accessibility served this purpose. Consequently, not accepting intentionality meant either not accepting it as a justification for extensionality or not accepting the need to justify extensionality at all. The latter option is found in Carnap's Aufbau. Carnap, however, used the principle of accessibility as a demarcation criterion.The first part of the talk describes the context of the Husserlian analysis of the concept of extension: on the one hand, Lotze's distinction between real and formal meaning in logic; on the other, the debate on judgement involving Bolzano (predicativity), Brentano (existentiality), Jevons (identity) and Schröder (subsumption).The second part analyzes Husserl's writings on logic (1891-1894), the controversy with Voigt and the correspondence with Venn. It examines the criticism of the subsumption sign adopted by Schröder, in which subordination and equality are merged (a criticism later shared by Church); the proposal of a calculus of conceptual objects, rather than intentions; and Venn's thesis, expressed in Symbolic Logic (2nd ed. 1894), that Husserl occupied an intermediate position between intensionalists and extensionalists.The third part compares Husserl's and Twardowski's distinctions between content and (intentional) object to analyze the dual function of intentionality, expressive and referential. The result is a state-from-it semantics, in which each assertion always has a state of affairs (an intension) as its content and references at least one individual object (an extension) to which one can have empirical access, but which does not coincide with the expressed state of affairs. The solution to this problem – the difference between 'state of affairs' and 'individual object' – is offered by the principle of accessibility, formulated by Husserl as a demonstration of transcendental idealism (1913) and taken up by Becker (1923) in the form later adopted in Aufbau. The conclusion argues that Husserl provides an example of how German logic at the turn of the century identified extensionality as the best expression of the problematic relationship between logic and the world, which is still decisive in Quine's confirmed extensionalism.Essential Bibliography:Becker, O. (1923). Beiträge zur phänomenologischen Begründung der Geometrie und ihrer physikalischen Anwendungen, «Jahrbuch für Philosophie und phänomenologische Forschung», 6, 385-560.Dipert, R. R. (1991). Individuals and Extensional Logic in Schröder's ,,Vorlesungen über

die Algebra der Logik", «Modern Logic», 1/2-3, 140-159.Husserl, E. (1979). Aufsätze und Rezensionen (1890-1910), Nijhoff.Husserl, E. (2003). Transzendentaler Idealismus, Springer.Peckhaus, V. (1997). Logik, Mathesis Universalis und Allgemeine Wissenschaft, Akademie.

Matsui, Takaaki

Carnap, Esperanto, and Language Engineering

Abstract: As he describes in his autobiography, Carnap was interested in problems of language construction in two very different domains: one is the construction of language systems in symbolic logic, and the other is the construction of auxiliary languages for international communication, such as Esperanto. According to Carnap, although these two projects of language construction have different goals, and the problems and their possible solutions in these two domains are very different, there is a psychological affinity between them. As Carnap notes, Leibniz, Peano, and Couturat were also actively involved in both projects. While much work has been done on Carnap's engagement with symbolic logic, there have been few studies of his engagement with international auxiliary languages. The few exceptions are McElvenny (2013; 2017) and Lins (2022). McElvenny examines Ogden's Basic English project in relation to Neurath's ISOTYPE project and Carnap's interest in Esperanto and other international auxiliary language projects. Lins investigates Carnap's involvement with Esperanto in detail, using his diaries and archival materials. However, important questions remain: How might Carnap's interest in the construction of international auxiliary languages be related to his interest in the construction of language systems in symbolic logic? And how might an understanding of his engagement with international auxiliary languages be relevant to an understanding of his philosophy? The aim of this paper is to answer these questions, using not only his published works but also his diaries (Carnap 2022a; 2022b), his correspondence with Neurath (Cat and Tuboly 2019), and other archival materials available in the Virtual Archive of Logical Empiricism (VALEP). First, I show that Carnap used his expertise in symbolic logic to compare the logical structures of various international auxiliary languages. According to Carnap, some international auxiliary languages are less satisfactory than others in terms of the expressive power of quantifiers, so that the translation of sentences containing quantifiers into English may change their meaning. Second, I argue that some of Carnap's key philosophical ideas are better understood if we take into account his interest in international auxiliary languages. For example, his claims that different linguistic frameworks serve different purposes and the choice among them is a practical matter that depends on purpose apply particularly well to international auxiliary languages and their choice. Third, turning our attention to Carnap's interest in international auxiliary languages sheds light on his hitherto neglected concern with linguistic injustice. Carnap was attracted to the humanitarian ideal of improving the understanding between people across linguistic barriers. He expresses his regret that the work of Lesniewski and Kotarbinski, written in Polish, was inaccessible to most philosophers in the world, and calls for the need for an international language for science.

McCoy, C.D.

There is thus not a "time of the philosophers": Philosophy of time and empiricism in Bergson and Einstein

Abstract: In introducing Albert Einstein on the occasion of his visit to the Societé Française de Philosophie, co-founder of the society Xavier Léon remarked that "la date du 6 avril 1922 fera époque dans les Annales de notre Société et c'est pour elle un honneur dont elle sent tout le prix que la présence du génial auteur de la théorie de la relativité restreinte et généralisée." The occasion can in fact be seen as marking an epoch in the philosophy of science as well, for it marks the meeting and confrontation of the most lauded 20th century physicists, Einstein, and the most lauded philosopher in the world at the time, Henri Bergson. At the meeting, Bergson, somewhat reluctantly, offered critical remarks on the philosophical interpretation of time in the theory of relativity on the basis of views developed in his famous books Essai sur les données immédiates de la conscience, Matière et mémoire, and L'Évolution créatrice. Einstein summed up his brief reply to Bergson's remarks with the famous retort, "Il n'y a donc pas un temps des philosophes; il n'y a qu'un temps psychologique différent du temps du physicien." Time, then, became a key battleground between (what might be crudely put as) the scientific worldview and the philosophical worldview.Jimena Canales' recent book The Physicist and the Philosopher has thoroughly traced the history and context of this debate on time. In this paper, my goal is to build on her exemplary historical work by narrowing the focus to the philosophical and scientific influences that trace through this event, as they are realized in the views of Bergson and Einstein, in order to bring to the fore their differing (but similarly positive) assessments of the place and significance of science in our worldview, especially regarding the relation of science and philosophy. Important in this regard are Bergson's 19th century predecessors in the "French Spiritualist" movement, such as Félix Ravaisson and Émile Boutroux, and the influences on Einstein's thought from Henri Poincaré, Ernst Mach, and David Hume. Having gathered the key threads that bind the philosophy of time and science in the Bergson-Einstein confrontation, I conclude by outlining these threads' subsequent careers in the bifurcating interpretations of what an "empiricist" philosophy, and specifically an "empiricist" philosophy of science, can be, as witnessed in later 20th century "empiricisms," such as Gille Deleuze's "Bergsonism" on the one hand and Bas van Fraassen's "constructive empiricism" on the other.

McDowell, Jacob

Thomas Kuhn and the Causal Theory of Reference

Abstract: It is typically held that incommensurability was only a possibility on Thomas Kuhn's view given his commitment to a descriptivist conception of the meaning of theoretical terms. On such a reading of Kuhn, the meaning a theoretical term is wholly constituted by the definition used for that term by a given community of scientists. If the definition changes while the same string of symbols remains in use, the earlier and later term are in no way talking about the same thing, and the theories in which they figure are incommensurable. Causal Theories of Reference ala Saul Kripke and Hilary Putnam, however, rule out incommensurability by allowing the extension of a term, rather than merely the intension (or speaker's definitions) to (at least partly) constitute the meaning of the term, and thereby ensuring that part of the 'meaning' remains constant across theory changes. Given this reading, it is therefore surprising to find Kuhn endorsing the causal theory of reference in several later essays while still maintaining the possibility incommensurability. For instance, in "Metaphor and Theory Choice" Kuhn declares that he sees in "the causal theory of reference a significant technique for tracing the continuities between successive theories and, simultaneously, for revealing the nature of the differences between them" (p.104). This paper would investigate how Kuhn understood both the causal of theory of reference and incommensurability, such that his endorsement of both was not the bald-faced contradiction it would be according to the standard reading of Kuhn. The paper will also explore the relation of Kuhn's appeals to natural kinds in works collected in The Last Writings of Thomas S Kuhn, and their relation to views of natural kinds in Putnam, Kripke and mainstream analytic philosophy.

McNulty, Bennett

What Is This Thing Called HOPOS?: Love, Marriage, and Disciplinary Fragmentation

Abstract: This paper explores the nature of History of Philosophy of Science (HOPOS) as a discipline and intellectual community. I argue, especially means of historical contrasts with related approaches, that HOPOS is an isolated archipelago of loosely related historical studies, lacking substantial teleological and methodological unity. I conclude by noting the costs and benefits of this disunity.1. Teleological DisunityIn the wake of Kuhn's The Structure of Scientific Revolutions (1962) and the downfall of logical positivism, the relationship between history and philosophy of science was heavily scrutinized. Some debated whether history and philosophy of science form a genuine union, or 'marriage' (Giere 1973, McMullin 1974, Burian 1977), while others sought better to systematize the way in which history provides case studies for the philosophy of science, culminating in the Scrutinizing Science project (Donovan, Laudan, & Laudan 1988). However, foci shifted with the practice turn in the philosophy of science. As philosophers of science became more interested in local studies of particular scientific practices, wide-ranging theorization about explanation, theory change, confirmation, and so forth also abated. Likewise, after the practice turn, the ambitions for the use of history in philosophy of science became more humble (Dresow 2020). Indeed, official documents from the formation of HOPOS (qua society) express no stance on the role of history with respect to philosophy of science, nor can a univocal position on this relationship be discerned in HOPOS scholarship. In lieu of an overarching commitment, the field lacks a unifying end; the medley of HOPOS scholarship wants for teleological unity.2. Methodological FragmentationInitially, HOPOS (qua society) was formed so as to be methodologically inclusive, and that inclusivity persists in the field today. HOPOS embraces "diverse methodologies," (Howard 1992, HOPOS 1993, HOPOS 2023) and "methodological pluralism" (Patton 2010, 2), which includes "more internalist, history of ideas" along with "more externalist, social and institutional history" (Howard 1992).This inclusivity contrasts not only with late-20th-century historically informed theorizing about science, like the Strong Programme (Bloor 1976) or the Scrutinizing Science project, but also with more contemporary approaches, like a resurgent Confrontation Model (Hull 1992; Scholl 2018), Historical Epistemology (Daston 1994, Daston & Galison 2007), Synthetic History (Domski & Dickson 2010), and Dynamics of Reason (Friedman 2001). All such approaches assume a unifying methodology at some level of granularity. While HOPOS' methodological pluralism has benefits, like sidestepping the virulent internalism/externalism debate, it also splinters the field.3. Love is All You NeedWithout unifying ends or methodologies, HOPOS scholarship shares little more than a name. Indeed, in the initial statement about the formation of the HOPOS working group, conveners are described as motivated by a "simple love of history" (Howard 1992). This paper concludes by examining the trade-offs of HOPOS' lack of unity, posing questions like whether this

disunity is appropriate to our community goals. Is shared love alone enough to hold us together?

McOuat, Gordon

Radicalising the predicate: Can modern logic have a social history, and is it worth trying?

Abstract: This paper explores the origins of modern mathematical logic in the struggles over political radicalism of the early 19th century. At the very foundation of those struggles sat a thorough rethinking of the very meaning of logic and its structure, setting the stage for the great revolution in mathematical logic in the later 19th century.Jeremy Bentham, familiar mostly for his radical moral/political philosophy established on his principle of utilitarianism, proposed as the proper grounding of his proposed political/moral reform a through overthrow of 2000 years of logical tradition – founded on a concept of ultra-nominalism and attack on Aristotelean essentialism. For Bentham a reform in logic was far more significant and foundational than all his reworking of ethics, legal theory and prison reform. Bentham's notebooks team with explorations of logic. Logical notes crisscross with proposals for constitutional and prison reform. Several volumes of his notebooks were wholly devoted to the study of logic, haunted with the urgent yet unrealized hope of producing a work specifically on the topic. Bentham's posthumous editor, Bowring, struggled with the glut of notes and eventually gave up, giving us a Bowdlerised version of what he could make out, all tucked into a single volume (Volume 8) of Bower's Collected Works of Bentham (1843). Yet, almost nothing of Jeremy's proposed Logic appeared in print in his lifetime (or since – the editors of the new "Collected Works" are still struggling!) -- except in Bentham's essay on language and some promising appendices in his enormous work on educational reform, the Chrestomathia of 1815. It was left to Jeremy Bentham's nephew and amanuensis, George Bentham(the future President of the Linnean Society of London) to bring the new logic into fruition and public life, and in the process going far beyond anything Jeremy had proposed in his long encounter with logic. George's Essai sur la nomenclature et la classification des principales branches d'art-et-science (1825), ostensibly a translation of the logical section of Jeremy's Chrestomathia, introduced the French to Benthamite radicalism, with the aim to push the nominalist reform of the French Encyclopedists one step further into logic. But it is George Bentham's Outline of a New System of Logic (1827), based on a reading of Jeremy's notes, that would introduce a thoroughly "Radical" reworking of logic, both in its meaning and in its procedures and structures, and would later spark a vicious priority dispute between conservatives and radicals over the discovery of the quantification of the predicate and the origins of modern logic.Drawing on hitherto unpublished papers of both George and Jeremy Bentham and others in the radical reform movement, this paper will examine the "quantification of the predicate" and the sweeping proposal for a nominalist (non-essentialist) logic in relation to the Radical political reform movement of the early and mid 19thCentury, and what this might say to reimagining our stories of the history of modern logic.

Meer , Rudolf

An Alternative Kantian Inspired Understanding of Einstein's Theory of Relativity. On the Philosophical Background of Ilse Schneider

Abstract: Ilse Schneider combines elements of positivism and neo-Kantianism in her studies and integrated these different approaches in her German book "Das Raum-Zeit-Problem bei Kant und Einstein" (1921). In addition, her philosophical studies were always combined with concrete physical research questions. Schneider's book illuminates "the profound and far-reaching problems of the theory of relativity, its foundations and its consequences, from an epistemological perspective" (Schneider 1921a, 1). Beyond the detailed physical questions of the early 1920s, it is a programmatic attempt to reconcile Kant's transcendental idealism with Einstein's theory of relativity. Although her book was published in the same year as the famous ones of Ernst Cassirer and Hans Reichenbach it has been almost forgotten. This is because too little attention has been paid in research to the fact that Schneider's book, like that of Cassirer, is Kantian in inspiration, but nevertheless shows significant differences. These primarily concern the realist reading of Kant and, respectively, the postulated close relationship between philosophy and the sciences.In opposite to positivism, especially to Moritz Schlick, Cassirer applies his formal or epistemological idealism to Einstein and immunizes Kant's theory of space and time against physics (Cassirer 1921, 419). In doing so, he concedes the existence of something mind independent but all we can know about it is held to be permeated by the creative, formative, or constructive activities of the mind. In this sense, all claims to cognition must be considered to be a form of self-cognition (Cassirer 1921, 419). To make a long story short, Cassirer argues for a one-world-interpretation and gives up the 'independence thesis' of a thing in itself. This in turn leads to a strong separation of philosophy and science.In contrast, Schneider represents a more realistic criticism. She argues for a methodological dual aspect interpretation of Kant's transcendental idealism. In doing so, she is with one foot still in the tradition of critical realism (like Külpe, Messer, etc.). To be more concrete, Schneider's approach falls back on a specific interpretation of Kant – that is the one of Alois Riehl. Consequently, she does not separate philosophy and science in the strict sense that Cassirer does and writes for example: "The general theory of relativity allows the truest interpretation of transcendental philosophy." (Schneider 1921a, 47) Unfortunately, Schneider was no longer able to address Cassirer's position in detail. However, in 1921, she hints at this criticism in a review of Cassirer's book (Schneider 1921b).If Schneider's approach had been taken more into account and if her independence from the philosophy of Cassirer and Schlick/Reichenbach had been recognized, the conflict between positivism and Kantianism would certainly have been interpreted differently. Her interpretation of the theory of relativity bridges the gap between positivism and neo-Kantianism.

Meyer, Steven

John von Neumann the First Post Hilbert Programme Philosopher of Computation

Abstract: .John von Neumann was arguably the best applied mathematician of the20th century. Beginning in the early 1940s, Neumann devotedmost of his research effort to studying computation (W. Aspray, "John vonNeumann and the Origins of Modern Computing", 1990). Neumann both designedbuilt computers and studied philosophy of computation. He believed that computationmerited study itself not just study of computing applications applied to scientificproblems. Neumann continued the traditional going back to the founders of modernphysics Max Planck and Albert Einstein who called themselves naturalphilosophers and studied methodology and philosophy as part of scientific research.Neumann criticized Rudolf Carnap's theory of information and advisedClaude Shannon to use the term entropy for Shannon's theory of information(Kohler, E. in Vienna Circle yearbook 8, 2001, 97-134). In my view, too oftenthe historical image of Neumann includes only his pre WW II work on formalquantum logic. The possible connection of Neumann's philosophical writingsto Thomas Kuhn's "The Structure of Scientific Revolutions"period is discussed in Collodel[2014]. Neumann's study of the philosophy ofcomputation involved discussions with the founders of modern physics inparticular Wolfgang. (Thirring, W. inVC yearbook 8, p. 5, 2001).The study of computation is currently often viewed as two areas. One isscientific study of the brain and mind. The other is the engineering activityof building intelligent artifacts. John von Neumann's development of thefirst digital computers and the now almost universally used von Neumanncomputer architecture arose from Neumann's study of philosophy.In order to appreciate Neumann's philosophy of computation,his terminology needs to be understood. Neumann's use of the word "automaton"does not have the modern meaning from brain science but should be readas "computer". The word Automaton made sense because it was still not clearin the late 1940s if it was even possible to build digital computers. "Selfreproducing automaton" should be read as a computer program that writes(generates) other computer programs. "Axiomatics" should be read as "algorithm".This paper discusses the modern alternative to Turing Machines called MRAM modelthat includes the von Neumann architecture array selects and has a finite number ofunbounded size memory locations (Hartmanis, J. in Lecture Notes in CS, vol. 26,1974, 1-49). I argue that Neumann's axiomatics involved his thinking aboutalternative machine models that are now expressed as algorithmics of the P?=NPproblem.Eckehart Kohler in his excellent and detailed paper "Why von Neumann rejectedCarnap's Dualism of Information Concepts" (Kohler op. cit.]) argues thatNeumann's (and Pauli's) criticism of Carnap was incorrect because Neumannwrongly assumed information only has physical meaning. Both Neumann and Pauliwere so sure of their criticism that they recommended Carnap not publish hisstudy of information. Kohler's mistake is that starting after world IINeumann rejected the very idea that formal sentence

based logic can describereality. I interpret Kohler and Carnap as arguing that Hilbert's programme couldstill be saved by using the dual idea that information has both a logical elementand an empirical element (p. 117).Neumann's advice to Claude Shannon (rejected by Shannon) to use theterm "entropy" for Shannon's codes is also discussed. Various arguments fromNeumann and physicists that information required physical signals are alsodiscussed.

Mihaljević, Katarina

Rose Rand's philosophical lifework – narrative models as research tools

Abstract: Rand's philosophical lifework spanned three continents and lasted over fifty years where, despite her best efforts, she was never able to land a permanent academic role. Her work in logic is considered as a part of the efforts to formulate what is today known as deontic logic. Her philosophical projects evolved together with her environment and thus, it is possible to read the work she did on the language analysis, Wittgenstein's impact on the philosophies of Vienna Circle as well as the various topics in philosophy of language. Besides that, her work consisted of numerous important translations, commentaries, and the notes of the important meetings at the Vienna Circle. I have argued elsewhere that the Rand's lifework is a great study case for understanding the influence of external factors in determining the success of publishing one's philosophical work, such as the peer-review, workspace, mentorship and the social status. In Rand's case, systematic absence of these factors has inevitably contributed to her failure to secure a permanent academic position and promotion of her philosophical work. Researching the processes surrounding her career has resulted in seeking to propose historical contexts and narrative models as research tools. As observers, we are facing a unique contrast between the twenty-first century technological prowess, nineteenth century understanding of archival science and tribal-style disciplinary divisions. As historians of philosophy, we can help make sense of the effects of digitization on our materials, and effectively our fields. As far as the philosophy is concerned, we have discussions about the absorption of new technologies (e.g., digital humanities) or lend views on ethics of archival research and value of conducting historical research. Firstly, to help bridge the gap between the two disciplines and secondly, to aid new types of research and challenge the traditional approaches to doing archival research on marginalized figures in the history of philosophy. Focusing on my own five-year research in various archives documenting work and life of the Viennese philosopher Rose Rand, I will use the lessons learned to explore a new way for historians of philosophy to engage with the archival records they research. One such way is building the narrative models that look to answer questions about e.g. the production of philosophical work or mechanisms of academic transfers. Not only could this be useful for archivists in making sense of their collections but also, these narrative models could prove useful to those looking to research the same archives but with no clear approach or research direction.

Milkov, Nikolay

Hans Reichenbach, Helmuth Plessner and the Scientifically Informed Philosophy in the Weimar Republic

Abstract: Hans Reichenbach often presented himself and his friends from the Berlin Group as standing alone in their attempt to bring philosophy in Germany close to science. In this paper I'll try to show that the new philosophical anthropology of Helmuth Plessner went in similar direction-Reichenbach and Plessner both tried to closely connect philosophy with science. Moreover, in their analyses Reichenbach and Plessner applied the same method-the method of topological ontology. Reichenbach did this first when he adopted the term genidentity in his philosophy of nature, Plessner in his investigation of the "ontic form" (1928, 168) of the organic world in which the concept of boundary played a central role. To be more explicit, Reichenbach introduced in his natural philosophy Kurt Lewin's term genidentity already in his first book, Theory of Relativity and a priori Knowledge (1920). He also used it in The Philosophy of Space and Time (1928) and later in his posthumously published work The Direction of Time (1956). In his essay "he Causal Structure of the World and the Difference between Past and Future" (1925), Reichenbach, again under Lewin's influence, introduced another notion of topological ontology-"bifurcation of time". The method of topological ontology played a central role also in Plessner's "philosophical biology". The basic idea in his opus magnum, The Levels of Organic Life and the Human: Introduction to Philosophical Anthropology (Die Stufen des Organischen und der Mensch (1928), was to arrange the various genera of organisms according to the relationship between individuals and their boundaries. This involved their "positionality" or staking out in their environment. In other words, Plessner presented the biological individuals as topological beings determined by the environment through their boundaries. Despite these similarities, a clear difference remained between Reichenbach's and Plessner's analyses of the newest results of science. Reichenbach made it within the framework of epistemology-in this respect, he continued the philosophical program of the Neo-Kantians. He wanted to find out what we can know (erkennen) and, in final reckoning, what the building blocks of the world are. In contrast, Plessner tried to build "a logic of living form". To be more explicit, his program was a kind of "philosophical biology" that investigates "ontological necessity" (1928, 321) but strictly in accordance with the latest results of science. Apparently, his philosophy was a true successor of the old metaphysics. The relatedness between Reichenbach and Plessner was not only theoretical, though. Both philosophers founded journals that had as an objective the cooperation between philosophy and science. In 1925 Plessner started to publish the journal Philosophischer Anzeiger. Zeitschrift für die Zusammenarbeit von Philosophie und Einzelwissenschaft. Five years later Reichenbach, together with Carnap, launched the legendary journal Erkenntnis. Among other things, Plessner arranged a symposium on Reichenbach's The Philosophy of Space and Time (1928)

in the journal that he edited. This was the only open discussion of Reichenbach's masterpiece in Germany of the time.

Miller, Ryan

The Modeling Revolution: A Case Study in Descartes' Optics

Abstract: Descartes' rhetoric about a radically new and certain mathematical method in science would justify the appellation "revolution" if it were accurate. Unfortunately, Descartes takes the infinite speed of light as the most certain belief on which his entire philosophy stands, his explanations of sense-perception and weather seem anything but certain or mathematical, and even by the standards of his own day he errs in rejecting Galileo's law of fall and Galilean invariance. Attention has therefore shifted to Galileo as the locus of idealization and mathematicization constitutive of a scientific revolution. While Galileo's arguments have held up better than Descartes', however, the Aristotelian roots of his methods are so extensive that they look more evolutionary than revolutionary. I suggest that a genuine scientific revolution can be found by turning from Galilean idealization (which mathematicises by removing extraneous details) to model-based science (which mathematicises by creating multiple incompatible models of a phenomenon whose principles are not well-understood). I argue that Descartes' empirically successful optical texts reveal a groundbreaking use of multiple contradictory models in a way utterly rejected by the scholastics.Descartes certainly uses Galilean idealization in his discussions of gravity and inertia, but his pathbreaking Optics relies on stronger idealizing assumptions which allow self-contradiction-the hallmark of modern model-based science. Here Descartes claims that his demonstrations concerning refraction do not depend on correctly understanding the nature of light. This change is important because Descartes makes incompatible assumptions about the nature of light between his discussion of transmission and reflection and his two discussions of refraction. In Descartes' wine-vat fluid model of transmission and his tennis-court model of reflection light must always move at infinite speed, whereas in his first model for refraction light makes instantaneous speed changes and his second model for refraction implies that the speed of light changes progressively. In the transmission and refraction cases Descartes' model for light is something like our idea of a molecule in an ideal gas-a small particle which rarely hits anything but collides elastically when it does. The angle of refraction requires a speed change, however, which Descartes motivates by introducing a sheet that the tennis balls must perforate-now suggesting that every particle of light must have an inelastic collision with the surface. Descartes then replaces the sheet with water in order to motivate the asymmetry of refraction angles and makes the ball travel faster in water than air (as if through a new impulse of the racquet when it strikes the surface) in order to salvage the correct direction of refraction. This gerrymandered model produces the right angular results at the cost of a coherent account of speed, since any slowing will be continuous or nonexistent as the ball passes through the substance. Here Descartes uses totally incompatible models to motivate different aspects of the same phenomenon, and justifies the whole by its ability to compute a focal point rather than any coherent account of the nature of light.

Mina, Andrea

Max Scheler's Project of a 'Pure Biology'

Abstract: In 19th century Germany, Darwin's theory of evolution was received by both its advocates and its opponents as the central point of contention between conflicting worldviews, rather than as matter of purely scientific inquiry (Kelly 2009). At the turn of the 20th century, Max Scheler observed that, while the fact of species evolving could no longer be disputed, different interpretations of this fact could be advanced, and these interpretations were rooted in underlying worldviews (Scheler 1913). He claimed that Darwinism – defined as the popularization of Darwin's theories by Populärwissenschaftler such as Ernst Haeckel (Scheler 1993, 279) – was founded on a mechanistic conception of nature. Its inherent mechanistic assumptions prevented it from ever truly grasping the essence of life, which, according to Scheler, lied in its perpetual state of «becoming» (Scheler 1979, 163). In contrast to «mechanistic biology,» Scheler aimed to develop a «pure biology» conceived as a science that remains faithful to the essence of life (Scheler 1997, 195). His intent was not to contradict Darwin, but rather to make theory of evolution compatible with a conception of life as a driving force (Drang) that continuously pushes organisms towards the disruption of any balance (Ana 315.B.1.89). In crafting this ambitious project, Scheler drew inspiration from a multitude of sources: from Henri Bergson's philosophy of life to Hans Driesch's holistic biology, from Jakob von Uexküll's dynamic concept of environment to the early investigations on heredity by August Weismann, Herbert Spencer Jennings, and Hugo de Vries. Scheler's writings on philosophy of biology and the «phenomenology of the living» have largely remained unpublished. Nonetheless, they provide an exceptionally rich insight on a period characterized by vibrant dialogues between philosophers and life scientists. Scheler emerges as a receptive thinker who is eager to engage in dialogue with the sciences of his time. He perceived the theory of evolution as part of a broader "transformation of the scientific image of the world," encompassing not only biology but also physics with Einstein's and Planck's theories and philosophy of science with Ernst Mach, Pierre Duhem, and Henri Poincaré (Scheler 1987, 141-143; Ana 315.B.1.106). In his view, the old notion of nature as a realm of static, mechanical relations among forces was giving way to a new perspective in which nature appeared as a dynamic field of energies in constant evolution. Philosophy was tasked with reinterpreting and leading this transformation.Kelly, Alfred: Descent of Darwin: The Popularization of Darwinism in Germany, 1860-1914 (Chapel Hill, 2009).Scheler, Max: Versuche einer Philosophie des Lebens, In: Die weißen Blätter 1, 1913: 203-233.—— Schriften aus dem Nachlass. Band II. Erkenntnislehre und Metaphysik [GW 11] (Bern/München, 1979).—— Schriften aus dem Nachlass. Band III. Philosophische Anthropologie [GW 12] (Bonn, 1987).—— Schriften aus dem Nachlass. Band V. Varia I [GW 14] (Bonn, 1993).—— Schriften aus dem Nachlass. Band VI. Varia II [GW 15] (Bonn, 1997).—— Ana 315: Nachlass von Max Scheler in der Bayerischen Staatsbibliothek (München): B.1.106: Wesen und Geschichte des Pragmatismus / B.1.89: Alleben u. All-leib.

Miyake, Teru

John Herschel's "Physical Astronomy" and the Method of Residual Phenomena

Abstract: The concept of residual phenomena plays an important role in the method for doing natural philosophy that John Herschel presents in A Preliminary Discourse on the Study of Natural Philosophy. In 1823, seven years before publishing the Preliminary Discourse, Herschel wrote the "Physical Astronomy", a detailed, 83-page long article on the methods of physical astronomy for the Encyclopaedia Metropolitana. Although this article is clearly of great importance for understanding the initial formulation of the methodology presented in the Preliminary Discourse, I do not know of any previous work that examines it in detail. In this talk, I will show how the idea of residual phenomena, and other features of the method presented in the Preliminary Discourse, grew out of Herschel's review of the methods of physical astronomy. In the "Physical Astronomy", the major methodological challenge of physical astronomy is presented as being the complexity of the planetary motions. Although the planets move roughly in accordance with the Keplerian laws, there are subtle deviations from Keplerian motion that are extraordinarily complex. These deviations arise due to many sources, including, for example, the gravitational effects of other bodies in the solar system, the eccentricities of the planetary orbits, and subtler details of the solar system such as orbital resonances. The effects of individual such sources may already be complex, but the situation is made almost hopeless by the fact that the deviations may be the result of numerous sources working conjointly. For Herschel, the major task of physical astronomy is thus to separate out the various components of these deviations, and to identify the source of each component. Herschel shows how this is done using numerous examples, including detailed analyses of the Great Inequality of Jupiter and Saturn and the motion of the lunar apsides. An important feature of this method is the role of physical theory. The primary use of theory throughout the "Physical Astronomy" is as a tool that allows natural philosophers to separate out the various components that together make up the complexities of the planetary motions, and to identify the details in the solar system that give rise to them. These complexities in the planetary motions are prime examples of the kind of phenomena that Herschel would later in the Preliminary Discourse refer to as residual phenomena. I will end the talk by showing how the methods of natural philosophy described in the Preliminary Discourse, especially those involving residual phenomena, are a generalization and extension of the methods described in the "Physical Astronomy".

Mohammadian, Mousa

Peirce Disappears: (Non-)Reception of Peirce as a Philosopher of Science

Abstract: HOPOS Scholars read and hear a lot about Pierre Duhem, Ernst Mach, Henri Poincaré, Moritz Schlick, Otto Neurath, Rudolf Carnap, and Hans Reichenbach. Charles Sanders Peirce, however, is not generally considered a canonical figure in the history of philosophy of science. But in the early years of the logical empiricist movement in the United States, Peirce received a warm reception from prominent representatives, proponents, and sympathizers of logical empiricism including Charles Morris, Ernst Nagel, Phillip Frank, Hans Reichenbach, and W.V.O. Quine. This reception was short-lived though and Peirce gradually disappeared from the mainstream philosophy of science while logical empiricism turned into a formidable movement. This paper investigates the interactions between early logical empiricism and Peirce's philosophy:to study why Peirce's philosophy of science got marginalized in the course of the development of the 20th-century philosophy of science in the US, despite the warm reception that it received from early advocates and sympathizers of logical empiricism; to bring to the foreground some of Peirce's valuable insights, missed from the mainstream philosophy of science due to this marginalization.I begin by discussing some examples of the early reception of Peirce's philosophy by proponents of the logical empiricist movement such as Morris, Nagel (and his student Justus Buchler), Frank, and Reichenbach. Through these examples, I show a wide range of topics (e.g., logic, probability theory, theories of truth and meaning, and social dimensions of science) in which Peirce received a warm (though not uncritical) reception. We see that the engagements with his works are persistent (from 1930s to 1950s) and get more refined over time. Nevertheless, Peirce gradually disappears from the mainstream philosophy of science. Then I offer some complementary explanations for this disappearance based on how Peirce was understood by the above-mentioned figures. Briefly, I show that Morris describes Peirce as a relatively unsophisticated logical positivist while Ayer thinks of his philosophy as an anticipation of falsificationism. But then a more sophisticated version of Peirce's philosophy of science is available in logical empiricism or falsificationism and hence it can be dropped. Furthermore, for figures like Nagel, Frank, and Buchler, the most interesting aspect of Peirce's philosophy of science is its emphasis on the social context of knowledge production. But as it is extensively argued in the literature, due to the political aftermaths of the Cold War, philosophical discussions about the social dimensions of scientific knowledge faded out of the mainstream philosophy of science in the US.Finally, I discuss two of Peirce's interesting insights that did not receive the attention that they deserved due to the marginalization of his philosophy: (1) Peirce's dynamic and evolutionary view of scientific method that can be contrasted with the right-wing logical empiricists' static view of scientific method and (2) his view about the limitations of the intuition-based method of inquiry when the object of inquiry

is the "deep" structure of nature, an issue that recently has received much attention in scientifically oriented attacks on analytics metaphysics.

Moon, Stella

Demarcating Descartes's Geometry by Clarity and Distinctness

Abstract: Descartes's doctrine of clarity and distinctness states that whatever is clearly and distinctly perceived is true. This paper looks at his early doctrine from Rules for the Direction of the Mind, and its application to the demarcation problem of curves in Descartes's Geometry. This paper offers and defends a novel account of the demarcation criterion of curves: a curve is geometrical just in case it is clearly and distinctly perceivable. I argue that there are two kinds of ways of understanding Descartes's clear and distinct perception, in terms of deductions. On one hand, there is deduction by the faculty of memory, which shows how we can hold true propositions in memory and arrive at certainty. One the other hand, there is deduction by imagination, which can be expressed diagrammatically and involves immediate grasp at certain diagrammatic expressed truths. By looking at particular examples of geometrical curves, I explain how these two deductions are used for clear and distinct perception of curves. I argue that, in the Rules, Descartes uses both, and these deductions give rise to an epistemic criterion for demarcating geometrical curves in Descartes's Geometry.This epistemological criterion also offers explanations for the existing matheamtical criteria. They claim that a curve is geometrical just in case it can be constructed in a single continuous motion, and that a curve is geomtrical just in case it can be expressed in algebraic equations. In my interpretation, the continous motion criterion is explained by the deduction by imagination, while the algebraic criterion by the deduction by memory. Hence, my epistemic criterion is more fundamental to understanding Descartes's Demarcation problem. I further defend my view from the challenges posed by Descartes's remarks that he clearly and distinctly sees certain string-like curves. I argue that what Descartes sees clearly and distinctly is not the curve itself, but physical properties which he observes in the motion of constructing the string-like curve. Hence, this does not pose a threat to the epistemic demarcation criterion.This epistmic demarcation criterion also shows that Descartes's mathematical project in the Geometry is a foundational one for his epistemology. Geometry is the science of exact magnitudes, and what should be included in geometry is what can be epistemically known for certain. Demarcating geometrical and non-geometrical curves by clarity and distinctness explains this traditional view of geometry as a foundation of mathematics.

Moullard, Florian
Nicolas, Millot

When the fire (re)start to burn : Back to the Piaget/Chomsky debate, Royaumont 1975

Abstract: Almost fifty years later, we propose to revisit the debate between Noam Chomsky and Jean Piaget at Royaumont Abbey in France in 1975.Firstly, we want to focus less on the disagreement about their respective theories of language than on the biological bases around which the debate actually took place. Indeed, the general theme of the debate (Language and Learning) conceals the far more important role, in the discussions between supporters of the Piagetian sensorimotor/constructivist model and supporters of the Chomskyan innateist/modularist model, of two fundamental notions: that of stationary state, dear to Chomsky, and that of self-regulation, dear to Piaget (who at the time was discovering the Foerster's work on second-order cybernetics). In both cases, the issue that concerned Chomsky, Piaget and their no less famous guests (Jerry Fodor, Jacques Monod, François Jacob, Dan Sperber, David Premack, Jean-Pierre Changeux, Gregory Bateson) was the fate of the notion of information–both at the cognitive level (as a tool to understand the acquisition/inborn capacity and development of language and learning) and at the biological level (as a tool to explain the evolutionary roots of these capacities).Of particular interest to us is the way in which the colloquium coordinator (Massimo Piatelli-Palmarini 1978) clarifies the positions of Chomsky and Piaget on the basis of Imre Lakatos and Gerald Holtons' philosophies of science, exposing the 'hard cores' and 'protecting belts' of each of the two theories, and diagnosing the irreconcilable themata inspiring the two–to put it simply: flame versus crystal, i.e. 'order through noise' (supporters of self-organisation and a dynamic model of living systems) versus the innate/static paradigm (supporters of an explanation of behaviour through the genetic programme and, in cognitive terms, through the specification of modules according to a finite set of biological potentialities).The second part of our talk will attempt to tackle two problems, the first relating to history of philosophy of science and the other to history of science proper :(1) Is it possible to present developments in biology from the 2000s onwards as falling within the same epistemic categories as Piaget's genetic psychology? We will then draw particularly on Evelyn Fox Keller's work (2005) on the transition from the genomic paradigm to the Developmental System Theory programme, and on Eva Jablonka (2002) on the operational limits of a purely quantitative concept of information.(2) Did Piaget finally 'win' (a reading of the 1978 work alone shows the definite aporias of Piaget's reception of molecular biology, despite Gould and Lewontin, Henri Atlan and Foersters' parallel work), if we suggest few modifications to the protective belt of Piagetian constructivism? To do this, we will focus on the notion of information (in the biological/genetic sense and in the cognitive sense) and how it is treated by contemporary theoretical biology (Longo et al. 2012, Mossio et al. 2016). Our

interpretation of the 1975 debate will thus complement in its own way a recent re-reading, more favourable to Chomsky (Graffi 2019).

Nascimento, Rodrigo

The Epistemological and the Sociological Goals of History of Science in Philipp Frank's Philosophy.

Abstract: This talk aims at discussing the role played by the history of science in Philipp Frank's philosophical and sociological analysis. Based on works such as The Law of Causality and its Limits (1932), The Variety of Reasons for the Acceptance of Scientific Theories (1956), and Between Physics and Philosophy (1941), we recognize not only the emphasis given by the author to epistemological and logical aspects of the recent development of science of his time but also a historical perspective of science as a social activity. We shall argue that the historical perspective adopted by Frank has both an epistemological and a sociological goal. The first goal is to assess the increase of knowledge in the historical development of scientific concepts. Recovering mainly the influence of authors such as Duhem and Mach, Frank discusses the epistemological role played by scientific concepts in relation to theoretical changes in the history of physics. It is against this background that Frank develops his well-known analysis of common sense, his rendering of the law of causality, and his conventionalist approach to science. The second, sociological, goal is to understand the historical development of the relationship between science and society. Frank points out that science has historically been seen as part of hierarchical philosophical systems, in which it both influenced and was influenced by other institutions in society. This aspect becomes relevant to understand the importance of the history of science in the elaboration of a critical view of traditional philosophy. Using the first decades of the twentieth century as a historical background, the history of science appears in Frank's work as a critical tool to evaluate and reflect on the social consequences of an intellectual and scientific mainstream pervaded by mystic, esoteric, antisemitic, and racial-segregationist elements. This talk considers Frank's use of the history of science as a starting point to recognize the different elements of the relation among science, society, and philosophy.

Nemeth, Elisabeth

Scientific Norms and the Autonomy of Science. Re-considering Merton in the Light of Recent Sociology of Science (Bourdieu, Lamont)

Abstract: Reichenbach's distinction between "context of discovery" and "context of justification" (Reichenbach 1938) met a similar fate as Robert K. Merton's theses on the scientific ethos of 1938. Both had a profound impact on the philosophy and the sociology of science until the 1970s, but were considered completely outdated in the last two decades of the 20th century. The turning point is associated with Kuhn and Quine (Zammito 2004). From the 1980s onwards, new approaches to the study of science established themselves, moving – in different combinations – between history of science, philosophy of science and sociology of science (SSK, Laboratory Studies, HOPOS, HPS...). Most of them considered the context distinction as obsolete as Merton's account of science as a norm-governed institution.In my paper, following Richardson (2004), I would like to develop the historical and conceptual proximity between Reichenbach and Merton a little further. I will focus primarily on the question of whether the normative structure of science in Merton's sense can be understood as an attempt to make the context of justification itself an object of sociological investigation. I am interested in this topic with regard to later approaches in the sociology of science where this is exactly what happens: The context of justification becomes the object of sociological research as a dimension of scientific activity itself.One case here is Bourdieu. He was one of Merton's many critics in the 1970s (Bourdieu 1975). However, Bourdieu, unlike many others, never rejected the context distinction, but considered it an indispensable element of scientific work itself. (There are also other traits that link him rather to the "positivists" than to their critics, e.g. a tendency towards the "unified science" view that the human and social sciences do not have a methodology fundamentally different from the natural sciences.) Like Merton, Bourdieu wanted to pursue the sociology of science as part of general sociology and thus to get a sociological view of science in its relation with other social fields. Importantly, he later relativised his 1975 criticism of Merton and even came to defend Merton against his critics (Bourdieu 1990, 2004).My focus will be on the fact that Bourdieu's theory of the scientific field assigns a defined sociological place to the norms of scientific activity. They are the subject of constant disputes between the "players" in the field, who demand compliance with existing norms from each other, but who can also question the justification of these norms and struggle for changing them. An essential function of these disputes is that norms intruding from the outside (which tend to be brought in from other fields by each player) are refracted (like a lightbeam) and not only repelled. This process is what constitutes the "relative autonomy" of the scientific field. Here lie key similarities, but also differences, between Merton and Bourdieu. As an example of how this approach can be further developed and concretised I will discuss Lamont (2009).

Neuber, Matthias

American "New" Realism and Behaviorism: A Case for Interdisciplinary History of Ideas

Abstract: It is often claimed that behaviorism in psychology and logical empiricism in philosophy were closely connected to each other. This view is somewhat distorted, to say the least. For one thing, the relations of behaviorism to logical empiricism were much weaker than commonly supposed. Secondly, there was another philosophical current that, in contrast to logical empiricism, virtually influenced the development of the behaviorist school, namely, American "new" realism. Having emerged at the very beginning of the twentieth century, the new realism presented itself as a movement in the form of the manifesto "The Program and First Platform of Six Realists," published in the Journal of Philosophy, Psychology and Scientific Methods in 1910. Two years later, the same collective of authors published the nearly 500-page volume The New Realism: Coöperative Studies in Philosophy. Among the six new realists were two who particularly engaged in the field of psychology: Edwin B. Holt (1873–1946) and Ralph Barton Perry (1876–1957). Regarding Holt, it is interesting to note that he developed behaviorist ideas as early as 1908, that is, five years before the appearance of John B. Watson's pioneering "Psychology as the Behaviorist Views It." In his 1914 The Concept of Consciousness, Holt developed an approach to consciousness that implied "that consciousness or mind is not inside the skull nor secreted anywhere within the nervous system; but that all the objects that one perceives, including the so-called 'secondary qualities,' are 'out there' just where and as they seem to be" (1914: 181). Thus, just as with Watson, according to whom "there are no centrally initiated processes" (1913: 423), Holt argued in explicitly externalist terms. Moreover, he, again like Watson, rejected introspection as a method in psychology, claiming that "introspection reveals just enough to demonstrate its own inadequacy" (1914: 199). At the same time, however, his account differed markedly from Watson's reductive materialism in ascribing a fundamental role to purposes in the context of explaining (human and nonhuman) behavior. Perry, in his 1918 "Docility and Purposiveness," applied this conception to the issue of (human and nonhuman) learning. It was in the context of these developments that Edward C. Tolman (1886–1959) – the founding father of neobehaviorism – began his academic career. Between 1911 and 1915 he studied under Holt and Perry (among others) at Harvard, where he also received his PhD. His seminal Purposive Behavior in Animals and Men from 1932 builds to a considerable extent on Holt's and Perry's neorealist ideas. The main goal of my talk is to substantiate this claim and thereby take into focus a largely under-investigated chapter in the interdisciplinary history of ideas.

Nicholson, Daniel

Jordan vs. Schrödinger: The Forgotten Dispute that Decided the Fate of Determinism in Molecular Biology

Abstract: The spectacular rise of molecular biology is one of the most momentous events of twentieth-century science. One of the instigators of the molecular revolution was Max Delbrück, a theoretical physicist who decided to turn his attention to biology after listening to Niels Bohr deliver a lecture in 1932 in which he applied his principle of complementarity to biology. Inspired by Bohr, Delbrück became excited about the prospect of encountering a paradox that would demonstrate the limits of mechanicist approaches to biology, just as quantum theory had done in physics. In the end, however, no paradoxes were found. Instead, the molecular basis of life appeared to be amenable to reductionistic explanation. The mechanicist and explicitly Cartesian view of life spawned by molecular biology (articulated in such influential works as Jacques Monod's Chance and Necessity) was clearly deterministic, and it was squarely grounded on the old physics.This paper explores a part of this story which has so far received remarkably little attention. It concerns a dispute during the 1930s and 40s involving two other theoretical physicists that I claim was as significant to the eventual fate of molecular biology as Delbrück's famous contributions as leader of the 'Phage group'. This was a dispute over the role played by determinism on biological processes. On the one side, Pascual Jordan (like Delbrück, a student of Bohr's who had become interested in biology) argued that the Copenhagen interpretation of quantum mechanics had fundamental biological implications. Specifically, Jordan proposed that organisms serve as amplifiers of quantum effects, and he went to lay down the foundations of a new science that he called 'quantum biology'. One of the key contentions of this paper is that Jordan's speculations about how the indeterminacy of quantum effects shapes biological processes was the main incentive that drove another founder of quantum mechanics, Erwin Schrödinger, to write his very famous book What is Life? in 1944, which exerted a huge influence on the pioneers of molecular biology. Newly uncovered archival evidence suggests that Schrödinger was appalled by Jordan's attempts to smuggle indeterminacy into biology via quantum mechanics and he developed his idea of a deterministic aperiodic crystal (later identified as DNA) responsible for the order of organisms as a result. More generally, I argue that the mechanicist conception of life that Schrödinger articulated, and which molecular biology embraced, emerged from his desire to refute Jordan's argument of the relevance for biology of (the Copenhagen interpretation of) quantum mechanics. And Schrödinger was so successful that quantum effects have been deemed irrelevant for molecular biology ever since.Although Schrödinger won, and molecular biology came to be recognized as the paragon of a deterministic science, there are signs that quantum biology might be making a comeback after many years of ridicule and neglect. New empirical studies suggest that molecular processes like photosynthesis

may take place by means of quantum effects, such as tunnelling. Perhaps the biology of the future may end up being grounded on the insights of modern physics after all.

Nickel, Gregor

Ethics and Mathematics - Mutual Perspectives

Abstract: @page { size: 21cm 29.7cm; margin: 2cm } p { line-height: 115%; margin-bottom: 0.25cm; background: transparent } According to a common prejudice, mathematics as a pure structural science is completely independent of value questions and thus also of their ethical reflection. This demarcation thesis seems to be so self-evident that it is usually not even explicitly formulated. One of the few authors who take up the issue is Friedrich Kambartel. According to Kambartel, the strict demarcation is related to Weber's catchword of "value-free science" and its "scientific theoretical elaboration" (p. 491) in the philosophy of the Vienna Circle. However, it is precisely the mathematics of the 20th century with its formalistic impact that suggests a strict exclusion of questions of value from the realm of science. However, Kambartel rightly points out that the history of ideas knows at least phases of an intensive contact between ethics and mathematics. Kambartel himself finally refers to parallels in their respective forms of discourse.In keeping with the emphasis on pure mathematics in the 1970s, Kambartel hardly mentions the applications of mathematics. In the meantime, however, mathematics is essentially legitimized by reference to its applicability. The trend within the discipline corresponds to an increasing mathematization of science, technology and society. The far-reaching shaping of the living world, e.g., through the use of computers already represents a more direct form of mathematization. Here, among other things, the question arises as to which ‘decisions' are to be calculated by machines in the future, and who can subsequently assume responsibility for them. Hard to overestimate, but often overlooked, is the direct influence of mathematics on social and cultural conditions of modern societies. In particular, the increasing use of mathematical methods in the context of economics and especially in finance raises ethical questions.The self-observation of society, which is often perceived as ‘overcomplex‘ (partly due to implemented mathematics), is increasingly served by simple numbers as evaluation and orientation criteria. Key figures now also dominate (political) decisions in the education and science system, for example in various 'rankings' or the key figures of bibliometrics. The drastic reduction to a few, easily comparable numerical values trivializes difficult and possibly controversial evaluation questions and, at the same time, hides a discussion about the quality of the evaluation standards in extreme cases. If the situation outlined above corresponds even roughly to social reality, dramatic threats to the ability to influence and control, and thus to act responsibly, follow both at the individual and at the collective level. The situation becomes even worse when descriptive and normative functions of mathematics are confused.Since there is hardly any established discourse framework for an ethics (in) mathematics, the lecture will first present a brief sketch of the general framework, leaving out the historical dimension to a large extent. Subsequently, first thoughts on such a subject ethics will be presented, focusing on the role of the ‘specific mathematical' in the questions outlined above.

Ohnesorge, Miguel

Why Peirce Became a Frequentist

Abstract: I defend a new interpretation of the content and origins of Charles Sanders Peirce's frequentist philosophy of statistical inference. On that interpretation, his frequentism is not primarily a view about the nature of probability, but – in good pragmatist fashion – a view about the two-fold role that probabilistic reasoning should play in scientific practice. First, probability is only meaningful when applied in conjunction with models of stochastic processes (first and foremost, models of physical measurement devices). Second, such models do not serve to license inferences to the most likely output in a given situation, but to distinguish systematic errors from expected variations in the outputs of these processes. In this manner, statistical inference guides the identification of and learning from systematic errors, facilitating a long-run convergence of our beliefs towards the truth. This reading makes Peirce's philosophy of statistical inference continuous with (i) his general philosophy of scientific inquiry and (ii) his scientific work in physical geodesy (the science of measuring the Earth's shape and gravity field).I justify this interpretation on historical grounds, by reconstructing why Peirce became interested in statistical inference and when he developed his mature philosophical position. Peirce first studied the foundations of statistical inference in a paper on measurement errors, published in his capacity as assistant at the US Coast and Geodetic Survey. His distinctive branch of frequentism, however, only took shape after he visited the German lands in 1875, where he met Prussian geodesist Heinrich Bruns. Bruns, at the time, worked on a serious empirical problem at the heart of geodetic research: despite increasing precision and large quantities of data, the two major measures of the Earth's shape disagreed systematically. This circumstance did not prevent many geodesists from accepting their mean value as the Earth's true ellipticity, effectively treating systematic errors as randomly distributed. Bruns, among others, critiqued this practice in an influential book, arguing that statistics serves not to control errors but to guide our learning about their causes. Peirce's archived papers and correspondence illustrate that he not only read and acquired Bruns's work but became one of its most vocal advocates in geodetic circles.

Oliva, Luca

The A Priori in Logical Empiricism

Abstract: Among other factors, logical empiricism builds its identity by rejecting the fundamental claims of Kant's epistemology. A shared, although heterogeneous, criticism of his central notion of a priori characterizes the movement from the outset. In contrast to Kant (Parrini 2002), logical-empiricists hold that(a) the meaning of a statement is constituted by its method of empirical verification, where evidence obtained through experimentation proves or disproves a hypothesis; (b) all logical-mathematical truths have a tautological nature, including the a priori propositions (Russell 1897; Wittgenstein 1922), and their apodicticity require no synthesis a priori; (c) every transcendental epistemology shows signs of anthropological or psychologistic contamination (Schlick 1917). Nevertheless, specific properties of Kant's a priori, such as its constitutive function, are relativized but retained (Friedman 2001, 2007, 2009; Parrini 1998; De Boer 2010). The notion has two meanings (Friedman 2007): 'necessary and unrevisable, true for all time' and 'constitutive of the concept of the object of [scientific] knowledge.' The distinction is prefigured in Kant (Friedman 2001). Accordingly, logical-empiricists transform instead of abandoning the notion a priori.However, I argue that this evolution of the a priori in logical empiricism hides tensions and remains problematic. Readings range from a weak to a strong rejection of Kant and lack consistency. Therefore, I analyze the notions of coordinative principles (Reichenbach 1920), implicit definitions (Schlick 1918), and L-rules (Carnap 1928), which revise the a priori according to the new physics (Lorentz, Einstein) emerging from the discovery of non-Euclidean spaces (Bolyai-Lobachevsky, Riemann, Minkowski).1. The early Reichenbach (1920/1965) argues for principles coordinating cognitively dependent but metaphysically separate levels of conceptualization, the theoretical and the empirical. He sees these principles as "equivalent to Kant's synthetic a priori judgments" (1965:48), a notion he "aimed to transform rather than abolish" (De Boer 2010:508). So, De Boer argues that the differences with Kant have been overestimated by logical empiricists and their readers, including Friedman. Later, Reichenbach (1924, 1928), adjusting to Schlick's criticism, dropped the apriority's constitutive feature.2. Dissimilarly, Schlick (1918/1974) follows Poincaré and argues for the conventional nature of the a priori by replacing the Kantian notion, based on sensible intuition, with that of implicit definition, primarily justified by mathematical developments (Popper1959:§17). For Schlick, modern geometrical axioms avoid visual thinking and rely on conventional definitions stating analytic relations. He refers to Hilbert (1902), whose solution for apriority is "simply to stipulate that the basic or primitive concepts are to be defined just by the fact that they satisfy the axioms" (Ibid).3. Finally, within any physical language, Carnap separates logical from physical principles, L-rules (i.e., analytic sentences) from P-rules (i.e., synthetic sentences), all revisable and subject to pragmatic and Duhemian holistic considerations (Duhem 1906). "Carnap's L-rules or analytic sentences," says Friedman, "can be profitably viewed as a precise explication of Reichenbach's notion of the constitutive or relativized a priori" (1999:69) but differ from

Schlick's implicit definitions.Although these logical-empiricist variations on the a priori share the purpose of revising Kant, they remain inconsistent and ultimately irreducible.

Oliveri, Lucia
Davide, Dalla Rosa

Hypotheses as Particular Kind of Doxastic Attitude for Kant

Abstract: Kant defines hypotheses as "the opinion of the truth of a ground from the sufficiency of said ground for its consequences" (R 2678, AA 16 465), and insists on the indispensability of hypotheses in empirical sciences (Lo-Blomberg AA 24.1 222-3). Good hypotheses are not just the starting point for enquiring whether something is the case, eventually resulting in knowledge; the entire method of inquiring – from where to begin in collecting empirical data to constructing experiments – depends on what is assumed as a hypothesis. Still, hypotheses may be confused with other forms of assuming without investigation, like prejudices and axioms.The aim of the paper is to reconstruct a Kantian account of hypotheses as proper forms of doxastic attitudes, by contrasting hypotheses to other forms of assumptions without investigation listed by Kant, thereby locating them within Kant's modes of holding-to-be-true (opinion, belief, knowledge) as a peculiar form of opinions (Meinungen). We argue that, while other forms of assumption are not apt to arouse in the subject an inquisitive disposition towards the phenomena at stake, hypotheses for Kant correspond to a particular form of entertaining the content of a judgement, that motivates to ask questions in order to find an explanatory ground for some phenomena. We use Palmira (2020), in which hypotheses are singled out as doxastic attitudes, as a framework to show the epistemic value of Kant's account both in a historical and systematic way. Kant develops his account in a period of general mistrust of hypotheses in scientific fields because of their misuse in metaphysics. On the contrary, Kant's account implies that hypotheses are necessary for every form of inquiry and scientific discovery were impossible without them. On this understanding, hypotheses cannot be forms of Kantian beliefs, while they are forms of opinions. Through this reconstruction of Kant's understanding of hypotheses, one can shed new light on the Doctrine of Method, the part of the Critique of Pure Reason where he addresses the problem of hypotheses in science and metaphysics. Kant's account can allow the use of hypotheses in sciences but bans them from metaphysics.Selected ReferencesButts, R. (1961). Hypothesis and explanation in Kant's philosophy of science. Archiv für Geschichte der Philosophie, 43(2), 153–170.Butts, R. (1962). Kant on hypotheses in the "doctrine of method" and the Logik. Archiv für Geschichte der Philosophie, 44(2), 185–203.Capozzi, M. (2019). Le ipotesi secondo Kant: Requisiti, giustificazioni, stato epistemico e euristica. Syzetesis VI (1), 153-89. Cooper, A. (2023). Hypotheses in Kant's philosophy of science. Studies in the History and Philosophy of Science 99, 97-105.Demarest, B., & van den Berg, H. (2022). Kant's theory of scientific hypotheses in its historical context. Studies in History and Philosophy of Science 92, 12-19. doi: 10.1016/j.shpsa.2022.01.011.Kant, I. (1900-2022). Gesammelte Schriften. Hsg. von der Königlich Preußische Akademie der Wissenschaften. De Gruyter (= AA)Palmira, M. (2020) Inquiry and the doxastic attitudes. Synthese 197, 4947–4973.

Omodeo, Pietro Daniel

Historical and Political Perspectives on Science in the Leftist 1970s: A Reappraisal

Abstract: In the aftermath of the cultural and political struggles of the 1968 movement, news perspectives on science emerged. These marked a leftist phase in the critical meta-reflection of the 1970s on science and its relation to society and history. Here, some denunciation of the ideological abuses of science and technocratic policies became widespread, as exemplified for instance by Habermas in his Marcusean works, Technik und Wissenschaft als 'Ideologie' and Erkenntnis und Interesse, both published in 1968, However, today, these critiques are often coopted in post-modern and populist agendas that obliterate the leftist legacy of approaches once fostering an emancipatory development of science for just and communitarian societies. The political, historical and philosophical potential of works such as those by Hilary Rose and Steven Rose (Ideology of/in the Natural Sciences, 1976, which connected economy struggles, anti-capitalist ideology criticism, feminist standpoints and environmentalist concerns) has been diluted in recent decades of neoliberal hegemony and distorted by de-historicizing and de-subjectivizing approaches, including in STS. Moreover, historical-political perspectives of the 1970s that are linked to non-Anglophone areas have been too often neglected. In this communication I will reassess leftist perspectives on science in the 1970s with special consideration of books that now appear in English translation for the first time: Ciccotti, Cini, De Maria, and Jona-Lasinio's L'Ape e l'architetto (Milan, 1977) and Wolfgang Lefèvre's Naturtheorie und Produktionsweise (Darmstadt, 1978). What is still alive of the historical-political epistemologies of the 1970s in the present conjuncture, marked by a deep crisis of neoliberal hegemony and an environmental turn in culture and politics?

Onnis, Erica

Emergent causal efficacy: from the British Emergentists to the contemporary debate

Abstract: In the last few decades, the notion of emergence has become increasingly relevant for the description of several phenomena. Defining its meaning, however, has generated extensive debates because of the various features that emergent entities exhibit when observed in different contexts.Despite the various models offered by the literature, most authors agree on a basic distinction between (i) strong or ontological cases of emergence and (ii) weak, epistemic or "metaphysically innocent" ones. The latter exhibit deductive or theoretical unpredictability; the former often involve novel causal powers and this is what allows strongly emergent phenomena to be admitted in our ontologies as irreducible and autonomous entities.This distinction rests upon a platonic metaphysical assumption called the "Eleatic principle". It states that everything that really is has to possess some capacity. In the Nineties, the principle was introduced in the contemporary philosophical debate about emergence by Kim, who attributed it to the British Emergentist Samuel Alexander. But what is worth noticing is that Kim – and the same can be said about McLaughlin, who wrote about C. D. Broad – presented causal efficacy in terms of the exercise of causal powers. This circumstance produced subsequent attention to the concept of emergent causal powers, which, as mentioned, became the mark of ontological or strong emergence for many authors.Despite what was suggested by Kim and McLaughlin, however, British Emergentists were not committed to a power-based view of causation. For Alexander, causality corresponded to the relation of continuity and succession existing between different Space-Time points. In Space, Time and Deity, Alexander repeatedly states his aversion to the concept of causal power, which, in his view (as also for Hume), is not empirically graspable, and should be recognised as "a fiction" (1920: 188). The same can be said about Broad, who states that causality is a matter of regularity, uniformity, and continuity between spatiotemporal regions (1925: 454-456).It thus seems that the authors who reintroduced the concept of emergence into the philosophical debate about emergence in the 1990s did so by attributing to the early emergentists contemporary metaphysical positions that the latter did not share. From a historical perspective, ascribing to Broad, Alexander and British emergentism in general the thesis according to which emergent causal efficacy – and the ontological autonomy of emergent phenomena – would depend on the exercise of causal powers seems therefore illegitimate. The association of emergentism with the ontology of powers follows a different, more articulated, and recent path, namely that corresponding to the recovery of the notion of emergence to contrast it with contemporary (micro)physicalism. This is a "battle", however, that is played in the field of the latter and draws on its conceptual repertoire, referring to issues such as realization, dispositionalism, causal inheritance, and so on.Reading – or re-reading – the emergentist debate in this contemporary key is not necessarily wrong, but it is important to recognise that doing so is

not the only possible way to revive the emergentist worldview, nor is it a metaphysically neutral approach.

Pascual Alba, Joseba

E Pluribus Unum Or Divide et Impera? Styles of (Dis)Unities of the Sciences

Abstract: The main aim of this paper is to assess Ian Hacking's notion of "styles" and its role in the debates about the unity and plurality (or disunity) of the sciences. And, viceversa, to explore how these debates can be better clarified by focusing on the notion of "styles". "Styles of reasoning" or "styles of scientific thinking and doing" (Hacking 1982, 2012), are "plural" by their definition. They imply what can be called a 'stylistic plurality'; further emphasized by the methodological, historical and ontological levels of "disunity" in the sciences they elicit. However, at the same time, for Hacking (1996) this stylistic plurality (or "disunity") somehow allows the sciences to be "grouped together" and to be distinguished from other kinds of human activities. That is, the sciences are unified not despite, but precisely because of their disunite character. In this view, styles may also somehow allow a kind of 'unity of the sciences'. Thus, a key element to investigate is also what kind of unity (or unities) they introduce, taking Hacking's (1996) dual or plural concept of "unity". In the same vein, Ruphy has conceived of another kind of 'stylistic unity' through plurality that she called "foliated pluralism". Ruphy (2011, 2016) starts from the empirical acknowledgement of plurality as a matter of fact: styles co-exist. In her view, this fact is key to understanding the relations among styles. Consequently, she identifies four main features of styles : "transdisciplinarity", "synchronicity", "nonexclusiveness" and "cumulativeness". Following both philosophers, I will try to pin down styles so as to clarify in what senses they can be seen as producing a unified or diversified view of scientific knowledge and practices. Using the 'styles-project' as a framework, I will also try to show how the relations between unity and plurality of the sciences may require a philosophical re-conceptualisation.

Patton, Lydia

The Present and Future of the HOPOS Journal

Abstract: A presentation on the state of the HOPOS journal.

Pearce, Gareth
Maximilian, Noichl

Mapping the Practical Turn: A Digital Pre-History of the Philosophy of Mathematical Practice

Abstract: In his edited volume, Mancosu (2008) outlined what amounted to a manifesto for the emerging philosophy of mathematical practice (PoMP). In this manifesto, Mancosu outlined a (purported) history of the practical turn in the philosophy of mathematics. For the last few decades, philosophy of mathematics had strayed from the path, becoming overly concerned with overly philosophical questions about realism, abstraction, indispensability and the Benacerraf problems. The philosophy of mathematical practice had been sidelined, limited only to the likes of Lakatos (1976) or Kitcher (1983). Since then, a new generation of philosophers of mathematical practice have established the discipline convincingly within the philosophy of mathematics as a whole.But how accurate is Mancosu's account of the history of PoMP? Are there early philosophers of mathematical practice forgotten or missing from Mancosu's narrative? How would one go about establishing this claim, one way or the other?In this contribution, we apply unsupervised machine-learning techniques to over 5000 philosophy of mathematics papers written in English between 1950 and the present day to attempt to answer these questions.As there are only few journals dedicated specifically to the philosophy of math, the majority of the philosophy of mathematics over the time period has been published in mixed journals. The first challenge for a digital analysis of the philosophy of mathematics is therefore to identify and isolate the corpus in question. We begin by identifying a spine of nearly one thousand known PoM papers from across the timeframe, which we consider an incomplete sample of the field, that contains examples from most approaches. This sample includes the whole journal philosophia mathematica, as well as manually selected examples from the journal Synthese., Using a classification method known as Positive-Unclassified (PU) Learning (Liu, et al 2002), we expand our sample to include 5000 PoM papers from dozens of journals. This is, if not a near-complete list of all English PoM papers within the period, at least a highly representative approximation.Using a philosophy-fine-tuned BERT model (Noichl &Panzer 2023) to represent the texts in a format amenable to computational analysis, we identify clusters of related articles in the history of PoMP, an analysis that we supplement with a qualitative investigation into individual articles. Although we use different computational techniques, this approach is comparable to that of Malaterre, Chartier and Pulizzotto (2019).Our preliminary findings challenge the established narrative by highlighting overlooked contributors and themes that date back further than commonly acknowledged.

Pearce, Trevor

Alain Locke and the Brentano School

Abstract: Alain LeRoy Locke (1885–1954) was an African American philosopher who is now remembered primarily as a cultural pluralist and as the father of the Harlem Renaissance. But Locke's graduate work and his only foray into professional philosophy were both focused not on culture but rather on general value theory. The goal of this paper is to return Locke's general value theory-his account of value classification and value psychology-to its original context: namely, the late nineteenth and early twentieth-century tradition of "scientific philosophy" familiar to HOPOS scholars.Although it has been noted that Locke's work engaged closely with Austrian value theory, especially that of Christian von Ehrenfels, scholars have failed to situate his approach within the broader "Brentano School," which had as its unifying feature the claim that philosophy should adopt the rigorous methods of science. Members of this school joined positivists and Neo-Kantians in contributing regularly to the Vierteljahrschrift für wissenschaftliche Philosophie (Scientific Philosophy Quarterly), which declared in its opening issue that to make itself scientific, philosophy must be the pinnacle rather than the foundation of the empirical sciences.Locke's early work was directly connected to this tradition of scientific philosophy. Ehrenfels's first writings on value theory were published in the Vierteljahrschrift, which was by then co-edited by one of Locke's teachers in Berlin, Alois Riehl. Although work at the intersection of psychology and philosophy was not valued at Oxford, where Locke was a Rhodes Scholar from 1907 to 1909, it was a different story in Berlin, where he studied in 1910–11. Carl Stumpf, Brentano's most famous student, had become Rector there only a few years before Locke's arrival, and had declared in his inaugural address that the best philosophy was Erfahrungsphilosophie, which "grows out of the individual sciences and tries to retain a tight connection with them," speaking their language and following their methods.After providing some background on scientific philosophy and positioning Ehrenfels's System der Werttheorie (1897–98) as a product of the Brentano School, I will interpret Locke's Harvard dissertation of 1917 as a work within that same tradition. Taking Ehrenfels and American philosopher Wilbur Marshall Urban as his main interlocutors, Locke described his approach as seeking "a morphology of values" by employing methods akin to those of the natural sciences.

Phillips, William

Spinoza's Critique of Applied Analytic Geometry and the Representational Relationship Between Explanatory Tools and their Explananda

Abstract: During the early modern period, many thought extended substance was the metaphysical ground for the existence of physical reality. In this presentation, I argue that Spinoza's theory of the imagination and account of quantity puts pressure on the view that we can infer properties of extension from properties of analytic geometric structures. Afterwards, I show that this implies that some common-sense properties of the physical world are actually imaginary in virtue of being unique to analytic geometry. Following this, I explore what information is lost when thinking about extension analytically. I sketch a generalization of Spinoza's critique of analytic geometry which entails that some equivalent mathematical structures can be intrinsically unequal in explanatory power. For example, on this view, algebraic structures are less accurate than equivalent geometric structures in explaining properties of extension.The reason Spinoza thinks that we cannot infer properties of extended substance from properties of analytic geometric structures is because analytic geometric structures are mental tools forged to explain extended substance but not represent extended substance. Puzzlingly, Spinoza also argues that these mental tools are evolutionarily adapted to explain their explananda. While not implausible, it's not obvious to me how a mental tool can be naturally adapted to explain a particular explanandum but not represent any of that explanandum's properties. For guidance on understanding what mental tools tell us about their objects, I look toward prehistoric archaeology's methods for inferring facts about the world from facts about the physical tools crafted to navigate it.

Pignatelli, Giuseppe

Res per se notæ: Descartes' theory of definition and Arnauld's epistemology of fundamental mathematical propositions

Abstract: The paper aims to assess Descartes' views on the indefinability of self-evident knowledge and to explore how these relate to Arnauld's conception of the epistemic properties of the fundamental propositions of mathematics. Over the past years, scholars have suggested that Arnauld advocated a form of intuitionism in mathematical epistemology that drew mainly on Pascal's activity as a mathematician but also Descartes' epistemology of "clear and distinct ideas" (Itard 1939-1940; Gardies 1984, De Risi 2016, Molina 2017). However, a thorough analysis of the bearing of this Cartesian background on Arnauld's philosophy of mathematics is still lacking. The paper provides new insight into this issue by examining two different sets of primary sources. Firstly, the texts most relevant to understanding Arnauld's interest in Descartes' epistemology of self-evidence are analyzed, namely Descartes' posthumous Regulæ ad directionem ingenii, a manuscript copy of which Arnauld had the chance to read before their first publication in Latin in 1701, Arnauld's Objectiones Quartæ to Descartes Meditationes metaphysicæ (1641) and the four letters that the two exchanged in 1648. Secondly, the focus shifts to Arnauld's methodological and mathematical writings, namely the Quatréme Partie of La logique ou l'art de penser (1662) and the Nouveaux éléments de géométrie (1667). The analysis will shed light on two interesting aspects of Arnauld's reception of Descartes' views on definitions by focusing on a case study, namely Arnauld's innovative treatment of perpendicular lines in the Livre Cinquième of the Nouveaux éléments. On the one hand, Arnauld takes up Descartes' view that definitions explain complex cognitions in terms of simple and self-evident ones and that, for this reason, any attempt to explain what already is self-evident results in obscurity and sterile philosophical quarreling (AT V, 222, 15-20; AT X, 426, 22-25; 433, 14-23). On closer inspection, Arnauld applies this methodological precept precisely in his redefinition of perpendicular lines in terms of the equality of the distances between points taken on the intersecting lines rather than on the equality of the adjacent angles formed by their intersection. In this sense, Arnauld observes that "using triangles to demonstrate the properties of lines" is the same as "using the most compound to explain the simplest" (1667, bk. V, 88). On the other hand, Arnauld also loosens up Descartes' strict conception of the non-explicability of self-evident cognitions. The introduction of a new definition for perpendicular lines is followed by (and grounded in) a specific axiom, which Arnauld extensively explains. This practice recalls one of the two 'rules' for axioms that Arnauld devised in the Quatréme Partie of the Logique. There Arnauld argued that some mathematical axioms, although non-demonstrable qua axioms (i.e., self-evident propositions), need to be explained precisely for the sake of making them easier to understand (1662, pt. IV, chap. V, 329-330). As the paper concludes, although Arnauld's interest in Descartes' epistemology of self-evidence appears unmistakable, his projects and

objectives in the foundations of mathematics shaped his unique understanding of such Cartesian background.

Pillon, Perceval

Opposing methodologies in deontic logic, a historical perspective

Abstract: Deontic logic is the formal study of a whole array of concepts having to do with normative reasoning, obligation being the first of them. Even though there has been some works done on those questions, we can trace the birth of modern deontic logic to the seminal article of von Wright from 1951 (von wright 51). The article, amongst other things, builds on the analogy between obligation and necessity as modalities. This analogy will have a structuring effect on the field as it will allow people to use the development of general modal logic and its semantics (one thinks in particular of Kripke (Kripke 59)) as a stepping stone for deontic logic. This is to say that the formal tools built for general modal logic will be the ones used for deontic logic. One has to remember nevertheless that those modal logics are mostly thought for alethics modalities.We can thus see this way of building formal systems as an adaptation of preexisting formal tools. Opposite to that, one can find authors that defend the idea that a formal system should come from an analysis of the concepts one wants to formalise leading to the creation of ad hoc systems. We want to present this opposition within the historical context of deontic logic focusing on two authors : Georg Henrik von Wright and Georges Kalinowski. My main focus would be to show how the contrasting methodologies lead not only to different formal systems - which would be expected. But we also want to show that those systems actually formalise different things. For example we can use a distinction used by Kalinowski dividing the case at hand between a logic of normative propositions and a logic of norms, one being descriptive, the other prescriptive. For the sake of this paper the discussion would be constrained to those two authors, but we want to defend the argument that this distinction shaped a lot of what deontic logic has come to be. Moreover we will show this distinction can also be made for a whole range of projects of formalisation of concepts. Our presentation would first present a brief history of deontic logic and the place occupied by both authors, then would analyse the philosophical and then methodological distinctions made by them and how they are transcribed into their formal systems. We would then draw a more general conclusion on the influence that such differences have on how one considers and uses logical systems as philosophical tools.Bibliography : Hilpinen, R., & McNamarra, P. (2013). Deontic Logic a Historical survey and Introduction.Kalinowski, G. (1972). La logique des normes. Presses Universitaires de France.Kripke, S. A. (1959). A Completeness Theorem in Modal Logic. Journal of Symbolic Logic, 24(1), 1–14. von Wright, G. H. (1951). Deontic Logic. Mind, 60(237), 1–15.

Pisano, Francesco

“This is Socrates”. Realism in the 19th-Century Debate on Logic

Abstract: One distinctive aspect of the Anglo-German debate in the late 19th century is a logic that combines Kantian issues with Aristotelian solutions. This talk aims to elucidate some implications of this junction, whose core is the adoption of the Aristotelian axiom that a concept must be the concept either of a real individual or of a predicate pertinent to at least one real individual. This realist assumption aims to heal the rift between 'thought' and 'being' in Kantian logic. Its centrality in the Kant-Aristotle convergence becomes evident once one clarifies its consequences for the historical shift that post-Kantian logic has undergone from being a pure doctrine of intellect to a technology (Kunstlehre) for constructing propositions. The first part of the talk sketches out the Kantian premise that formal logic reached completion with Aristotle and thus cannot evolve further. On the one hand, formal logic is a doctrine of pure thought; on the other, its formality is inseparable from the transcendental actualization of the intellect. This ambiguity fuels the subsequent debate between Hegel's idealism and Herbart's realism. This debate poses a choice between two senses of logic: as a science and as a constructive technique. The related "logical question" was first posed within Trendelenburg's effort "to solve the contrast between Thinking and Being" (Trendelenburg 1840, I, 106) by reintroducing Aristotle into the logical debate in order to object to the detachment of Kantian logic from real 'being'. Kant's 'empty' logic should be integrated with an Aristotelian theory of movement as a bridge between thought and being. Consequently, logic should be based on the dynamic and causal interactions between the mind and the world. This is discussed in the second part.The third part deals with the constructive interpretation of Trendelenburg's realist logic proposed by Christoph Sigwart, the author of the reference text (Sigwart 1873-1876) for German logic studies in the late 19th century and a forerunner of German scientific philosophy. Sigwart's idea that logic should begin not with concepts but with the construction of indexical judgments such as "This is Socrates", is considered as a case study. This idea implies an eversion of thought toward its actualization in contextualized assertions and operations. The resulting psychologistic logic claims to be a realist reinterpretation of Kant's logic. It is realist since it is a naturalized technology, instrumental to the optimization of actual acts of thinking within scientific inquiry, and it is formal since it is a priori constructive of the objects of knowledge while being itself a construction.The talk ends by showing that this logical framework has significant implications for the subsequent early developments of the formal turn in philosophy up to Carnap.Essential Bibliography:Käufer, S. (2005). Hegel to Frege: Concepts and Conceptual Content in Nineteenth-Century Logic, «History of Philosophy Quarterly», 22/3, pp. 259–280.Lapointe, S. (ed., 2019). Logic from Kant to Russell, Routledge.Sigwart, Ch. (1873-1876). Logik (2 vols.), Laupp.Trendelenburg, F. A. (1840). Logische Untersuchungen (2 vols.), Bethge.Vilkko, r. (2002). A Hundred Years of Logical Investigations, Mentis.

Polak, Paweł

Philosophizing engineers at the roots of modern philosophy of science in Poland: the case of Bronisław Biegeleisen (1881-1963)

Abstract: At the beginning of the 20th century, engineers began to play an important role in Polish society and became an important part of the Polish intelligentsia. This was connected with the difficult political situation (the Polish state did not exist and was divided among neighboring countries) and attempts to modernize Polish society. A special role was played by the scientific center of Lwów (now Lviv), where the only Polish polytechnic was active. The community of Polish engineers had a great interest in science and the philosophy of science. This specific intellectual milieu developed in the shadow of the famous school of analytical philosophy of Kazimierz Twardowski (today known as the Lvov-Warsaw School)(Chybińska et al., 2016; Brożek, Stadler and Woleński, 2017; Woleński, 2019). The philosophical involvement of engineers was clearly evident during the polemics surrounding the reception of the theory of relativity in the early 1920s (Polak, 2012; 2016).The most interesting figure in this milieu was the engineer Bronisław Biegeleisen (Polak, 2013). Biegeleisen was initially interested in the philosophy of mechanics, but under the influence of inspiration coming from Twardowski's circle, he took up philosophy of science directly. His work is an important contribution to the development of Polish philosophy of science. However, this contribution has not been analyzed in detail (only brief mentions are available), and the question of the milieu he represented has generally been ignored.Therefore, the purpose of this article is to present Biegeleisen's achievements for the development of philosophy of science in Poland. The second purpose is to make a pioneering analysis of the role of engineering circles in the development of philosophy of science in Poland, using Biegeleisen as an example.ReferencesBrożek, A., Stadler, F. and Woleński, J. eds., 2017. The Significance of the Lvov-Warsaw School in the European Culture. Vienna Circle Institute Yearbook. [online] Cham: Springer International Publishing. https://doi.org/10.1007/978-3-319-52869-4.Chybińska, A., Jadacki, J.J., Woleński, J. and Brożek, A. eds., 2016. Tradition of the Lvov-Warsaw School: Ideas and Continuations. Polish Analytical Philosophy. Leiden; Boston: Brill-Rodopi.Polak, P., 2012. 'Byłem Pana przeciwnikiem [profesorze Einstein]...': relatywistyczna rewolucja naukowa z perspektywy środowiska naukowo-filozoficznego przedwojennego Lwowa. Kraków: Copernicus Center Press.Polak, P., 2013. Bronisław Biegeleisen - zapomniany lwowski inżynier i filozof. In: M. Przeniosło and L. Michalska-Bracha, eds. Znani i nieznani dziewiętnastowiecznego Lwowa. Wydawnictwo Uniwersytetu Jana Kochanowskiego w Kielcach. pp.123–131.Polak, P., 2016. Philosophy in science – a case study of the reception of the Special and the General Theory of Relativity in Kraków and Lwów before 1925. Studia Historiae Scientiarum, [online] 15, pp.245–273. http://dx.doi.org/10.4467/23921749SHS.16.010.6153.Woleński, J., 2019. Lvov-Warsaw School. In: E.N. Zalta, ed. The Stanford Encyclopedia of Philosophy, Spring 2019. [online] Metaphysics Research Lab, Stanford University. Available at: [Accessed 6 May 2019].

Poliseli, Luana

The kaleidoscope of aesthetics in science: lessons from the past, thinking in the future

Abstract: Aesthetic responses serve as the basis for an aesthetic judgment where one can ascribe value to something (Brady 2003). Scientific theories, models, and experiments are uncontroversially often subject to aesthetic assessment (Elgin 2020). For instance, visualizations and representations are two ways in which aesthetic values can contribute in and to science. These practices reflect that choices and evaluation of theories can also be depicted by aesthetic values such as beauty, symmetry, systematicity, elegance, and so on (Ivanova & French 2020). They can have, therefore, several epistemic advantages in science (Stuart 2018) such as empirical adequacy, heuristics in the underdetermination of a theory, relations to truth, and so on (Ivanova & French 2020). This strengthens the idea that aesthetic experiences and values can serve as conditions for understanding (Kosso 2002), and can therefore play an epistemic role in science by ading understanding (Breintebach 2013). Even though aesthetic experiences are (slowly) but increasingly gaining attention in philosophy of science, traditionally, the appreciation of beauty in science has long been associated to the success of a theory or an experiment (McAllister 1996; Ivanova & Murphy 2023). In this talk I will convey that this is a simplification and does not entirely represents the cornucopia of aesthetics values and experiences in science. To do so, this work draws lessons from the history of philosophy of science to communicate the existence of distinct aesthetic engagements (or dimensions) in and of science, they are: emotional, methodological, and artistic. It is expected that by looking into the past we can put aesthetic engagements in the spotlight of contemporary philosophy of science, and if acknowledged as powerful instruments of science, it can formally become part of scientists' epistemic toolboxes enhancing scientists' future performances.

Potters, Jan

Questioning the Quantum: On Thomas Kuhn's Interviews with Quantum Physicists

Abstract: In her 1999 book Quantum Dialogue, Mara Beller claims that philosophers, in their accounts of scientific change, have focused too much on consensus, and not enough on the role played by doubt and disagreement. On the basis of a study of the history of quantum mechanics, she argues that science is primarily to be seen as a dialogical activity: "from the dialogical perspective, it is 'creative disagreement' – with oneself (doubt) or with others (lack of consensus) – that plays the crucial role in the advance of knowledge" (1999, p. 3). Her primary philosophical target is Thomas Kuhn, whose notion of a paradigm "[b]y definition [...] excludes any diversity of opinion or open-minded discussion. It legitimates dogmatism and silences the opposition." (1999, p. 286).In this talk, I will argue that Beller does not do justice to Kuhn's views. My starting point for this will be a lecture by David Bohm (1974), in which Bohm criticized Kuhn's Structure in a way very similar to Beller, namely that it did not leave room for disagreements regarding the interpretation of quantum mechanics. In response to this, Kuhn argued that his interviews with quantum physicists had shown that such discussions had been part of the quantum paradigm, but only for a few years and primarily only in one particular place, Bohr's institute in Copenhagen. Physicists at other places and at other times did not really concern themselves with such discussions, which Kuhn took to show that, later on, they had become philosophical rather than physical in nature. I will argue, in response to Beller, that this suggests that Kuhn's paradigms did leave room for productive disagreements, albeit in historically situated way.I will then use this reply by Kuhn to elaborate two claims: first, that his interviews with quantum physicists between 1962 and 1964 brought him to refine and rethink specific elements of Structure, in particular the situatedness of paradigms (in textbooks, institutes, etc.); and second, that these interviews also brought him conceptualize more explicitly the relationship between science, history and philosophy in terms of historically situated disciplines. I will then illustrate the influence of the interview project on these topics by connecting them to his later work, in particular The Second Tension and his 1978 book on the early quantum. If time permits, I will also use these findings to respond to recent claims (following Bird 2002) that in his later work, Kuhn made a turn from a more naturalistically inspired philosophy towards more a priori reflection. I will argue, more specifically, that Kuhn did not turn away from empirically-informed approaches, but rather that he became more reflective about the limitations of integrating history and philosophy of science.Sources:Beller, Mara (1999). Quantum Dialogue. University of Chicago Press.Bird, Alexander (2002). 'Kuhn's wrong turning'. Studies in History and Philosophy of Science 33: 443-463.Bohm, David (1974). 'Science as Perception-Communication'. In Suppe, Frederick (ed.), The Structure of Scientific Theories.

Pottin, Ange

Simondon's theory of "concretization" as an idealized take on technological change

Abstract: Simondon's theory of technological 'concretization', as first expressed in Le mode d'existence des objets techniques, aims to explain the evolution of technical objects as proceeding from strictly technological constraints. These constraints include internal coherence (« résonance »), a stable relationship with the associated milieu, openness for the user and for the technician, or thriftiness in the use of materials (the list is open-ended). Simondon thus provides a theory of technological evolution that shows its irreducibility both to science – the object exceeds scientific knowledge – and to other historical factors, such as social demand or economic drivers – that are explicitly rendered secondary in Simondon's analysis. So, although Simondon departs from Bachelard by claiming the autonomy of technology from science, he ultimately provides a theory of technological evolution that is homologous to the Bachelardian take on the history of science – i.e., as a progress towards a norm, irreducible to other historical fields. In this presentation, I will argue that this theory can be understood as an idealized reconstruction of technological change, and I will question its fruitfulness for Science and Technology Studies – following, among others, (Feenberg, 2017). Through the construction of technological lineages, the Simondonian technologist, heir to the Bachelardian epistemologist, engages in reconstructions that have two goals, descriptive and prescriptive: (i) they highlight the specitifity of the 'obscure zone' of technicity that is too often veiled by our understanding of technology; (ii) they exemplify a series of norms that help one identify proper technological progress, as opposed to alienating evolutions. But I would argue that we must bear in mind that such reconstructions are idealized, as they abstract a technological core of technical evolution from the historical context in which it is embedded. Can the choice of pressurized water reactors for most of the world's nuclear fleet, for instance, be extracted from the commercial dominance of companies from the United States in the 1960s-1970s? Can these reactors be seen as representing a technological progress in terms of miniaturization and standardization compared to gas-cooled and boiling water reactors?References : Bachelard, Gaston. 1972. "L'Actualité de l'histoire des sciences." In L'engagement rationaliste, 137–52. Bibliothèque de philosophie contemporaine. Paris: Presses universitaires de France.Feenberg, Andrew. ''Concretizing Simondon and Constructivism: A Recursive Contribution to the Theory of Concretization'', Science, Technology and Human Values, 42 (1), 2017, pp. 62-85. Simondon, Gilbert. 2012 [1958]. Du mode d'existence des objets techniques, Paris, Aubier.---. 2014. Sur la technique, Pairs, Presses Universitaires de France.

Preston, John

‘A Computer-Program for Comparing Physical Theories à la Tractatus 6.341-6.343’

Abstract: In Tractatus Logico-Philosophicus 6.341, Wittgenstein suggests a metaphor or analogy for understanding how what he calls 'Newtonian mechanics' bears on the world, and for how different forms of mechanics can be compared with one another. He asks us to imagine a white surface with irregular black spots on it, a surface which is then covered by a fine square net. For each cell of this net, we can then say whether it is black or white. This, he tells us, counts as 'imposing a unified form on the description of the surface'. But he then points out that the form we have just imposed is optional, since one could go through the same process using either a triangular or an hexagonal net. And he suggests that the descriptions resulting from the applications of the different forms can be compared with one another with respect to their simplicity and their accuracy. I planned a computer graphics program that would instantiate the features of Wittgenstein's net-analogy. A student placement at my University, supervised by myself, offered a computer-science student ([anonymised]) a chance to design and construct such a program. My paper first relates how the program was designed, then describes and illustrates the resulting program, which allows different nets to be superimposed on a white surface covered in black spots, and then compares the results of each net superimposed. The program is simple, but it raises at least two sorts of questions. First, questions about exactly how to understand what Wittgenstein had in mind in the analogy (e.g., how complex can those 'spots' be?, what counts as a cell being black?). Second, and far more importantly, philosophical questions about what might be meant by the comparison-features involved in the analogy, viz., completeness, simplicity, and accuracy. While remaining vividly aware that the net analogy is only an analogy, the program (and future evolutions of it) can nevertheless be used to ask and suggest answers to such questions, so as to see whether Wittgenstein's net-analogy for Newtonian mechanics is substantial, or illuminating, or perhaps even both. It identifies issues which Wittgenstein doesn't (in his typically under-specified presentation) mention but which must be resolved if the theory-comparator is to work. In the presentation itself, I demonstrate the program.

Queijo Olano, Juan A.
Antonio Augusto Passos , Videira

Argentina, Brazil and Uruguay in the 1950s: relations between physics and philosophy

Abstract: We align with the philosophical outlook that emphasizes the important relationship between philosophical investigations and scientific progress. However, the proof backing this assertion has chiefly been derived from the European context, which has presented a somewhat oversimplified and limited depiction of the diverse manifestations of these connections across universities worldwide.In this presentation, we aim to provide a historical case study exploring the interrelation between scientists and philosophers in South America. We will be focusing on Argentina, Brazil, and Uruguay between 1940 and 1960. Our intention is to present a distinctive set of philosophical discourses and trends that emerged alongside national concerns about the status of science, mainly physics, and its promotion.More precisely, this study explores the rise of philosophical thought in South America, initiated by physicists aiming to restructure university systems, to nurture a professional scientific environment within their institutions. They actively participated in debates on national science education and advocated for national development through scientific pursuits. Notably, physicists such as Mario Bunge (1919-2020) in Argentina, Francisco X. Roser (1904-1967) in Brazil, and Félix Cernuschi (1907-1999) in Uruguay played pivotal roles in reshaping the philosophical traditions within their respective universities.A dynamic interaction between the practice of physics and the formulation of novel philosophical inquiries is observed in all three cases. This communication facilitated a noticeable change in the broader philosophical outlook in these nations. For instance, Roser's examination of topics linked to the sciences began in the 1930s but reached its zenith during a very fruitful period commencing in 1950, during which he created one of Brazil's most substantial Physics Institutes at the Pontifical Catholic University in Rio de Janeiro. Bunge, who attained his Ph. D. in physics int he early 1950s, focused the next twenty years on examining the foundations of quantum physics which eventually led to him becoming a renowned philosopher. In contrast, Cernuschi returned to Uruguay in 1950 after a distinguished scientific career in physics that included positions at various universities in Argentina, England, France, and the United States. He was appointed the Chair of Astronomy at the Universidad de la República. He developed a close association with Philipp Frank and became a prominent supporter of the Unity of Science movement in Uruguay.Together, these three figures played crucial strategic positions in their individual nations and in the scientific and philosophical circles of their time, as we will strive to clarify.

Reck, Erich

Ian Hacking and the Crystalizations of the Mathematical Reasoning Style

Abstract: Ian Hacking's notion of reasoning style, or style of scientific thinking, introduced a kind of pluralism into the philosophy of science. One of several reasoning styles identified by Hacking, following A.C. Crombie, is the mathematical style, with its postulational, proof-based character and its legendary origin in Thales' work on geometry (6th century BCE). More recently some philosophers of mathematics, including myself, have suggested that one can find not just one but a plurality of reasoning styles within mathematics, originating at distinct points in its history. Are these two perspectives incompatible, e.g., by being based on very different notions of reasoning style? In this talk I will try to reconcile them, at least in part, by focusing on Hacking's notion of the "crystallization" of a reasoning style. Hacking himself, e.g. in his later book Scientific Reason (2007), discusses two distinct crystalizations of the postulational mathematical style: besides Thales' geometrical work, later codified in Euclid's Elements, there is the legendary emergence of a more arithmetic, algorithmic approach, including the use of Arabic numerals, in Al-Khwarizmi (around 780-850 CE), picked up by European mathematicians in the 12th century. Building on this idea, I will suggest that one can distinguish several further, significantly different crystalizations later on, including the symbolic algebra associated with François Viète, characterized by its use of letters for "unknowns" (15th century), the Differential/Integral Calculus based on infinitesimals as introduced by Leibniz and Newton (16th century), and the set-theoretic, infinitary, and structuralist mathematics one can attribute to Richard Dedekind (19th century).

Rédei, Miklós

On the Notion of Categoricity in Axiomatic Approaches to Quantum Theory

Abstract: The talk articulates the claim that axiomatic approaches to quantum theory typically have the feature of intended non-categoricity. Intended non-categoricity is the property that the postulates in the axiomatization are intended to allow, as models, dierent, non-equivalent quantum theories that describe dierent quantum physical systems, and the non-equivalence of the theories is the consequence of presence of non-isomorphic mathematical structures for the same type of concept in the dierent theories. To make this explicit, the talk argues that the structure of an axiomatic formulation of quantum theory has three components: First, the group of concepts is formed in terms of which the quantum system is grasped and described. These concepts include: the observables O, the states S, the time evolution/dynamic D, the measurement M. Second, postulates are formulated that fix the mathematical representatives of the concepts by specifying the concrete mathematical categories from which one can take objects as representatives of concepts O, S , D, M to obtain a quantum theory. The third component relates elements in the mathematical models to physical reality through empirically testable propositions. The intended non-categoricity enters the axiomatization through the specication of the mathematical representatives of the physical concepts: The mathematical categories containing the objects representing the concepts come with their natural (iso)morphisms and typically there is more than one isomorphism-equivalence class of objects in those categories. This allows taking dierent, non-isomorphic mathematical objects from each of the categories from which representatives of O , S, D , M can be taken. This results in different, non-isomorphic instances of (O, S, D, M), to be regarded as mathematical models of different types of quantum systems. Furthermore, since O, S, D, M are conceptually different notions, it also can happen that fixing (up to isomorphism) a mathematicalrepresentative of an element in one of O, S , D, M still allows taking different, non-isomorphic objects from the category specied for other elements in O, S, D, M. Thus the intended (non-)categoricity has a fine-structure, and full categoricity of an axiomatization would require component-wise categoricity. But full categoricity is not aimed at in axiomatizations because this would lead to excluding the possibility of describing certain physical phenomena by theories satisfying the restricted set of axioms.This general pattern, and in particular intended non-categoricity, is illustrated in the talk on the example of the axiomatization that species quantum theories within the framework of non-commutative probability theory based on the theory of operator algebras. Non-categoricity is the result of existence of non-isomorphic von Neumann algebras, and it is intended because certain quantum systems provably cannot be described using certain types of von Neumann algebras. Viewed from the perspective of this axiomatization, both the Stone-von Neumann theorem and the closely related theorem of uniqueness of the C-algebra of the Canonical

Commutation Relations are in harmony with the intended non-categoricity of the axiomatization.

Reichenberger, Andrea

Does God Really not Play Dice? Ilse Rosenthal-Schneider's Cautious Struggle with Einstein

Abstract: In October 1931, the German Newspaper Vossische Zeitung published the announcement of an event with the headline: "Professor Albert Einstein and Professor Erwin Schrödinger will speak tonight at Schöneberg Town Hall on the subject of 'Natural Science and World View'. In doing so, they will also address the problem of causality." The author was Ilse Rosenthal-Schneider. In fact, her advertisement was a masterpiece of philosophical argumentation concerning foundational questions of physics at that time by focusing on the then hotly disputed topic of causality and probability in quantum mechanics. At the same time, her essay takes a more nuanced look at the standard narrative that Einstein did not understand QM-ironically, given his role in establishing quantum statistics. While Hans Reichenbach claimed in a 1928 essay entitled "Causality and Probability" in the same newspaper, the Vossische Zeitung, that mathematical physics had thrown the concept of causality overboard, advertising at the same time a new scientific philosophy, Ilse Rosenthal-Schneider drew attention to the problem from the Kantian a priori perspective: if the concept of causality precedes all experience, as Kant believed, then the causality thesis can neither be verified nor falsified on the basis of experiment and measurement. The punch line now is, whether causality holds or not, or whatever causality means: Even from the point of view of quantum physics, probabilities are preserved under symmetry operations. Conservation laws, deeply connected with principles of invariance under transformation, are not violated in quantum physics. According to Ilse Rosenthal-Schneider the property of invariance under transformation, in relativity theory as well as in quantum physics, constitutes the criterion for a "carefree objectivity" and reality. Ilse Rosenthal-Schneiders elaborated this idea in her 1952 paper "Limits of modern physics and their epistemological implications" in more detail, with a critical outlook on Werner Heisenberg's search for a smallest length. From a philosophical point of view, Ilse Rosenthal-Schneider continued her programme she began in her doctoral thesis in 1921: invariance principles consolidate the structure of reality and scientific truth.

Reiss-Sorokin, Ohad

An Untimely Meditation: F.A. Hayek and the Study of the Mind from the “Beiträge zur Theorie der Entwicklung des Bewußtseins” to The Sensory Order

Abstract: It is not for his work in psychology that Friedrich A. Hayek won its intellectual stardom. Yet, he considered his small contribution to the field-a short book titled The Sensory Order (1952)-an apex of his oeuvre. The Sensory Order presented a theory of the development of consciousness and its modusoperandi. In the book, Hayek reconsidered the criteria that explanation of phenomena such as the mind should meet and set the basis for his new methodological conception of science. Sciences that consider complex phenomena, he argued, cannot account for each detail. They should, instead, modestly look to explain the principles of its operations. What was true for consciousness, Hayek later argued, was also true to the study of society, the market, and evolutionary biology. "Untimely Meditation" follows the history of Hayek's theory of mind. Using archival documents and published sources, the paper explores the immediate reception of Hayek's ideas, when he first presented them in 1920 Vienna, when he further developed them in 1950s Chicago, and they finally found an audience in the 1970s. "Untimely Meditation" argues that Hayek's ideas were never considered "belonging to their time"; its early readers initially considered his work an outmoded remanent of the 19th century. Twenty years later, the new readership of The Sensory Order considered it prophetic. The paper uses this unusual reception history to ask two sets of questions. First, Hayek's career put him at the epicenter of three highly productive intellectual environments, all critical to the history of the philosophy of science: interwar Vienna, the LSE in the 1930s and 1940s, and mid-century Chicago. To be sure, Hayek was a developing thinker who changed his interests and ideas over the years. Yet, he returned to his theory of mind in these locales. Therefore, we can use it as a common denominator for comparing the three intellectual environments. Second, I see the story of The Sensory Order as a fascinating case study for exploring the tension between ideas and discipline. What makes an idea timely? Is untimeliness a criterion for the rejection of ideas? And what can it teach us about the relationship between discipline and truth? What are the social uses of "prophetic" ideas? And how and for what purposes are ideas being "resurrected"?Hayek, some argue, saw in his theory of mind a key to understanding his methodology and, thereby, his many contributions to the study of society. The curious history of its reception reveals the nonobvious relationship between ideas and their historical context.

Rendl, Lois

Peirce on the Logic of Science – Induction and Hypothesis

Abstract: In his Harvard Lectures on the Logic of Science from 1865 Peirce for the first time presented his logical theory of induction and hypothesis as the two fundamental forms of scientific reasoning. His study of the logic of science seems to have been initiated by the claim of William Hamilton and Henry L. Mansel that "material inferences", which Peirce calls a posteriori and inductive inferences, are to be considered as "extralogical". In consequence they regarded the principles of science, which Kant maintained to be valid a priori, to be as axioms "not logically proved". In opposition to this view Peirce in his Harvard Lectures seeks to establish first, that deduction, induction and hypothesis are three irreducible forms of reasoning which can be analysed with reference to Aristotle's three figures of syllogism as the inference of a result, the inference of a rule and the inference of a case respectively and second, with reference to Kant's doctrine that a synthetic "inference is involved in every cognition" and Whewell's distinction of fact and theory, that "every elementary conception implies hypothesis and every judgment induction" and that therefore we can never compare theory with facts but only one theory with another one and that consequently the universal principles of science, for instance the principle of causation, as conditions of every experience, understood as a theory inferred from facts, can never be falsified and are therefore valid a priori. Peirce develops his position examining the theories of induction of Aristotle, Bacon, Kant, Whewell, Comte, Mill and Boole. The paper will first reconstruct the main points of Peirce's discussion of the theories of these authors and give a critical account of his arguments and motives and second analyse his syllogistic and transcendental solution of the problem of a logical theory of scientific reasoning. This second part will be supplemented by an account of the significance of later revisions of the logic of science by Peirce in his Lowell Lectures (1866), the American Academy Series (1867), the Cognition Series (1868/69), the Illustrations of the Logic of Science (1877/78) and How to Reason (1893). The main focus of the paper will be Peirce's reformulation of Kant's conception of transcendental logic as a logic of science and therefore of synthetic reasoning with reference to a reinterpretation of Aristotle's Syllogistic.

Richardson, Alan

The Urgent Social Task of Hans Reichenbach's Natural Philosophy circa 1930

Abstract: In a variety of essays written in the final years of the 1920s and the beginning of the 1930s, Hans Reichenbach presented his "contemporary natural philosophy" as a resolution to a problem in the place of science in his culture. His essay "The philosophical significance of modern physics" (1930) for example begins and ends with concerns about the "alienation" [Entfremdung] between the world of science and the world of daily life. It is indeed the point of the essay to show how this alienation is to be resolved. This is also the framing of his "Aims and Ways of Contemporary Natural Philosophy" (1931) in which he presents an up-to-date scientifically and logically precise natural philosophy as the antidote to a misunderstanding about modern scientific method within "present day academic philosophy" that had led to an attempt to contrast the methods of science to the principles of understanding in everyday life. He presents his new technical natural philosophy as "determined by the present' [gegenwartsbestimmt] and as leading to a "turn, conscious of the present" [gegenwartsbewusste Wende] in theoretical philosophy.Attention to these concerns of Reichenbach provide one route toward reevaluating his place in the current literature on "the political project of logical empiricism." Far from being a mere Berlin exemplar of "right-wing Vienna Circle" thinking, he offered his own account of intellectual crisis of his times and how natural philosophy could help solve it. Whether or not the social task of natural philosophy is "political" by our lights or anyone else's, it is clear that Reichenbach thought natural philosophy had an urgent social task it needed to resolve.The paper will look at the ways Reichenbach presented the "alienation" between the world of science and the world of every life, who he thought was urging that this task was unresolvable and pointed to a need for an epistemology based on principles of everyday understanding (principally, Helmuth Plessner and Theodor Haering), and his alternative solution to the problem. Along the way we will see reasons why Reichenbach viewed his "public-facing" work in popular books and on radio not as mere add-ons but as central to his philosophical project, which from start to finish always had an important pedagogical aim.Works CitedHaering, Th. 1923. Philosophie der Naturwissenschaft.Plessner, H. 1930. "Das Problem der Natur in der gegenwärtigen Philosophie."Reichenbach, H. 1930. "Die philosophische Bedeutung der modernen Physik."Reichenbach, H. 1931. "Ziele und Wege der heutigen Naturphilosophie."

Rizza, Davide

Mal'tsev's early model-theoretic work

Abstract: Although the history of early model-theoretic ideas, between Loewenheim and Tarski, has been widely studied, comparable attention has not been paid to the model-theoretic work of A.I. Mal'tsev. This imbalance is perhaps not entirely surprising, since Mal'tsev's earlier contributions are marked by a discontinuity with dominant concerns and approaches that had characterised the best known model-theoretic work predating his. What is distinctive in Mal'tsev is a general neglect of foundational or metamathematical issues: in its place stands a newly gained awareness that logical results can be used to tackle algebraic problems, in the same way in which, within a different mathematical context, analytical results can be used to tackle number-theoretic problems. As a consequence, standard logical equipment acquires a novel significance and function. First-order languages are essentially problem-solving instruments, not means of rigorous codification; cardinality constraints on the language solely depend on the character of the algebraic problem involved, not on foundational scruple; logical results are regarded as a general framework presiding over the controlled reconstruction of algebraic particulars, rather than evincing a self-contained significance.I illustrate and examine these features of Malt'sev's work with special reference to three key articles on local theorems (Mal'tsev (1941, 1956, 1959), relating them to similar, contemporaneous investigations (notably Neumann (1954) and Robinson (1955, 1960)). The central idea I extract from this analysis is that Mal'tsev's use of logical ideas and results to transform algebraic subject matter endows those ideas and results with a novel significance. They are now regarded as constituents of a formal working environment in which the treatment and understanding of algebraic problems can be evolved in a new direction. New epistemic activities are consequently enabled: foremost among them are the detection of fundamental unifying features, the reconstruction of algebraic concepts and results, the expansion of logical work directed towards further refinements of interactions with algebraic content. This outcome displays a mathematical attitude of lasting significance in subsequent model-theoretic work. REFERENCESMal'tsev, A.I. (1971) The Metamathematics of Algebraic Systems. Amsterdam: North-Holland.Mal'tsev, A.I. (1936) `Untersuchungen aus dem Gebiete der mathematischen Logik', Matematicheskii Sbornik 1 (43): 323--336 [translated in Mal'tsev (1971), pp.1--14]Mal'tsev, A.I. (1941) `Ob odnom obščem metode polučenija lokal'nyh teorem teorii grupp', Ucenye Zapiski Ivanov. Ped. Inst. (Fiz-mat. Fakul'tet) 1 (1): 3--9 [translated in Mal'tsev (1971), pp. 15--21]Mal'tsev, A.I. (1956) `O predstavlenijah modeleĭ', Doklady Akademii Nauk (SSSR) 108: 27--29 [translated in Mal'tsev (1971), pp. 22--26]Mal'tsev, A.I. (1957) `Model'nye sootvetstvija', Izvestiya Akademii Nauk (SSSR), Seriya Matematicheskaya 23: 313--336 [translated in Mal'tsev (1971), pp. 66--94]Neumann, B.H. (1954) `An embedding theorem for algebraic systems', \textit{Proceedings of the London Mathematical Society 3: 138--153 Robinson, A. (1955) `Note on an embedding theorem for algebraic systems', Journal of the London Mathematical Society 30: 249--252Robinson, A.

(1960) `Recent Developments in Model Theory', in Logic, Methodology and Philosophy of Science: Proceedings of the 1960 International Congress, E. Nagel, P. Suppes and A. Tarski (eds), Stanford University Press, pp. 60--79

Robiolio, Amedeo

Leibniz on the Materialist Roots of Hobbes's Conventionalism

Abstract: Leibniz put forward a theory of truth surprisingly close to Hobbes's, the conceptual containment theory: a categoric proposition, A is B, is true if and only if the concept of the predicate, B is 'contained' in the concept of the subject (LA 56, DM 13). This definition reverses the proposal that Hobbes had given in De Corpore, that a proposition is true iff the concept of the subject is contained in the concept of the predicate (OL I 31). Leibniz's definition is intensional while Hobbes's was extensional, but they seem to agree that truth is in the conceptual subject-object relation, and does not refer to extraconceptual reality.Yet since even before Leibniz had formulated his conceptual containment view, he had insistently been attempting to refute the 'arbitrariness of truth' in Hobbes's "plus-quam-nomialism", "because truth allegedly depends on the definition of terms, and definitions depend on the human will." (A VI ii 428). Scholarship has recognised that Leibniz's reaction is more sophisticated than the standard (AT 178/79; Arnauld & Nicole p.68) response to Hobbes. That is because Leibniz recognised that for Hobbes words are both signs of thoughts and names of things (EW i, 17; EW, 18 iii, 25).But the root of Leibniz's rejection of Hobbes's truth theory has gone unrecognised. In Hobbes's materialism, the relationship between the thing which the word names and the thought which it signifies is a purely causal one (AT 168). But this is the destruction of what we would today call intentionality: the thoughts are not about the things, but merely occur because of them, and thus they cannot be said to correspond to reality as needed (NE 397). Various commentaries have recognised that Leibniz aims for intentionality through a language invariant structural analogy between thought and reality (L 184; G, vii, 219). This is because "... ideas are in God from all eternity, and they are in us, too, before we actually think of them." (NE 300). But the ideas in question here are the possibilities of individuals, not of universals (NE 323).What recent commentaries have overlooked is that this explains why Leibniz needs to move from the extensional to the intensional definition of truth. Leibniz is a (sui generis) nominalist (LA 101), thus, if truth depended on containment in the predicates (universals), the definition would be only nominal. Instead, by placing all predicates in their subjects, the God given idea which enables the thought of the concept is isomorphic to what is real, namely individual substances.Therefore the ultimate basis for Leibniz's rejection of Hobbes' theory of arbitrary truth is not just that 'truth is in things, not words'. More in depth, it is that Hobbes's theory leaves no room for a representation of things within us which is proportional to reality, and the reality-thought relationship is only causal. Leibniz's shift to intensionality was thus motivated by the goal of saving the veracity of intentionality.

Rodríguez Puente, Antonio

John Dewey on the Source of the Epistemic Value of Scientific Kinds.

Abstract: Despite the growing attention that John Dewey's philosophy of science has received recently, there has been almost no discussion of his views on scientific classifications. A reason for this could be that Dewey's view on classification is found in passages that seem to be concerned with logical questions (e.g., the nature of general propositions) rather than the epistemological and metaphysical issues that theories of natural kinds usually address. However, under closer inspection, these passages contain a robust and original account of the function and development of the classificatory tools of science. Even more, the case could made that some of Dewey's latter writings (1936; 1938) contain the prototypes of two recent developments in the philosophy of natural kinds, namely, the practice turn in the study of scientific classifications and the project of a purely epistemic (anti-metaphysical) account of the inferential roles of scientific categories. In regard to the former, Dewey holds that the structure of scientific kinds is shaped by the activities scientists are capable of performing at particular periods of time and that each new refinement of the definition of a given category is to be understood as a broadening of our potential ways of interacting with the environment. These ideas bear certain parallels with Catherine Kendig's (2016) and Hasok Chang's (2016) analysis of scientific classifications in terms of "activities of kinding" and "epistemic iteration". With respect to the latter, Dewey argues that the validity of scientific kinds as inferential tools is grounded on their systematic relations with other categories and generalizations. This idea departs from how most theories of natural kinds have explained the epistemic usefulness of classificatory categories. According to the standard explanation, the source of the inferential value of scientific kinds is a type of metaphysical structure that accounts for the stability of their properties. For example, in the HPCK theory (Boyd, 1999), the projectibility of a kind is explained in terms of the underlying causal mechanisms responsible for the clustering of its properties. This approach has been criticized for not being able to explain how we can know if our classificatory categories are aligned with the relevant metaphysical structures (Franklin-Hall, 2015). In stark contrast, Dewey (1938) holds that the inferential validity of scientific kinds is achieved by their transformation from "discontinuous successions of qualities" to "continuous interactions of characters". This transformation requires redefining the components of the set of properties of a kind and identifying the precise relationships that hold between them using previously validated categories and generalizations. The result is a pattern of interactions in which every change in a property of a kind relates to changes in other specific properties. Furthermore, through this transformation process, new kinds are integrated into a systematic network of categories and become tools for validating further kinds. This way of understanding the source of the inferential validity of classificatory

categories bears certain parallels with the purely epistemic accounts that some philosophers have recently proposed (Slater, 2015; Franklin-Hall, 2015).

Rodriguez-Warnier, Pascal

Bohrian perspectivism

Abstract: The present paper argues that Niels Bohr's views about quantum mechanics propose one of the first conceptions of perspectivism. In the contemporary general philosophy of science perspectivist frameworks have proliferated fruitfully, giving rise to different characterisations of this stance. The common element is that scientific knowledge should be understood as being situated. However, there are different ways and contexts to define such situatedness. Bohrian perspectivism, with its close connection to the philosophy of physics, advances the idea of the need for physical perspectives. The motivation of the Danish physicist is that quantum mechanics forces us to give a different answer to the question "How can we learn about the empirical world?" Unambiguous descriptions (or the ascription of well-defined values to a given quantity) of quantum systems can be made only relative to a perspective. Perspectivism enables objectivity and the possibility of cross-perspectives, hence the possibility of research communities, experimental replication, and the communication of scientific results. The perspective is physical in the sense that it can be defined purely by means of a set of physical variables that shall be described classically and in relation to which other systems (a special case being the target system of measurement) acquire definite values. Measurements are special cases of perspectives since the physical variables that record properties of the target system define a context of observation (the perspective). Interestingly, physical perspectives can be shared by different epistemic agents (for example, following a protocol for the measurement of a physical quantity) but, crucially, different perspectives might not be compatible among them – because of the quantum nature of the world. The latter characteristic is called the complementarity between perspectives: the description of a single quantum phenomenon requires perspectives that are generally exclusive between them, although jointly necessary for its exhaustivity.The paper is divided into three sections. The first one briefly presents perspectivist approaches in the contemporary general philosophy of science, together with their motivation. The second section offers an opinionated introduction to the crucial elements of Bohrian quantum mechanics. In the third section, it is argued that Bohr is a perspectivist and that the historical notion of physical perspectivism found in his readings has not been researched in all its deepness in the general philosophy of science.References:Bohr, N. 1935. Can Quantum-mechanical Description of Physical Reality be Considered Complete? Physical review, 48(8): 696. Bohr, N. 1949. Discussion with Einstein on Epistemological Problems in Atomic Physics. In: P.A. Schilpp (ed.), Albert Einstein Philosopher-Scientist. The Library of Living Philosophers, vol. VII. Pp. 201-241. Bohr, N. 1961. Atomic Theory and the Description of Nature. Cambridge: Cambridge University Press.Giere, R. 2006. Scientific Perspectivism. Chicago: The University of Chicago Press.Massimi, M. 2018. Perspectivism. In: J. Saatsi (ed). 2018. The Routledge Handbook of Scientific Realism. New York: Routledge. pp: 164-175.Massimi, M. 2023. Perspectival Realism. Oxford: Oxford University Press.

Romagni, Domenica

Kepler’s Geometrical Music Theory: Motivations and Significance

Abstract: In Book III of his comprehensive treatment of harmony in the universe, the Harmonices Mundi Libri V, Kepler delivers his theory of musical harmony. The account that he provides is striking for a number of reasons. For one, it is exceptionally detailed and complex. For another, it is highly original, making a significant break in its approach when compared to other theories offered at the time. In particular, instead of proceeding from an arithmetic basis, Kepler prefers to root his theory of music in in geometry. However, rather than sparking a new trend, Kepler's theory remained an outlier. Finally, his account of musical harmony is of central importance in providing part of the foundation for his his astronomical conclusions in Book V of the work. All of these considerations compel us to inquire into why Kepler was drawn to his particular account of musical harmony. In this paper, I will show that a number of considerations converged to lend support for his theory. These considerations are of two types: those that stem from Kepler's broader theoretical commitments and those that can be seen as free-standing. The former considerations include Kepler's criteria for the possibility and confirmation of scientific knowledge, his views on the nature of mathematical objects, and his views on the nature of the human mind. The latter include criteria that are not directly tied to the details of his philosophical program and that were shared by other theorists in the period. In particular, Kepler was concerned with accounting for available empirical evidence and providing conditions that adequately demarcate the phenomenon in question. I will argue that Kepler's theory satisfies both kinds of considerations especially well. On the side of the theory-specific considerations, Kepler's account of musical harmony serves as a paradigmatic case of the application of his theory of knowledge acquisition. On the side of the free-standing considerations, Kepler goes to great lengths to show that his account both captures the relevant features of our experience of musical harmony and picks out all and only the relevant phenomena.Recognizing these converging motivations behind Kepler's theory of harmony is important for a couple of reasons. First, it is significant for bringing out the place of his theory of harmony in his philosophical program. Concerns with internal consistency constrain what kind of a theory Kepler could accept, but the success of his theory according to independent criteria lends further support for his philosophical framework as a whole. Second, it provides a particularly interesting case of a philosopher in this period weighing considerations of internal consistency, empirical adequacy, and target precision in the formation of a highly original and complex account. Thus, it is important more broadly for understanding theory preference and selection in this period.

Romizi, Donata

Zilsel’s politically engaged philosophy of science

Abstract: Whether someone can be considered "a political philosopher of science" very much depends on the criteria that one takes as a normative standard for this definition. In this contribution, I would like to show how Zilsel's work can be regarded as a prime example of a politically engaged philosophy of science both according to the criteria formulated in Romizi (2012) with reference to the Vienna Circle's "scientific world-conception", and to most criteria formulated by Schlaudt (2018). In fact, Zilsel's philosophy of science and Zilsel's political commitment were two expressions (enactments) of the same attitude, which he supported publicly, and which had a clear political meaning at that time. Moreover, his involvement in the Social-Democratic party was essentially related to his work and position as a philosopher of science. To support my theses, I will draw in particular on Zilsel's writings preceding his forced emigration (e.g. Zilsel 1916, 1918, 1926/1972, 1927, 1931a, 1931b, 1932).ReferencesRomizi, Donata. 2012. „The Vienna Circle's ‚Scientific World-Conception': Philosophy of Science in the Political Arena". HOPOS: The Journal of the International Society for the History of philosophy of Science, Vol. 2, No. 2 (Fall 2012): 205-242.Schlaudt, Oliver. 2018. „A Political Meaning of 'Scientific Philosophy'? The Case of Edgar Zilsel". Philosophia Scientiae. Travaux d'historie et de philosophie des sciences, 257-287.Zilsel, Edgar. 1916. Das Anwendungsproblem. Leipzig: Barth.Zilsel, Edgar. 1918/1990. Die Geniereligion. Frankfurt am Main: Suhrkamp.Zilsel, Edgar. 1926/1972. Die Entstehung des Geniebegriffes. Ein Beitrag zur Ideengeschichte der Antike und des Frühkapitalismus, Hildesheim – New York: Olms. Zilsel, Edgar. 1927. „Über die Asymmetrie und die Einsinnigkeit der Zeit". Die Naturwissenschaften, 12: 280-286.Zilsel, Edgar. 1931a/1992: „Materialismus und marxistische Geschichtsauffassung", in: E. Zilsel, Wissenschaft und Weltanschauung. Aufsätze 1929-1933, ed. G. Mozetič, Wien / Köln / Weimar: BöhlauZilsel, Edgar. 1931b/1992. „Partei, Marxismus, Materialismus, Neukantismus", in: E. Zilsel, Wissenschaft und Weltanschauung. Aufsätze 1929-1933, ed. G. Mozetič, Wien / Köln / Weimar: BöhlauZilsel, Edgar. 1932. „Das mechanistische Weltbild und seine Überwindung". Der Atheist, 9: 129-131

Rosenhagen, Raja

Hansonian Seeing As: Contemporary Variations

Abstract: In this paper, Hanson's notion of seeing that is placed in the context of the contemporary debate, within philosophy of perception, between representationalists, relationalists, and presentationalists. The question to be explored is how cousins of this notion would feature in some of these contemporary accounts, and whether the resulting views are theoretically attractive. The paper proceeds by first contrasting the ability to see things as some kind of thing (or as being some way) or other with the notion of a perceptual-recognitional capacity-a notion that prominently features both in contemporary representationalist and in contemporary relationalist accounts. Hanson's notion of seeing as is shown to differ from such a notion in at least two important and related ways. First, the notion of a perceptual-recognitional capacity is factive; if perceptual-recognitional capacities are successfully deployed, this entails that the kind of perceivable features they latch on to are in fact perceptually available to the perceiver. Occurrences of seeing as, on the other hand, bear no such implication. Integrating the non-factive ability of seeing as into contemporary accounts of perception, it is argued, yields a potential advantage: the resulting accounts better accommodate that our background understanding of the world we encounter is constantly under development. Second, investigating Hanson's notion of seeing as–especially how for him, 'seeing as' is linked with 'seeing that'--reveals that instances of seeing as are intelligible only in contexts that are shot through with explicit or implicit modal assumptions. It is argued that while this is true also of perceptual-recognitional capacities, the facticity attributed to the latter entails that the modal aspects featuring in their individuation conditions are rigid, fully determinate, and (thus) typically reflectively opaque. Individuation conditions of seeing as, in contrast, are flexible, determinable, and (thus) less reflexively opaque. With this contrast in hand, it is shown how counterparts of Hanson's notion of seeing as can be integrated in each of the three contemporary kinds of account under discussion. Doing so will reveal that for differences obtaining between these accounts with respect to how in them, concepts are taken to operate, we find corresponding differences in what exactly, in each of them, seeing as talk will serve to capture. Such differences, it will be argued, constrain the degree to which these contemporary variants of a Hansonian conception of seeing as remain true to Hanson's own conception, as well as the degree to which they are theoretically attractive.

Roure, Pascale

Nezahat Arkun on “logical behaviourism” (1938). An early contribution to scientific psychology in Turkey.

Abstract: My paper examines a little-known contribution to discussions on behaviourism within the framework of logical empiricism in the 1930s. It presents and comments an early work by Hans Reichenbach's student at Istanbul University, Nezahat Nazmi [Tanç], better known under her marital name Nezahat Arkun, and situates it in the context of the development of scientific philosophy in interwar Turkey, including Reichenbach's efforts to establish and develop scientific psychology at Istanbul University. Nezahat Nazmi's graduation thesis on "logistic behaviourism according to Carnap and Reichenbach" (Mantıkî Behaviorism'in Carnap ve Reichenbach'a Göre Tefsiri) was completed in 1938 and published in the unique issue of the journal of the Istanbul University Department of Philosophy, Felsefe Seminarı Dergisi (1939). This thesis documents the reception of behaviourism in the movement of logical empiricism, combined with Reichenbach's interest in Gestalt psychology. This constellation is visible in the sources used in the thesis, which discusses the works of Egon Brunswik, who established in Ankara the very first psychological laboratory in Turkey in the early 1930s, and of Edward C. Tolman, who studied with Kurt Koffka in Giessen and developed according to Reichenbach a "very convincing form of behaviorism" (Experience and Prediction, 1938: 163) . Another aspect taken into consideration in this paper is the development of scientific psychology at Istanbul University, where Reichenbach was able to establish two additional chairs for exiled professors before leaving for the University of California in 1938. Ernst von Aster, who was in his early career in München and in Giessen interested in experimental psychology and psychoanalysis, was thus appointed in 1936 to the Chair of History of Philosophy and the psychologist Wilhelm Peters was appointed in 1937 to a new Chair of Experimental Psychology. This chair and the related institute were initially meant to be directed by a Gestalt psychologist such as Wolfgang Köhler or Adhémar Gelb. Peter's assistant and translator, Mümtaz Turhan, had studied in Germany between 1928 and 1935 and written his doctorate thesis under the direction of Max Wertheimer. As for Nezahat Arkun, she wrote her doctoral thesis on "Statistical Study on Suicide in Istanbul" (1948), under the supervision of Wilhelm Peters. She became in 1968 professor of psychology at the Istanbul University Department of Psychology and mainly pursued studies in social psychology, using statistical methods.

Roux, Sophie

The issue of the historicity of the sciences in Canguilhem

Abstract: In a book that is now twenty years old, Les Inquiétudes de la raison, épistémologie et histoire en France dans l'entre-deux-guerres, Enrico Castelli Gatinara mentioned the "double articulation" that French philosophers of science after Bachelard established between history and science, history asserting its scientificity and the sciences discovering their historicity. But is there a connection between the history of science and the history as it is written by historians? And, if not, where does the historicity of the history of science come from?Focusing on Canguilhem, I will show that, like Bachelard before him, he neglected history as a discipline and subscribed to Bachelard's thesis that, because of its progress, history of science is not a history like any other. But, contrary to Bachelard, he did not make use of the method of recurrence for writing history of sciences. His aim was indeed to reconstruct the history of science as an adventure in which the decisions that are taken lead to the emergence of unpredictable innovations and to the institution of norms of truth. The origin of this adventurous character of the history of science is, in his view, the gap existing between action and knowledge, between technology and science, or between life and concept. If the history of sciences is historical, it is therefore for Canguilhem because it requires the same kind of engagement as the one that makes one enter in the Resistance. These considerations lead me to show that Canguilhem was closer to Cavaillès than to Bachelard: for Cavaillès, as for Canguilhem, there are actions that imposed themselves but that nonetheless result from the decisions of an individual; there are actions that, while being totally unpredictable, in the sense that they can not be derived from the prexisting state of things, nevertheless create necessary truths.ReferencesBachelard, Gaston. 1951. L'activité rationaliste de la physique contemporaine. Bibliothèque de philosophie contemporaine. Paris: Presses universitaires de France.---. 1972. "L'Actualité de l'histoire des sciences." In L'engagement rationaliste, 137–52. Bibliothèque de philosophie contemporaine. Paris: Presses universitaires de France.Canguilhem, Georges. 2011-2018. Œuvres complètes, 5 vol., Paris, Vrin.Cavaillès, Jean. 1994. Œuvres complètes de philosophie des sciences, Paris, Herman

Russo Krauss, Chiara

The Philosophy of Joseph Petzoldt

Abstract: Avenarius and Mach are often regarded as a kind of double star in the history of philosophy. However, the idea that their philosophies are closely related goes back to the activities of their pupil, Joseph Petzoldt, who promoted the idea that the two thinkers marked the beginning of a new era in philosophy, namely, the rise of a proper scientific philosophy, capable of getting rid of the residues of metaphysical thinking hidden in both philosophy and science, and of giving science an epistemological foundation. The talk reconstructs Joseph Petzoldt's attempt to unify the ideas of his mentors into a coherent system of thought, Empiriocriticism. In particular, Petzoldt believed that Mach and Avenarius could provide an answer and antidote to the skepticism implied in the Kantian epistemology, which was the dominant philosophy at the time. Petzoldt's goal was to overcome the philosophical position that we only know appearances but are never able to truly grasp reality. However, he did not want to return to a naïve realism, but rather to prove that although we know the world only through our sense experiences, we are still able to gain an objective knowledge of actual reality. To accomplish this, Petzoldt emphasized the realist aspects of Avenarius' and Mach's philosophy and supported them with the concept of univocity (Eindeutigkeit), which he developed to explain the necessity of natural laws without falling into a metaphysical account of causality. Petzoldt uses Mach's and Avenarius' notion of "functional relationship" to explain the causality of natural events, according to which "if X changes, then Y changes", but he also emphasizes the univocity of this relationship, i.e., the fact that given a set of conditions, only one outcome can occur. The univocal relations are what hold the world together, the determinacy of reality expressed in scientific terms. Since the relations between the world and our nervous and sensory system is also a kind of univocal relation, our sense experience is not an appearance, but a necessary knowledge that gives us insight into the necessity of empirical reality. Thus, unlike Kant believed, necessity is not merely an a priori postulate that we impose on the chaos of sense impressions, but rather a feature of reality that we discover through our experience. However, the introduction of this strong notion of the necessity of reality meant a departure from Mach's philosophy, which presupposed a certain degree of indeterminacy. Therefore, it is important to highlight the differences between the Empiriocriticism of Avenarius, Mach, and Petzoldt in order to avoid projecting one set of ideas onto the others.

Sanches De Oliveira, Gui
Vicente, Raja

Radical Empiricism Repeatedly Misunderstood

Abstract: It is often said that "history repeats itself." While not defending this as a general thesis, in this paper we examine a particular instance of partial historical recurrence, namely in the critical reception of radical empiricism in two different contexts. First with William James, and later with James J. Gibson, radical empiricism has been repeatedly misunderstood by critics. As we propose, the misunderstandings were motivated by the same philosophical assumptions in both cases; curiously, however, those assumptions led to conflicting, even opposing criticisms in each instance.As articulated by James in the 1890s, radical empiricism agreed with classical empiricism in rejecting rationalistic nativism and, accordingly, seeing experience as the only possible starting point for mental life. Unlike classical empiricists, however, James rejected elementarism and associationism: for him, rather than having to combine independent atomic sensory impressions to try to make sense of reality, we directly perceive the world in its complexity, as made up not only of things (or their parts) but also of (meaningful) relations between (meaningful) things. A key feature of this framework was, therefore, its pragmatist orientation: meaning (including "truth") is built with practice, i.e., through experience. Radical empiricism gained a second life in the 1950s/60s through Gibson's research program of "ecological psychology" (Heft 2001). Gibsonian ecological psychology offered a comprehensive reworking of fundamental psychological categories, developing experimental methods to investigate how regularities in organism-environment systems enable the direct perception of relational meaning, i.e., affordances (Gibson 1966, 1979). In its original context, radical empiricism was widely misunderstood by James's contemporaries, who accused it of subjectivism or relativism, in more recent terminology (James 1907, 1912). Half a century later, radical empiricism gained a new range of detractors: from computationalists/cognitivists to enactivists, a common complaint has been that ecological psychology amounts to a problematic kind of externalism, sometimes even pejoratively compared to behaviorism (e.g., Fodor and Pylyshyn 1981, Varela et al 1991, Di Paolo et al 2017). Criticisms to each "incarnation" of radical empiricism thus curiously point in opposite directions: while James was seen as mistakenly overemphasizing the subject, Gibson is seen as mistakenly overemphasizing the external environment; while James was not enough of a realist, Gibson is too much of a (naive) realist. Both in the case of James and of Gibson, we propose, the same dualistic worldview is responsible for the criticisms. Ontologically and methodologically, radical empiricism rejects the dualities of mind/body, subject/object, organism/environment, as much as it rejects the dualistic separation between knowledge and its foundations, contingent practical experience and transcendent reality. Accusations that radical empiricism favored one or the other side of the duality are, therefore, only possible because of a (repeated) fundamental misunderstanding of radical empiricism, which is anti-dualistic

across the board. The primacy of practical embodied activity corrects the ontological and methodological misconceptions, and offers an alternative to foundationalism: pragmatism. Once such primacy is acknowledged, we are in a position to grasp the full scope of radical empiricism and the implications of its different manifestations.

Sandner, Günther
Lucas, Baccarat Silva Negrao De Campos

Life and Working Partnerships in Logical Empiricism: Anna Schapire, Olga Hahn and Ina Stöger

Abstract: Assessing the role of women in the history of philosophy implies not only revisiting the philosophical canon, by acknowledging the work of great women philosophers that went unnoticed because of socio-political issues. Besides the works of remarkable figures, one ought to recognize the extremely relevant role played by many of the partners of well-established academic philosophers. Many women that were prevented from getting academic positions and publication opportunities because of a repressive political environment, have nevertheless made substantial intellectual contributions by being critical interlocutors or intellectual reference persons of their partners. In our talk, we examine the role of the wives of two major figures of logical empiricism, and how their insights and cooperation were instrumental for the general development of the movement. In particular we shall present three case studies: Anna Schapire and Olga Hahn, the first two wives of Otto Neurath's and Elisabeth (Ina) Stöger, Rudolf Carnap's second wife. Otto Neurath's life partnerships were always also working partnerships: with Anna Schapire, Olga Hahn and Marie Reidemeister. Anna Schapire was older than Otto Neurath by about five years and politically active even before she met Neurath; she stood up for workers and for women's rights. She was active as a social and literary scientist, as a prose writer and poet, but also as a translator and she exercised great influence on Neurath's intellectual and political development in many respects. Olga Hahn studied mathematics and philosophy and in 1911 became only the third woman to receive a doctorate from the Faculty of Philosophy at the University of Vienna. She married Otto Neurath after Schapire's death (1911) in 1912. Olga Hahn already took part in meetings of the "first Vienna Circle", as well as those of the Schlick Circle and other philosophical discussion groups. She died in 1937 in exile in the Netherlands. Schapire and Hahn were intellectual women who, despite the restrictions that existed for women at universities, completed their studies and worked intellectually: they participated in discussion groups, corresponded in intellectual networks and took a stand on scientific and social issues. Both - Schapire and Hahn - wrote also joint publications with Otto Neurath.Even Ina Carnap had formal training in philosophy, unlike Schapire and Hahn, she never co-authored one of her husband's contributions. Nevertheless, as an analysis of the private Carnap´s correspondence shows, not only was she responsible for putting many of Carnap´s letters and writings on paper, but she would also critically engage in the writing process, sharpening his formulations and pointing out significant shortcomings in his writing-style and thinking. Furthermore, long before her husband, Ina has shown and did some work in modal logics. The topic would be later on taken up by Carnap, thus indicating some sort of intellectual influence from her. A constant and strong presence in academic

events, Ina actively discussed and assessed other philosophers' lectures and seminars, thus displaying a relevant role in logical empiricism.

Santos Genta, Benjamin

Inductive Logic and Analogies: A History

Abstract: How should a rational agent update their beliefs in light of new evidence? Consider the case where an agent has made a series of observations and wants to know what degree of belief they should assign to observing a certain outcome i in the next observation. Simple enumerative induction essentially prescribes that they should update their beliefs based on their initial probabilities of observing i, and the frequency with which i has previously been observed. Johnson (1932) and Carnap (1950) independently derived formal versions of enumerative induction that also allows for different initial priors and arbitrary sensitivity to new evidence. Though this achievement was monumental for inductive logic, the Johnson-Carnap system could not account for the effects of analogical influences on inductive inferences. Given that the system was supposed to rationally reconstruct the concept of confirmation in science and to provide a decision-making guide for scientists, this was a significant limitation. The contribution of this paper is twofold: first, it is to provide a conceptual mapping of the treatment of analogy in the inductive logic tradition, from Hosiasson-Lindenbaum (1941) and Carnap (1945; 1980) to modern commentators. In particular, I distinguish three kinds of extensions of the inductive logic system (with respect to analogies): axiomatic (e.g. Huttegger 2019), parametric (e.g. Romeijn 2006), and geometrical approaches (e.g. Sznajder 2021). I summarize some important findings of these approaches. The most notable criticism of the system with respect to analogical influences comes from Achinstein, who writes that Carnap's inductive logic cannot incorporate cases such as the following: "If we have evidence that metals such as copper, iron, and zinc can be melted, this provides background support for the hypothesis that rhodium can also be melted" (1963, 209). To address this concern, Carnap (1980) noted two types of analogical influence in scientific practice: (1) analogy by similarity, which captures the idea that observing an outcome increases the probability of observing another similar outcome in the future; and, (2) analogy by proximity, which is supposed to capture the importance of temporal proximity. The second contribution of this paper is the following: I argue that the literature was operating on a very productive but fundamental mistake, since the extensions of the system did not capture the analogical influence Achinstein had in mind. I show that neither analogy by similarity nor by proximity properly reconstructs this type of influence because the outcome is the same across the different metals ('melt' and 'not melt'). The analogical influence needed is an analogy between types-a recent paper by Huttegger (2019) captures this kind of influence but does not highlight it.

Santos-Sousa, Mario

Basic Number Knowledge: Recovery of a Humean Insight

Abstract: This paper vindicates Hume's deep, but widely neglected, insight into the nature of our basic number knowledge and argues that this insight is borne out by recent empirical research on the topic. In his Treatise (1.3.1), Hume suggests that we have a natural, albeit limited, capacity for quantity discrimination, a suggestion largely supported by the available empirical evidence. He even comes close to describing the discrimination thresholds observed in number comparison tasks and supplies a precise standard (in terms of one-to-one correspondence) for gauging exact numerical equality beyond these thresholds. It is to this standard, which has come to be known as 'Hume's principle', that most attention has been devoted in contemporary philosophy of mathematics—often at the expense of his broader views. This paper aims to cast new light on the epistemological status of Hume's principle against the backdrop of his original insight. Just as Hume anticipated, there's evidence for a natural capacity that allows us to detect the cardinal size of collections of perceptually presented items. However, this capacity is very limited, for it only comprises an exact sense of small cardinal size and a rough sense of larger cardinal values: we discriminate small collections of objects (but only up to a certain threshold of about four) and are able to approximate larger numerical quantities (yet fail to capture numerical differences below a certain ratio). According to Hume, "[i]n all other cases we must settle the proportions with some liberty, or proceed in a more artificial manner" (Treatise 1.3.1).It is in this context that he lays down his famous standard: "When two numbers are so combin'd, as that the one has always an unite answering to every unite of the other, we pronounce them equal" (ibid.).But how do we come to possess such a standard of numerical equality, and what justifies its adoption? In order to make progress on these questions, I will try to spell out what judging the equality of numbers 'in a more artificial manner' might possibly involve.Notice that we use all sorts of artificial methods to go beyond what our natural capacities can achieve (think of thermometry, or time measurement). Following Hume's lead, I would like to suggest that we use one-to-one correspondence as a method to get around the limitations of our sense of cardinal size. Crucially, and just as with any new method of measurement, its adoption is initially justified on the basis of sensation. When sensation gives out, however, it is the new method that sets the relevant standard for judging numerical equality with precision—so much so that, once established, we rely on it not only to sharpen, but even to correct sensation. What's the upshot? On the one hand, Hume's standard of numerical equality is not secured by merely reflecting on our ideas of number; and yet, on the other, it clearly transcends the evidence of our senses. Thus, I submit, Hume's principle might be regarded as synthetic (because non-analytic) and a priori (because non-empirical).

Schechtman, Anat

Locke on Whether a Science of Color is Possible

Abstract: A recent biography of Alexander von Humboldt by Andrew Wulf opens with his 1802 attempt to ascent Mount Chimborazo. Wulf writes:For the ascent of Chimborazo, he had left most of the baggage behind, but had packed a barometer, a thermometer, a sextant, an artificial horizon and a so-called 'cyanometer' with which he could measure the 'blueness' of the sky. (2015, 1)Invented in the 1760's, the cyanometer was a circular device comprised of 52 colored and numbered shades of blue, from white (0) to black (51). The "degree" of blueness was determined by matching the sky's color to the most similar shade on the cyanometer. Whereas Humboldt clearly accorded great importance to this measurement of color, to certain readers of Locke it may seem anything but scientific. Scientific knowledge of color would have to proceed by gaining "epistemic access to the primary qualities of the inner corpuscular structure of things" (Anstey 2011, 146) underlying color and other sensible qualities. The arrival on the scene of the cyanometer in the eighteenth century might appear as a disappointing backslide from this perspective. This essay challenges this reading, proposing that Locke's stance on the possibility of a science of color is more complex than the quick sketch above might suggest. I begin with a presentation of Locke's theory of quantity (in EssayII.xvii) and argue that it centers on three principles governing quantities and only quantities (section 1). Moreover, the satisfaction of these three principles entails that quantities and only quantities are measurable; qualities are not (section 2). The measurability of quantities but not qualities in turn explains Locke's position that "modes of qualities are not demonstrable like modes of quantity" (Essay IV.ii.11), i.e., they do not allow for demonstrative knowledge of equality and difference as quantities do (section 3). Taking stock at this point would suggest that a science of color is doomed for Locke. The last two sections examine two ways to avoid this dispiriting conclusion. First, Anstey's comments above can be understood as proposing that qualities are measurable and therefore demonstrable, if not directly then via a proxy, by measuring the quantitative properties of underlying corpuscles. While there are passages in Locke that can give rise to this reading, I believe that in the end it is not convincing (section 4). Instead, I propose that Locke can be understood as allowing for scientific yet non-demonstrative knowledge of color. This suggestion accords with recent positions by scholars (e.g., in Connolly 2018) that Locke makes room for both demonstrative knowledge and Baconian natural histories in his view of scientific knowledge. On this reading, Humboldt may be viewed as making a valuable contribution to the latter with his cyanometer, which was not carried up Chimborazo in vain. Works citedAnstey, P. (2011). John Locke and Natural Philosophy. Oxford: Oxford University Press.Connolly, P. (2018) "Locke and the Methodology of Newton's Principia" Archiv für Geschichte der Philosophie 100 (3): 311–335.Wulf, A. (2015). The Invention of Nature: Alexander von Humboldt's New World. New York: Penguin Random House.

Schemmel, Matthias

Reflection on Science and Society at the Starnberg Max Planck Institute (1970-1980): A Forgotten Starting Point for Political Epistemology

Abstract: In 1970 the Max Planck Society founded the Institute for the study of the living conditions of the scientific-technical world in Starnberg with Carl Friedrich v. Weizsäcker as its founding director. The institute was devoted to study the connection between the various ongoing crises of humanity (nuclear threat, economic injustice, ecological decline etc.). One element connecting all these crises was the role of science and science-based technologies in modern societies. The reflection on science and its history was therefore a natural task for a group of researchers at the institute. This group became known for the formulation of a thesis on the "finalization of science", but this was in fact only one aspect of their broader approach to the role of science in society. In my presentation I will highlight certain discussions pursued at the Starnberg institute on the connection between societal transformation and science throughout history and raise the question of how they can be used in the context of a political epistemology reflecting on the role and character of science in the Anthropocene.

Schindler, Samuel

Beyond footnotes: Lakatos' meta-philosophy and the history of science

Abstract: Lakatos famously advised to detail the actual history of science that "misbehaved" in the light of the rational reconstruction of science in the footnotes of philosophical texts (Lakatos 1971, 107). Unsurprisingly, this did not bode well with historians (Kuhn 1971, Holton 1974; 1978, Kuhn 1980, Arabatzis 2017). It did not help much that Lakatos later called this an "'unsuccessful joke" and claimed never to have suggested in all seriousness that one should treat history of science like that (Lakatos 1978, 192). The damage had already been done (see e.g., McMullin 1970, Kuhn 1971, Holton 1974, Koertge 1976, Laudan 1977, Holton 1978, Kuhn 1980, Godfrey-Smith 2009, Arabatzis 2017). This is unfortunate in many ways. Not only has the dismissive view of the history of science implied by Lakatos's "joke" given philosophers a bad reputation among historians, but it also has overshadowed some of the more reasonable things Lakatos had to say about historically grounded philosophy of science. In this paper I argue that – contrary to the widespread cliché – Lakatos actually believed that good philosophy of science ought to accommodate as many actual, undistorted historical facts as possible. More specifically he thought that philosophical methodologies could be compared by comparing how many historical facts would come out as rational. Thus, if methodology A accommodates x historical facts as rational and methodology B accommodates y historical facts as rational, then if y > x, we should prefer methodology B. I call this idea the "maximization of rational facts". This idea is only sustainable if historical facts are not distorted.My argument is based mostly on a close analysis of Lakatos's "History of Science and its Rational Reconstruction", which first appeared in the 1971 PSA proceedings (Lakatos 1971), and later also in Lakatos (1978). This paper has often been misconstrued as giving advice to historians of how to do history of science (Holton 1974; 1978, Arabatzis 2017, Kuukkanen 2017). But even though Lakatos indeed seemed to address "historians" in his paper, he never really engaged with the work of historians nor was he really interested in their concerns (see also Dimitrakos 2020). Instead, he squarely focused on philosophy and how history could be used to support philosophical claims. As he put it himself, his arguments were "primarily addressed to the philosopher of science and aimed at showing how he can – and should – learn from the history of science" (122). Lakatos's project is thus better described as meta-philosophical.

Schlimm, Dirk

An early debate about discovery and justification in mathematics: Klein and Pasch on intuition and proofs

Abstract: Both Moritz Pasch (1843-1930) and Felix Klein (1849-1925) made significant contributions to the development of geometry: Pasch presented the first axiomatic system for projective geometry and put forward a deductivist view of mathematics according to which inferences should be independent of diagrams; Klein presented a unified view of various geometries on the basis of projective geometry and suggested to characterize geometries based on invariants under group transformation ("Erlanger Programm"). Both shared a general empiricist outlook (a fact that both emphasized repeatedly) and they held similar views on the nature of intuition ('Anschauung'), namely that it can represent mathematical notions only inexactly and approximately. Klein and Pasch interacted in the course of five decades through letters and occasional remarks in their lecture notes and publications. These documents provide us with an ongoing series of interactions that allows us to follow their views over an extended period of time. In particular, they offer us invaluable insight into the origins of their empiricist positions and reveal how Pasch's view remained remarkably static, while Klein's position gradually evolved over time.The interchanges between Klein and Pasch revolve around a small set of themes and are also fairly self-contained, both in regard to the texts that are discussed and in regard to the authors that are alluded to. The recurring themes in their discussion are the role of axioms in mathematics, the examination of the nature of mathematical intuition and its role in proofs, and the underlying tension between seeing mathematics as a creative endeavor that aims at new discoveries and as an attempt to provide justifications for its results by rigorous arguments. At one point Klein remarks that the only people who have seriously paid attention to the problem of the reliability of spatial intuition from a mathematical point of view, are, besides himself, Pasch and Köpcke (Klein 1902, 41).Taking into account previous work by F. Biagioli, H. Heller, D. Rowe, D. Schlimm, and R. Tobies, my talk will present, on the one hand, the philosophical views on the nature of mathematics of Klein and Pasch in light of their interactions; and, on the other hand, I will discuss the dynamics of their interactions from first encounters, through emerging disagreements, convergence and reconciliation, to mutual disregard in the end.

Sciortino, Luca

Styles in Art and Styles in Philosophy of Science Historical Confluences and Hidden Connections

Abstract: In the course of the history of philosophy various scholars have argued that the analysis of the history of science reveals ways of thinking very different from one another. In order to formulate and illustrate this idea, these scholars have proposed, implicitly or not, different notions of the concept of 'ways of thinking'. Just to mention a few of these notions, Ludwick Fleck (1896-1961) introduced the notion of 'thought style', Michel Foucault (1926-1984) proposed the notion of 'episteme' and, more recently, Ian Hacking (1936-2023) put forward the notion of 'style of reasoning'. The term 'style', which appears in the label of some of the different notions of 'way of thinking', points to a concept born in the art-historical discourse. The sociologist Karl Mannheim (1893-1947) was one of the first scholars to adapt the concept of artistic style to the study of human thought. He praised the art historian Alois Riegl (1858-1905) for providing an exact description of this concept which served as a model of inspiration for the formulation of his notion of 'style of thought'. Later, the term style reappeared in the writings of several philosophers of science.There is little agreement among art historians about how to characterize the notion of style. Some of them have proposed conceptualizations different from that of Riegl, which also have had an echo in the philosophy of science. However, there are recurring aspects in the description of style which are fertile in applications in the study of the evolution of science. For instance, in the art-historical discourse often 'style' points to two different directions: on the one hand, it designates a formal, compositional element that appears repeatedly in many works of art of the same trend; on the other hand, it alludes to principles and norms related to representation within a social or historical context. The latter sense has had greater success in the philosophy of science, but I shall ask whether the former sense is susceptible of important applications in the conceptual analysis of the history of thought.In the first part of the paper, I reconstruct the close interplay between artistic and epistemological considerations in the genesis of the first notions of 'ways of thinking' in philosophy of science. What features of the concept of artistic style, in its various versions, have been introduced or adapted or reconceived by the philosophers of science for the study of scientific thought? This is the main question that I shall address first. In the second part of the article, I shall instead highlight those considerations in the art-historical discourse relating to the concept of style to which philosophers of science have not paid the necessary attention and which, however, are implicitly contained in some notions of 'ways of thinking'. The complex set of tangled congruities and incongruities between the two discourses, the artistic and the philosophical, will shed light into the different ways by which philosophers of science have tried to model the evolution of scientific thought.

Scott-Fordsmand, Helene

Reasoning at the fringe of meaning: Mary Hesse, analogies, and the dynamics of unframed enquiry

Abstract: Mary Hesse is undoubtably one of the major philosophers thinking about analogies in philosophy of science. Her work on analogical reasoning stretches across her oeuvre, and she draws on a variety of philosophical work to explore and understand the role of analogy in scientific reasoning and scientific practice; from Aristotle over Duhem to Max Black and, in her later work on to Habermas. Her motivations for invoking analogy are many, and her work has had an impact on several debates in philosophy of science, including questions on the nature of scientific theories, the role of models and questions on explanation.This paper, however, looks at the way she invokes the notion of analogical reasoning to make sense of what I will call 'reasoning at the fringe of meaning', that is, reasoning in territory which is unsettled. An important domain of scientific work is the engagement with, and the ability to reason about the yet-to-be-known; what Rheinberger has called 'epistemic things' (e.g., Rheinberger 1997), and what Henne – drawing on Dewey and Carnap – calls unframed enquiry (Henne 2023). Hesse asks: how do we use existing theories to grasp new – that is, un-theorized – phenomena, and how do theories extend and change to encompass these phenomena? And she answers that we and they do so, by way of analogy. That is, analogical reasoning is central in understanding science as a dynamic practice. For Hesse, scientific theories are not univocal in their definitions, but rather contain "a fringe of meaning not defined by observation" (Hesse 1962). It is this fringe which makes reasoning extendable through the use of analogy, but it is also this fringe which introduces a continuous instability of meaning in science, even at its established core, creating dynamic loops of meaning change. In the paper, I first explore how Hesse – drawing on Black's interactive view of metaphor – conceptualises the dynamics of reasoning at the fringe of meaning. Second, I relate Hesse's conceptualisation to similar tropes in historical epistemology, namely James' notion of 'fringes' in consciousness (1892) and Husserl's notion of horizons and institutions [stiftung] in scientific development (1936 [1970]). Doing so, I aim to deepen our understanding of Hesse' position through the contrast and similarity to these other positions. Finally, with a better grasp of the dynamics of analogical reasoning at the fringes of meaning, I return to the Hesse's broader work on analogy to show how the emphasis on dynamic enquiry permeates her work, and sets her theories and aims apart other accounts of analogical reasoning in philosophy of science.

Seifert, Vanessa

The changing meaning of discovery in chemistry

Abstract: In philosophy, the concept of scientific discovery has received considerable attention. Its investigation has been connected to issues such as the nature of knowledge, scientific realism, confirmation, and the distinction between science and pseudoscience. In this talk, I investigate how it has been construed in science with respect to the discovery of chemical elements. I show that what it means to discover elements is not understood in the same way today as it was in the past, resulting in a much more fluid concept. This in turn prompts us to revise the role philosophers assign to discoveries in science.Initially, due to the advances of Lavoisier and Mendeleev, it was accepted that in order to achieve a discovery, one would have to produce by some means of isolation a sufficient amount of relatively stable pure substance. Moreover, one should be able to produce empirical evidence that the pure substance possesses physical and chemical properties that are close to those predicted for the unknown elements posited by the periodic table. By the beginning of the 20th century, practical difficulties made it hard to fulfil these requirements. The turning point came between 1925 and 1937, when rhenium and technetium were respectively discovered. From this point onwards, elements were not found through a process of isolation but via synthesis. Further difficulties emerged between 1955 and 1984 with respect to elements with atomic numbers between 101 and 109. These lead to controversies within the chemical community, with false claims as to the discovery of elements and priority disputes about who should be credited and allowed to chose the name of a new element. All this lead the scientific community to an unprecedented institutional decision. The International Union for Pure and Applied Chemistry, together with the International Union for Pure and Applied Physics, established the Transfermium Working Group (TWG). The TWG produced a report which established the criteria for a purported element discovery (Wapstra 1991). These criteria show how the role of previously central features of element discovery became restricted or discarded.Moreover, the proposal of these criteria instead of rendering the concept of element discovery more precise, lead to the formation of a more fluid concept. The 1991 report acknowledged that it is not clear which set of criteria should be taken as decisive, nor how to interpret them in a way that matches all instances of element discoveries. Instead, it recommended that the criteria are weighed on a case-by-case basis, depending on the element that a research team is set to discover. The adoption of such a fluid concept of element discovery prompts pressing questions about standard philosophical issues around scientific discovery. This includes how philosophers should construe the role of discoveries in the evaluation of scientific advances. Wapstra, A. H. (1991). 'Criteria that must be satisfied for the discovery of a new chemical element to be recognized'. Pure and Applied Chemistry, vol. 63, no. 6, pp. 879-886. https://doi.org/10.1351/pac199163060879

Sekatskaya, Maria

Reductionism, Supervenience, and Carnap's Account of Empirical Confirmability

Abstract: The methodological discussion of reductionism has a far reaching history within the philosophy of science. Particularly Rudolf Carnap was one of the first proponents of logical positivism and empiricism who explicitly discussed reductionism with respect to the mental from a philosophy of science perspective already as early as in his (1928/2003). Reductionism plays also a central role in debates within the philosophy of mind. However, the predominant notion used there stems from Ernest Nagel (1961), a notion that was also proposed by the early Carnap but then increasingly modified and adapted in order to keep up with new developments in science as well as its logical methodology.In this contribution, we want to investigate Carnap's development with respect to reductionism and link it to debates and accounts of the philosophy of mind. Surprisingly, Carnap's reductionism did not gain much attention within this field. Although, e.g., van Riel (2014, p.154) and van Riel and Gulick (2019, sect.1) speak about the 'Carnap-model' of reductionism, they refer only to his early account of explicit definability. Also, quite interestingly, the most influential account of Nagel (1961) does not mention Carnap in the context of reduction. Although recently Leitgeb and Carus (2020, Supplement E, section 1) provide a comprehensive overview of Carnapean accounts of reduction (they cover basically all accounts of reduction we will also cover), it was mainly von Kutschera (1991) who discussed Carnap's reductionistic development and linked it to debates within the philosophy of mind. However, the latter investigation has a gap with respect to what we call the empirical confirmability account of reduction which, as we want to argue, can be connected to models of supervenience physicalism with an epistemic reading of supervenience. Although the main aim of our investigation is to outline Carnap's reductionistic development and indicate which models and accounts of the philosophy of mind can be considered to suit which parts of the development quite well, we also want to indicate a consequence one can draw from the more historical discussion for the systematic framing of positions within the philosophy of mind, namely that the notion of reduction should be not considered as a binary property that distinguishes reductive from non-reductive accounts; rather, it should be considered to be a gradual notion distinguishing stronger from weaker forms of reductionism.Our investigation proceeds as follows: In the first part we outline the big picture of philosophy of mind, a set of relevant conditions of adequacy, and accounts that are relevant for our investigation. In the second part, we discuss Carnap's reductionistic development. In the third part, we link the relevant accounts of the philosophy of mind to Carnap's reductionistic accounts and discuss how the latter fare with respect to the conditions of adequacy from the philosophy of mind. We conclude with a short comparison of the adequacy profiles of the accounts and an outline of what one might take away from this investigation for the framing of accounts within the philosophy of mind.

Simons, Massimiliano

What is the History of Philosophy of Experimentation a History of? The Cases of Hugo Dingler and Gaston Bachelard

Abstract: Though 'experimentation' as a philosophical topic is typically seen as a product of the 1980s, reflections on experimentation have a longer history. This longer history can be read as a hotbed of different ways in which the aim of philosophy of science was understood. This paper illustrates this through an examination of Hugo Dingler and Gaston Bachelard, embodying different conceptions of what philosophy of science should aim for, and how reflecting on experimentation is part of that project. I will argue that, though both authors put experimentation central to their projects, they do so for radically different - and even opposed - reasons: whereas Dingler stresses experimentation as a way to find closure and ground our scientific concepts, Bachelard emphasizes the experimental side of science precisely in order to put the openness of science central. The diversity of analyses of experimentation is thus a product of the diversity of identities that philosophy of science as a project can have. Understanding these underlying projects will moreover shed light on their reception histories and why both projects are so little discussed in current mainstream philosophy of science. Therefore, to understand the history of philosophy of experimentation, we need to write a history of the aims of philosophy of science.

Smeenk, Chris

Leibniz and Newton on Color

Abstract: Seventeenth century philosophers faced a profound challenge in characterizing the contribu- tion of sense experience to knowledge of nature. One response to this challenge took the sensible qualities characterizing objects to fall into two types: those that are in some way relational, in that they depend upon interactions between external objects and our senses, and those that are intrinsic to the perceived objects. The latter (later called "primary qualities") provide the tools to formulate a mechanistic view underlying ordinary phenomenal experience, which can be laid bare by stripping away embellishments due to our senses. Yet, to paraphrase Margaret Wilson (1999), it seems strikingly na ïve to assume that the fundamental explanatory concepts in natu- ral philosophy would be the properties "given" in ordinary experience of bodies – such as size, shape, and motion. Wilson credits Leibniz for his recognition that the account of bodies in the mechanical philosophy would need to be supplemented. Here I will argue that Newton deserves similar credit, and approach the general question of how Leibniz and Newton conceived of the contribution sense experience makes to knowledge through an assessment of their research in optics.Color was an exemplary "secondary quality," regarded as properly attributed to the perceiver, at least in part, rather than solely to external objects. Yet following Descartes it seemed feasible that a mechanistic account of light, and of our own sense organs, would be able to account for fea- tures of sense experience, including colors. Newton and Leibniz both developed theories of vision and theories of the constitution and nature of light in response to the Cartesian tradition. I read Leibniz as concerned with a collapse into a form of phenomenalism. He deployed the kind of ar- guments that other philosophers had used to motivate subjectivism regarding secondary qualities to apply to primary qualities as well. Although he (arguably) still held that there was something "real" in objects, it is not clear whether how this can be accessed through sense experience. By contrast, Newton treats one sense of color – the color of what he calls a homogeneal light ray – as a fundamental property, even though it clearly lacks one of the features of primary qualities. Explaining how color as a dispositional property of homogeneal rays relates to perceptual expe- rience requires a theory of vision. Such a theory is an essential part of Newton's explanation of his claim that a mixture or compound of rays, each possessing the innate dispositional property of "color-making," appears to us as an entirely different color. Further experiments isolate and amplify aspects of the phenomena not apparent in ordinary circumstances. There is no limita- tion, on Newton's approach, to identifying the objective features of the nature of things through a selection from among sensible quantities. This approach also provides a clear response to the worry regarding inaccessibility of the objective properties: insofar as it is reliable, the instrument of vision gives us access to real properties of light and, derivatively, objects.

Smith, Brian

John Locke, the Philosophy of Time, and the Occultic Origins of Revolution

Abstract: The purpose of this paper is to show that John Locke's well-known theory of revolution was grounded in both astrology and the seventeenth-century proto-scientific theory of time known as chronology. Not only did he believe that earthly events were connected to planetary movements (comets and convergences), but he held a view of time where past historical events were naturally divisible into epochs of political significance. Time had a "true order." In his unpublished manuscripts, Locke composed his own sacred chronologies that sought to account for this order. On this view, the classification of political epochs was not merely a means of historical narration but an attempt to understand the structure or being of time itself. Much like other measurements of duration (seconds, minutes, hours, etc.), epochs were believed to occur at regular intervals. Since epochs were definitionally linked to the duration of a political age, this implied that epochal periods would produce a determinative structure. Great regimes could persist only to the limit entailed by the epoch in which they were ensconced. This philosophy of time helps to explain Locke's view of terrestrial revolution. Revolution could either be seen as the epicycles of regime degeneration and repair within an epoch, or the often cataclysmic transition between epochal periods. Understanding the structure of time Locke was working with helps to shed new interpretive light on his "appeal to heaven," the ultimate uncertainty that arises during revolutionary moments, and his recommendation to the Grecian Christians to strive for representation rather than independent sovereignty.

Solomon, Monica

Choosing the Simpler Problem Proves to be a Difficult Problem

Abstract: One of the upshots of Brading and Stan's new book is the gradual displacement of the concept of "body" from its once central place in physics. They follow the arduous debates around the problem of collisions during the late seventeenth and eighteenth centuries, only to expand later to a vast landscape of problems in rational mechanics. This paper develops one strand of their insights: the role of abstraction and idealization in physical theorizing as illustrated in the work of eighteenth-century Newtonians, such as John Keill and Willem 's Gravesande.The problem on which I focus is the following one: both Cartesians and Newtonians thought that the rules of collisions (and laws of motion, in general) do not hold perfectly in any chosen instance of bodies interacting. For example, subsuming a pair of interacting bodies under a particular quantitative rule requires idealization and abstraction. Similarly, recovering "real" bodies from any set of given quantitative features will require a choice of relevant fundamental quantities, upon which more complex systems can be "built" at a later stage. I will show that John Keill, in particular, points out that the core challenge at the heart of mathematical philosophy is to make explicit how more complex problems (and systems) can be built successfully starting from simpler ones.One of his rules of physical theorizing states that "it is necessary to begin with the most simple cases at first; and having once settled them, we may thence advance to such as are more compounded. So the same mechanical philosophers at first suppose the motion of bodies to be in vacuo, or in a medium that has no resistance; and having determined the laws of motion in that case, they thence proceed to investigate the laws of resistance, and lastly to discover what changes are thereby likely to arise to bodies in motion." (Keill 1720, 10)I will explain how, on this account, Cartesians are seen at fault, while Newtonians have more to offer. Finally, I construct a parallel with a similar difficulty presented in recent discussions in the philosophy of physics concerning the role of approximations.

Spano, Christopher

Cassirer, Mathematics, and Intuition

Abstract: The dominant interpretation of the neo-Kantian Marburg School is that they deny intuition in founding mathematical and scientific thought. For example, in the recent comprehensive book Cassirer, Samantha Matherne describes the Marburg school as "intellectualist." In the concept/intuition distinction in Kant, the neo-Kantians are described as absorbing intuition into conceptual thought. The discoveries of non-Euclidean geometry, Relativity Theory, and Quantum Mechanics seem to confirm that intuition-at least in mathematics and the physical sciences-is obsolete in founding knowledge in these domains.Yet Ernst Cassirer-the most well-known of the neo-Kantians in the Marburg school-seems to endorse Life as the foundation of the dynamic 'form' construction characteristic of the sciences (as well as nature writ large). And his metaphysics of Life seems to be an about-face on his former stance on intuition. For- in his unpublished fourth volume of The Philosophy of Symbolic Forms (PSF IV)- there is more agreement than disagreement with the intuition-hailing Bergson and the Lebensphilosophie movement. What happened here? Did Cassirer betray the Marburg denial of intuition? Or was Cassirer consistent, denying one type of intuition and endorsing another?To answer this question, one must attempt to unite Cassirer's metaphysical comments on Life with his work on mathematics and science. This will be the focus of my talk. This is even more pressing when one analyzes Cassirer's later remarks on intuitionism: He seems to endorse a species of intuitionism in a broad sense in his late fourth volume of Problem of Knowledge. This, again, seems to conflict with his critique of the intuitionism of Herrmann Weyl and L.E.J. Brouwer in The Philosophy of Symbolic Forms, Volume Three (PSF III). Nevertheless, even in that book Cassirer emphasizes the temporality and becoming of mathematical thought as it constantly refines its foundations. Could this be an endorsement of intuition in a more expansive sense?I will argue that these questions are resolved if instead of thinking of intuition wholesale, we divide it into separate kinds; some of which Cassirer endorses, some of which he denies. The kind of intuition that emphasizes wholeness prior to parts, continuity over discreteness, becoming over being, and indeterminacy over determinacy is something that Cassirer never denies. The kind of intuition dealing with empirical perception, psychological immediacy, and sense certainty are something that Cassirer has always denied. I will contend that in the latter part of his career, Cassirer begins to read intuitionism in terms of the more favorable kind of intuition. And this provides the key to understanding both Cassirer's endorsement of intuitionism and the emphasis on Life in his metaphysics.

Spigola, Benedetta

Axiomatization as a Symbolic Activity: Hilbert's Program and Its Philosophical Import

Abstract: In this paper I will focus on Hilbert's axiomatic program in the light of the notion of symbol in order to argue that: (i) Hilbert's theory of symbol can be profitably read in the light of the re-conceptualization of the problem of representationality in mathematics and physics at the beginning of the 20th century; (ii) Hilbert's program bestows considerable importance on the notion of symbol both in the foundation of mathematics and in the epistemological reflections on physics. At the beginning of the 20th century, the question of representation in physics and mathematics was crucial in the philosophical debate on scientific knowledge. This was essentially due to what Ernst Cassirer defines as the "crisis of intuition" (Krise der Anschauung) being brought forth by the new way in which modern physics conceived of the notion of 'physical state', as well as by the advent of geometrical systems equiconsistent with Euclidean geometry. 'Intuition' is indeed closely related to the semantic field of concepts having visual connotations. Once 'visualization' is undermined, concepts of mathematics and physics risk being deprived of representational content. However, as Cassirer intelligently suggested, the crisis of intuition should not make us renounce representationality in mathematics and physics, but rather completely revise the way in which representationality is explained. I believe that Hilbert's program is in line with the idea that we should re-think the idea of representationality by way of clarifying the idea of mathematical symbolism (being used purely mathematically or in physical knowledge).I will argue that Hilbert's formalistic turn significantly contributed to disentangling the validity of mathematics and physics from intuitional evidence. Objects being described mathematically must no longer be exhibited in intuition (be 'intuition' explained in Kantian or intuitionistic terms). It is worth noting that this idea also applies to physical knowledge: for Hilbert, physical concepts and laws have no representational content to be filled intuitionally (cf. Hilbert 1967, p. 475). The representational content borne by a formula of a physical theory is sufficiently explainable in terms of the mathematical language that is used in that theory. Elsewhere (cf. Hilbert 1996b, pp. 1121f.), Hilbert again makes it explicit that the object of the number theory is the 'sign itself' and that what matters is 'its shape' (Gestalt; cf. Hilbert 1996a, p. 1100) whereby mathematics and physics have their proper foundation.I will provide a non-reductivist interpretation of Hilbert's claim by showing that the symbolic notation has for him two related functions: (1) a pragmatical-operational function, according to which the symbol allows for an overview of the complexly structured chain of thoughts being involved in mathematical and physical reasoning; (2) an ideal-representational function, according to which the symbol is the expression of the systematic body of structures and procedures whereby a theoretic object is built. I will defend the idea that Hilbert is anti-reductivist and more than simply an instrumentalist.Keyword: Hilbert; Axiomatics; Intuition; Symbol; Representation.

Stemeroff, Noah

Pauli on Plato

Abstract: Wolfgang Pauli is considered to be one of the greatest physicists of the 20th century. His critical insight was legendary and his judgement unassailable. Indeed, Pauli was long held as the living ` conscience of physics'. However, this high acclaim has not extended to Pauli's philosophical thought. The criticism of Pauli's philosophy is often tied to his collaboration with Carl Jung on Jung's analytic psychology. But Pauli's historical and philosophical studies spanned over two millennia in the history of ideas, and went far beyond the confines of Jungian psychology, to the very foundations of thought and reality. In this paper, I will explore some of the lesser known aspects of Pauli's later philosophical thought concerning the nature of complementarity and the relation between mind and nature. In particular, I focus on Pauli's defence of a form of Platonism, as it emerges from his broader correspondence in the late 40s and early 50s. For Pauli, Plato represents not only the progenitor of a distinct philosophy, but also a key stage in the historical development of philosophical thought. The rich interplay between Pauli's reading of Plato and the history of Platonism sets the course for his understanding of the expression of structural (or archetypal) forms of thought throughout the history of ideas. This expression is guided by what Pauli terms a cosmic harmony which underwrites both our understanding of nature and its physical form. Through a discussion of Pauli's Platonism and his appeal to a cosmic harmony in nature, I will outline the contours of his later philosophical thought, and his hopes for future physics.

Stöltzner, Michael

Metatheoretical Virtues in the Axiomatization of Physics

Abstract: The metatheoretical debates of the 1920s and 1930s were shaped by Hilbert's Foundations of Geometry, which the Sixth Problem had declared the model for the axiomatization of physics, and the axiomatization of set theory. Most philosophically challenging was, in the former case, the completeness axiom because it ranged over possible objects not fully specified in the other axioms. In the latter case, the emergence of unintended and non-isomorphic models left a mark of irreality upon the axiomatized theory – as von Neumann put it. Thus, it seems historically plausible to view the no-hidden-variable theorem of the Mathematical Foundations of Quantum Mechanics as motivated by the goal of (semantic) completeness and the Stone-von-Neumann theorem as establishing the categoricity of quantum mechanics.Von Neumann himself was well aware that these metatheoretical concepts were still in flux and posed intricate logical problems. Within Logical Empiricism both were sufficiently attractive that Carnap mistakenly believed to have shown their equivalence and developed a general metatheory of individual axiom systems. On the one hand, Logical Empiricists neatly separated physical theory, its mathematization, and the latter's formal-logical analysis. This allowed the mathematician to explore an axiom system in the face of physical theory, even if this temporarily expanded mathematical ontology. Categoricity, on the other hand, seemed essential to define truth – as Schlick had done – as the uniqueness of coordination between theoretical symbols and empirical observations; after all there was little conceptual space between the logical and the empirical to allow separating the physical model from the non-intended ones. Completeness and categoricity were thus metatheoretical quality criteria for an axiom system that had to be investigated by logical means.Today, the program of Logical Empiricism has largely lost its attraction, logical and model theoretic investigations have prospered independently of physics, and neither theorem of von Neumann has prevented the rise of alternative interpretations. For entirely different reasons, von Neumann quickly moved on from Hilbert spaces to operator algebras that are playing a key role in present mathematical physics. Are both metatheoretical motives still relevant quality criteria in the context of quantum physics? The claim of my paper is that they have assumed a role similar to the virtues of scientific theories. As Kuhn has argued, virtues like scope, simplicity, and consistency are contextual and often pull in different directions. This does not mean that they cannot be explicated by means of formal metatheories and weighed with other more pragmatic virtues. Applying this analogy allows one to admit that categoricity,properly explicated, refers to a specific feature of bounded operators in Hilbert space while in quantum field theory inequivalent representations abound and, during the renaissance of mathematical physics in the 1950s and 1960s, it was wise for top-down axiomatic searches of physical models not to be guided by categoricity.

Stoppelkamp, Bastian

The Blooming Landscapes of Logic: Heinrich Gomperz as an intermediary between empiriocriticism and logical empiricism.

Abstract: The empiricism of Ernst Mach and Richard Avenarius is commonly seen as a core influence on the Vienna Circle. An important intermediary in this context was the Viennese philosopher Heinrich Gomperz (1873-1942), who in the time around 1900 played a major role in the dissemination of empiriocriticism in Austria: He was among Mach's first doctoral students in Vienna and later worked as a colleague of Avenarius in Zurich. In the 1920s, after gaining a professorship at his alma mater, Gomperz grew into a close ally of the Vienna Circle. Besides contributing to several projects of logical empiricism, he established his own discussion group – the "Gomperzkreis" – with personnel overlaps to the Circle meetings. In my talk I will exemplify Gomperz intermediary role by looking at his early opus magnum Weltanschauungslehre. The work was originally conceived as an entire book-series, from which only two volumes eventually got published. The first volume on "methodology" came out in 1905; the second on "semantics" (Semasiologie) followed in 1908. Although the Weltanschaungslehre was scarcely received at that time, it later – in the 1920s and 1930s – got a lot of attention, especially in the broader vicinity of the Vienna Circle. This recognition can be traced back to three interconnected aspects, I will address in my talk: (1) a scientific meta-philosophy, (2) an empiricist methodology, (3) a proto-causal theory of meaning. (1) Similar to Mach and Avenarius, Gomperz conceives philosophy not as a foundational but as a "secondary science". Its main purpose is to solve conceptual/meta-theoretical disputes between different scientific disciplines in order to facilitate a unifying worldview. This idea comes with an anti-metaphysical twist: To count as a meaningful solution every philosophical meta-theory must in principle be verifiable by scientific procedures, which – as acknowledged by Karl Popper – could be seen as a proto-version of the "meaning criteria".(2) Gomperz Weltanschauungslehre was meant as a contribution to the then ongoing psychologism-debate. In the quarrels between logical and psychological theories of cognition and language he was trying to come up with a solution, that was compatible for both disciplines without falling into the pitfalls of metaphysics or traditional sensualism. For Gomperz, "noetic objects" like concepts or propositions could be explained in a empiricist way by including the affective/reactive side of experience in its explanatory repertoire – an option later discussed in Carnap's Aufbau. (3) Applying his affective version of empiricism (Pathempirismus) to logic and psychology, Gomperz developed a theory of linguistic and non-linguistic representation. According to Gomperz, a sign S (word or picture) means/represents an object O for a person P, when S, as a stimulus, has (partially) the same cognitive, emotional or behavioral effects on P as would belong to O. This idea marked an important step from earlier physiological to later causal theories of meaning picked up by several disciplines: like the semantics of Karl Bühler and Ogden/Richards or the art theory of Ernst Gombrich.

Storni, Marco

Optics, Cosmology, and the Scope of Teleological Explanations: Maupertuis's Principle of Least Action Reconsidered

Abstract: In a paper published in 1744 (Accord de différentes lois de la nature qui avaient jusqu'ici paru incompatibles) Pierre-Louis Moreau de Maupertuis (1698–1759) provides the first formulation of one of the fundamental principles of classical mechanics, namely the principle of least action (PLA). Until then, Maupertuis was best known as the first supporter of Newtonian theories in France, and a fierce defender of Lockean-inspired radical empiricism, which he never disavowed. In the 1744 paper, Maupertuis explains that, while the first two laws of geometrical optics – 1. light, in a uniform medium, travels in a straight line, and 2. the angle of incidence is equal to the angle of reflection (the "law of reflection") – are philosophically intelligible because they have obvious mechanical analogies (inertia and elastic collision), the same does not apply to the law of refraction or "Snell-Descartes law." This law states that, for all refraction, the ratio between the sine of the angle of incidence and the sine of the angle of refraction is a constant determined by the media through which light passes. The behavior of refracted light leads Maupertuis to affirm that "since light cannot go both by the shortest path and by the fastest one [...] it takes a path that has a more real advantage: the path it takes is that by which the quantity of action is the least" (Maupertuis 1744, p. 423). The quantity of action is defined as the sum of the distances traveled by a body, each multiplied by the speed of the body as it passes through. In later writings, Maupertuis explores the metaphysical implications of PLA, generalizing it from optics to any change that occurs in nature. In a 1752 text (Lettres), he states that PLA is a "universal law of nature" on which all other laws of nature, including the law of gravitational attraction, depend. In a 1746 paper, Les lois du mouvement et du repos déduites d'un principe métaphysique, then incorporated into the Essai de cosmologie (1750), Maupertuis presents PLA as the chief example of the rationality and economy of nature, and uses it to construct a proof of the existence of God as the intelligent designer of the cosmos. In my talk, I will discuss two key issues that are raised by Maupertuis's attempt to generalize PLA from optics to cosmology, and make it a general metaphysical principle that shows the teleology of nature, vis-à-vis the empiricist approach that he adopts in his philosophy: 1. What certainty can Maupertuis claim for his general metaphysical statements? 2. More generally, how can he demonstrate the existence of a teleological structure in nature on the basis of a physical principle that works mechanically? To address these questions, I draw a parallel with two Leibnizian texts on optics and metaphysics, the Unicum opticae, catoptricae, et dioptricae principium (1682) and the Tentamen anagogicum (1696, but unpublished until the nineteenth century), since this confrontation sheds light on the strengths and weaknesses of Maupertuis's philosophical endeavor.

Straetmanns, Vera

The Lady and the Plants: Two Notions of Teleology in Agnes Arber's Philosophy of Plants

Abstract: @page { size: 21.59cm 27.94cm; margin: 2cm } p { color: #000000; line-height: 115%; text-align: left; orphans: 2; widows: 2; margin-bottom: 0.25cm; direction: ltr; background: transparent } p.western { font-family: "Calibri", serif; font-size: 12pt; so-language: de-DE } p.cjk { font-family: "Calibri"; font-size: 12pt; so-language: en-US } p.ctl { font-family: ; font-size: 12pt; so-language: ar-SA } This paper discusses Agnes Arber's mid-20th century work on teleology in plants. Arber (1879-1960) was a today largely-forgotten British plant morphologist, historian of botany and philosopher of biology. She authored over 90 scientific papers and wrote several books of which especially the last three (The Natural Philosophy of Plant Form, The Mind and the Eye and The Manifold and the One) transcend her previous biological research and combine historical analyses, philosophical insights, theoretical considerations and detailed descriptions of plant morphology. Arber was convinced of the "vital necessity of a linkage between morphological and philosophical thought". Through the philosophical treatment of problems in the plant morphology, she hoped to find the means for a "synthesis of various theories, that are, from the standpoint of analytical science, irreconcilable".I will, first, discuss Arber's partial-shoot theory of the leaf that she developed in her 1950 book The Natural Philosophy of Plant Form and point out how it is informed by Arber's unique standpoint as a morphologist, historian and philosopher. Second, I focus on Arber's treatment of the problem of teleological explanations in biology. In her partial-shoot theory, Arber postulates an urge in the leaves of plants to develop whole-shoot features. This allows her to explain different morphological phenomena. I argue that the teleological notion in this theory and Arber's morphology in general is one of two different notions of teleology that can be traced in Arber's work. I call it formal teleology and contrast it to Arber's second notion: final teleology. Towards the second notion, which expresses the idea of evolutionary adaptation, Arber shows a reserved and sceptical attitude, whereas she is very sympathetic of formal teleology, basing her morphological argumentation on it.Finally, I take a step back and consider Arber's role in and impact on early philosophy of biology. Also, I propose avenues through which Arber's dialectical and pluralist mode of thinking and her innovative theory of teleology and purposiveness in plants can provide an inspiring outlook on teleological debates today.

Strauss, Martin

Forgetting Merton. Reexamining a Process of Marginalization in the Philosophy, History and Sociology of Science

Abstract: Robert K. Merton – arguably the founder of the sociology of science in the 1930s – is nowadays almost absent from debates in the history and philosophy of science. A rough survey of citations in some of the main journals of the field (e.g. Studies in History and Philosophy of Science, Philosophy of Science, Social Studies of Science, Science in Context, HOPOS) shows that most cited today are the various currents of so-called "new sociology of science" (e.g. Knorr Cetina 1991), which, from the 1970s onwards, defined themselves explicitly against Mertonian sociology of science. This paper offers a description, analysis and re-evaluation of the marginalization of Merton in the philosophy, history and sociology of science.In the first part, I provide an empirical description of this process using some simple quantitative indicators of citations in journals, books and reference works in the fields involved. I explore here the hypothesis that Merton fell prey to a negative "Matthew effect" (Merton [1968] 1973; 1988). In the second part, I map the main arguments used against Mertonian sociology between the 1970s and 1990s. Prominent issues here were the normative structure, peculiarity and autonomy of science (Barnes and Dolby 1970; Stehr 1978); the difference between Merton and sociological interpretations of Kuhn (Pinch [1982] 1997); the internalism-externalism debate and the sociological account of scientific "content" (Bloor 1973; Shapin 1988; 1992; Kaiser 1998); the relationship between sociology and philosophy of science (Knorr Cetina 1991); and issues in the epistemology and methodology of the social sciences involving the dismissal of "functionalism" or "sociology" altogether (Latour 1988; 2005; Lynch 1993). The various protagonists of the "new sociology of science" took here sensibly different stances regarding Merton, reaching from qualified criticism (in the case of the "strong programme", e.g. Barnes 2007) to outright dismissal or non-citation (in the case of Latour). These differences appeared also in the explicit controversies on Merton's relevance launched by some of his defenders (Storer 1973; Gieryn 1982).After this descriptive effort, I re-evaluate in the third part some of the main arguments against Mertonian sociology of science. I aim here at a nuanced view of both Merton's position and its various critics. While some arguments against Merton – e.g. regarding the autonomy of science, the neglect of the "contents" of science or the lack of "reflexivity" – miss their target, other issues – e.g. the status of the norms of science in relation to scientists' behavior – remain problematic (Bourdieu 1990; 2001; Shinn and Ragouet 2005). Finally, I offer two sets of conclusions. Regarding the sociohistorical interpretation of Merton's marginalization, I argue that this was the result of two concurrent trends: the dismissal of "positivism" in the philosophy of science and that of "functionalism" in the social sciences. Regarding the re-evaluation of the reasons for this marginalization, I argue that some of the tenets of Merton's sociology of science remain intact and suggest a

different, potentially still interesting model for the collaboration of sociology and philosophy of science.

Suárez Pascal, C. David

N. R. Hanson's Pragmatic Theory of Observation

Abstract: It is well known among current philosophers of science that Hanson was a strong proponent of the theory-ladenness of observation, which influenced directly or indirectly many other authors in the field. What does not seem to be so well known is Hanson's (1962) argument of the indispensability of theory-ladeness for scientific activity, which is sufficient reason to question whether Hanson's thesis about theory-ladeness is really well understood. Indeed, both past (see, for instance, Feyerabend(1960; 1962)) and recent (for example, Short (2007)) depictions of Hanson's views on this issue seem to be deeply misguided. Opposed to what some authors assert, Hanson's proposal is neither naive, nor intended to neglect the crucial role of experience in science. To the contrary, both in his Patterns of Discovery and in subsequent writings (1972, 2018), Hanson's efforts are clearly directed at building an original theory of science which shows how it is possible (Lund 2010). In this paper, I will examine Hanson's conception of the theory-ladenness of observation and will argue that it has been widely misunderstood both by adopters and detractors. Against common misconceptions, Hanson offers a pragmatic theory of observation which is very akin to Charles S. Peirce's views on experience, but which is also strongly connected to his own views on explanation and discovery. As will be argued, Hanson's theses regarding the latter issues are not only dependent on his pragmatic theory of observation, but also relevant to contemporary debates on causality and realism.

Suárez, Mauricio

James Clerk Maxwell on Analogy: A Philosophical History

Abstract: Philosophers and historians have long been fascinated by the uses of analogical reasoning in Victorian science – and James Clerk Maxwell's in particular. One could even speak by now of a history of the philosophy of Maxwellian analogy, which starts with Pierre Duhem's and Norman Campbell's critical analyses in the early 20th century, continues with seminal contributions by the likes of Joseph Turner (1955), Robert Kargon (1969), and Mary Hesse (1961, 1973), and eventually gives rise to the large contemporary literature devoted to Maxwell's modeling methodology (see Suárez 2024 for a review). The first aim of this talk is to characterize this history very briefly as an attempt to get to grips with analogy as a relation between representational sources and their targets. The earlier reification of analogy is an implausible viewpoint to understand the Victorian scientists' innovative modeling methodologies, and it gives way in the more recent literature to a more fertile vision of analogical reasoning as a dynamical modeling activity. There is some evidence that this 'modelling attitude', which places analogical reasoning at the core of modeling practice, was inaugurated by Maxwell himself. I then focus on Maxwell's notorious (1860/1890) derivation of the statistical distribution that bears his name – as well as Boltzmann's – which I present as a very early case of model transfer across domains driven by a dynamical analogy. It is sometimes acknowledged (Gyenis, 2017; Harman, 1998) that Maxwell picked up his assumptions regarding the overall symmetries of the probability distribution from John Herschel's (1850/2014) magnificent review of Quetelet's work. What is not always appreciated is that Herschel's assumptions were the application of his broadly inductive methodology (1830/1986) to experimental errors in the observation of astronomical phenomena. Herschel was of course enormously well-known, and a towering influence on Maxwell and other scientists at the time (Snyder, 2011). What led Maxwell to apply Herschel's error distribution to the theory of gases, I argue, were distinctly Scottish emphases on cross disciplinary discovery, and on the knowledge-generating role of geometrical analogies.

Suárez, Mauricio

Analogical Reasoning in James Clerk Maxwell: A Philosophical History

Abstract: Philosophers and historians have long been fascinated by the uses of analogical reasoning in Victorian science – and James Clerk Maxwell's in particular. One could even speak of a history of the philosophy of Maxwellian analogy, which starts with Pierre Duhem and Norman Campbell's critical analyses in the early 20th century, if not earlier. This history continues with seminal contributions by the likes of Joseph Turner, Robert Kargon, and Mary Hesse in the second half of the 20th century. It eventually reaches a climax in the large contemporary literature devoted to Maxwell's modeling methodology (see Suárez 2024 for a review). The first aim of this talk is to characterize this history very briefly as an attempt to get to grips with analogy as a relation between representational sources and their targets. The earlier reification of analogy is an implausible viewpoint on the Victorian scientists' innovative modeling methodologies, and it gives way in the more recent literature to a more fertile vision of analogical reasoning as a dynamical modeling activity. There is some evidence that this 'modelling attitude', which places analogical reasoning at the core of modeling practice, was inaugurated by Maxwell himself. I then focus on Maxwell's notorious (1860/1890) derivation of the statistical distribution that bears his name – as well as Boltzmann's – which I present as a very early case of model transfer across domains driven by analogy. It is sometimes acknowledged (Gyenis, 2017; Harman, 1998) that Maxwell picked up his assumptions regarding the overall symmetries of the probability distribution from John Herschel's (1850/2014) magnificent review of Quetelet's work. What is not always appreciated is that Herschel's assumptions were the application of his broadly inductive methodology (1830/1986) to experimental errors in the observation of astronomical phenomena. Herschel was of course enormously well-known, and a towering influence on Maxwell and other scientists at the time (Snyder, 2011). What led Maxwell to apply Herschel's error distribution to the theory of gases, I argue, were distinctly Scottish emphases on cross disciplinary discovery, and on the knowledge-generating role of geometrical analogies.References:Gyenis, B. 2017. 'Maxwell and the Normal Distribution: A Coloured Story of Probability,Independence, and Tendency toward Equilibrium'. Studies in History and Philosophy ofModern Physics 57, 53–65.Harman, P. 1998. The Natural Philosophy of James Clerk Maxwell. Cambridge: Cambridge University Press.Herschel, J. 1830 / 1986. A Preliminary Discourse on the Study of Natural Philosophy. Chicago: The University of Chicago Press, with a foreword by Arthur Fine._____. 1850/2014. 'Quetelet on Probabilities', in John Herschel: Essays from the Edinburghand Quarterly Reviews with Addresses and Other Pieces, 365–465. Cambridge: CambridgeUniversity Press.Maxwell, J. C. 1860/1890. "Illustrations of the Dynamical Theory of Gases, Parts 1 and 2." In The Scientific Papers of James Clerk Maxwell, ed. William Davidson Niven, 1:377–409.Snyder, L.

2011. The Philosophical Breakfast Club. Broadway books.Suárez, M. 2024. Inference and Representation: A Study in Modeling Science. Chicago: University of Chicago Press.

Subotić, Vanja

Neuroconnectionism Before It Was Cool: On the Overlooked Moments in the History of Neural Networks

Abstract: In a recent manifesto, Doerig et al. (2023) argue that applying deep neural networks in cognitive neuroscience should be regarded as constituting the neuroconnectionist research program. Clearly marking the legacy of connectionism in the 1980s cognitive science, when shallow neural networks were set forth like game changers, neuroconnectionism is a research program whose core is the computational modeling that implements artificial neural networks mimicking some aspects of the biological brain to solve a cognitive task. The canonical history of artificial neural networks, as endorsed in philosophy, mind sciences, and AI research (see, e.g., Buckner & Garson 2018, Berkeley 2019, Boden 2006, Mitchell 2019) usually goes as follows. Rosenblatt (1958) pioneered three-layered artificial neural networks, embodied in the so-called Mark I Perceptron machine. They were supposed to learn patterns from data with little handwired gear, thus suggesting that human cognition could do with almost no innate machinery given that many cognitive processes would be seen as originating from experience. Alas, in a co-authored book with Papert, Minsky (1969) refuted Rosenblatt's tentative conclusions based on the Perceptron machine's apparent success by formulating mathematical proof that such a machine is not capable of processing exclusive disjunction. The outcome of the Minsky vs. Rosenblatt dispute was detrimental to early connectionism: the first AI winter swept away the promise of the new revolution in both AI and psychology. The majority of projects based on neural networks lost funding in favor of symbolic AI, which became the backbone of the newly emerging field of cognitive science in the 1970s. Only in the 1980s, with the publication of the so-called PDP Bible (Rumelhart, McClelland & the PDP Research Group 1986), connectionism (or parallel distributed processing as it was called back then) developed enough to represent a revolutionary way of modeling cognitive processes and has been competing with symbolic models ever since resulting in the nowadays neuroconnectionism.My aim is to draw your attention to the often-overlooked moments in the history of artificial neural networks to make a case for two points. First, the first models were present already in the 1950s and were as noteworthy as Rosenblatt's. For this reason, the early AI movement and psychology almost went connectionist from the very start. Oliver Selfridge's Pandemonium (1958) was designed to account for image constancy in a biologically plausible way by postulating independent feature or letter detectors that are parallelly connected, amusingly named data demons, computation demons, and cognitive demons. Independently from Rosenblatt, psychologists wrote textbooks from the perspective of pattern-based cognition (see Neisser 1967) and a number of AI engineers worked on pattern classification through the conception of machines that "learn to learn" such as Nilsson (1965) and Ivakhnenko & Lapa (1965). Second, the halt in development actually never happened in neuroscience, so the revolution of the 1980s did not occur out of

the blue. The most influential learning algorithm for connectionism, namely backpropagation, was introduced in the 1970s and was intertwined with research in neuroscience (Werbos 1974, Anderson & Rosenfeld 2000).

Sznajder, Marta

Janina Hosiasson-Lindenbaum: building an international career in scientific philosophy

Abstract: In the history of philosophical groups and movements, the Lvov-Warsaw School is praised for the high proportion of women among its members, especially in the decades between the World Wars. The challenges and opportunities presented by the academic environment that these women existed in, however, is not well-explored. While there were no formal obstacles to women's academic careers in Poland at the time, cultural and economic factors still contributed to an unwelcoming environment for female scholars. This environment shaped women's careers, and as a result, influenced the way their work was taken up by their respective academic communities. In this paper, I show the working of these mechanisms in the case of Janina Hosiasson-Lindenbaum (1899–1899).Hosiasson-Lindenbaum never held a (paid) university post; yet, she was a well-published philosopher of probability and inductive logician, and an active participant in the broader community of logical empiricists. I will present the ways she forged an academic career for herself against the wider background of the academic spheres that she was interacting with: the philosophy and psychology community in Warsaw, as well as the international movement of scientific philosophy. In response to her domestic situation as a young, female, Jewish philosopher, Hosiasson-Lindenbaum developed and applied various approaches and strategies in order to be able to continue her philosophical work and be recognised as an expert in her field. One of these strategies was to connect with the international philosophical community. She did this by arranging long visits at universities abroad (Cambridge, Vienna, Paris), maintaining contact with leading and emerging scientific philosophers (Reichenbach, Carnap, Hempel, Nagel), and presenting at international congresses and conferences. These efforts, however, were also fraught with challenges. Her attempts at forging meaningful research connections with other philosophers appear stifled by not only the fact that she was a woman, but a woman from a non-western European country, with an imperfect command of the English language. Finally, I show how all the above factors influenced Hosiasson-Lindenbaum's attempts to secure research funding as a refugee scholar during World War II. The circumstances and mechanisms described in the paper influenced not only Hosiasson-Lindenbaum's career while she was alive, but subsequently contributed to the lasting reception of her work in Poland and internationally. I discuss the recognition herself and her work received among the (migrated) logical empiricists in the United States, as well as her-largely overlooked-place in the development of axiomatic confirmation theory and inductive logic in the 1940s.

Tamas Tuboly, Adam

Sociology of Science in Logical Empiricism: Frank's Research Group at the Institute for the Unity of Science

Abstract: If one field is still less associated with logical empiricism, it is sociology, especially sociology of knowledge and science. Although Otto Neurath's affinity with the social sciences (economics and sociology) are obviously known, it is often conceived either as a detour, or a personal interest, not having much to do with positivism itself. Nonetheless, recent attempts to incorporate Philipp Frank into the canon of history of logical empiricism and the history of philosophy of science have shown that even bigger, more comprehensive, and institutionalized efforts were made within logical empiricism to embrace and integrate sociology of science within the movement. After the establishment of Frank's Institute for the Unity of Science in Cambridge, MA in 1947, one of the most prestigious research groups there was concerned with various aspects of sociology of science. Frank did everything he could to assemble a group of leading sociologists, historians, and philosophers to shape the new discipline (that was just about to unfold in the States during the early 1950s); after many negotiations, he was able to put together a research program to circulate that included Ernest Nagel, Robert K. Merton, Thomas Kuhn, and later Ernst Topitsch, Lewis Feuer, and numerous other scholars. Though the group was not able to leave a mark as such on the intellectual history of sociology, but back in the day they were quite influential on various grounds: by preparing the first bibliography of sociology of science, organizing workshops, providing significant funding to various scholars in need, Frank and the group at the Institute was known for many as an important body of scholars with substantive institutional background. By uncovering all the work that has been done within the group and by individual members of the group after the Institute was dissolve (but having the cognitive roots of their papers or books within Frank's research proposal) helps us to provide a novel estimation of how sociology of science was integrated into twentieth-century positivism at a time when technical philosophy of science was on its exclusive rise. The talk is an excerpt from our forthcoming major intellectual biography of Philipp Frank (written with George Reisch) and is based on our years-long extensive archival research (with materials from the AAAS, Institute for the Unity of Science, Rockefeller Foundations, and individual archives of Merton, Barber, Nagel, Frank, and Kuhn).

Tejedor, Chon

‘Conditions, Form and the Net Metaphor in Wittgenstein's Tractatus’

Abstract: In this paper, I examine Wittgenstein's fascinating discussion of mechanics in the Tractatus Logico-Philosophicus, by focusing on his use of the net metaphor (TLP 6.341 – 6.343). Several notions are at play in these remarks: the notion of world or reality; the notion of a picture of the world (the original black and white iconic surface – let us call it AP) that provides a description of the world (let us call this AD); the notion of a mesh being placed on picture AP; the notion of the further picture that results from placing such a mesh (in Wittgenstein's example, a picture made up of black squares and white squares – let us call it BP), which provides a mesh-generated description of AP (let us call this mesh-generated description BD). Wittgenstein uses this complex metaphor to explore the internal relations between the principles of mechanics, which he considers to be both a priori and optional, and reality understood both as the totality of facts and as the set of conditions in the light of which we move from one system of mechanics to another following pragmatic considerations. The net metaphor plays on the idea of obscuring (in the sense of rendering less visible) certain possibilities so as to highlight others (i.e., those that remain visible through that particular net), thereby rendering them significant within a particular representational system. Wittgenstein regards this idea of obscuring certain possibilities so as to highlight others as central to the notion of form – that is, to the possibility of senseful representation - in mechanics. One of the aims of Wittgenstein's discussion is to defend an understanding of mechanics devoid of distorting metaphysics and in line with what he regards as scientific practice. In particular, Wittgenstein wishes to expose as nonsensical the view that causation, in mechanics, involves material, non-analytic necessity. He suggests that this understanding of causation, far from capturing genuine aspects of scientific practice, is a self-subverting metaphysical construct that collapses into nonsense. In order to shed light on the principles of mechanics, we need to set aside this view of causation – a view born out of entrenched philosophical scientism – and focus instead on the net metaphor.

Tho, Tzuchien

Locality and symmetry in the emergence of Lagrangian analytical mechanics

Abstract: Due to W.R. Hamilton, Lagrangian mechanics is often historically interpreted in a way that highlights the evolution of the least action principle (or, more accurately stationary action) due to the method of optimisation involved in mature analytical mechanics. This characterisation is indeed true for the historical developments of analytical mechanics before Lagrange (Maupertuis, Euler, et. al.) and after Lagrange (Hamilton, et. al.) but not true for the mature Lagrange who was very keen to distance his work from teleology. One of the contextual problems here is the implication of the least action principle teleology as a non-local cause. This paper therefore focuses on the method of virtual velocities which Lagrange used to replace the least action methods of his predecessors and examine the reestablishment of local causality that his implies. In particular, I focus on how this causal theory implied by Lagrange's methods challenges not only teleological theories but also reductive understandings of the Newtonian mechanical one. The paper begins by contextualising Lagrange's method of virtual velocities within the periods of his writing focusing on his 1764 work on the Moon's libration, borrowing from Fraser's (1985) work. We then move to contextualising these methods within the foundations of analytical mechanics, identifying the theoretical development of virtual velocity and virtual work methods that had come before. By doing this, we develop a sketch of the metaphysical assumptions of the method and use this to assess the deep differences between Lagrange's innovations and the standard understanding of the least (stationary) action principle. We finally make a case for a Lagrangian theory of causation that is local but not mechanical in the usual sense. This offers a unique causal concept that is structural but not teleological as his near predecessors would have it. The reexamination of the foundations of analytical mechanics will have significant concepts to offer contemporary philosophy of physics.

Tomasson, Julia

Averroes vs. Ibn Rushd in the ‘Quaestio de certitudine mathemaricarum’

Abstract: The so-called 'Quaestio de certitudine mathematicarum' debate of the sixteenth and seventeenth centuries is a rich site of inquiry for those interested in the early modern exact sciences, the history of mathematical proof, and the cultural history of mathematics more broadly. In the Quaestio, mathematicians-mostly associated with the University of Padua-argued whether the discipline of mathematics could claim that mathematical proofs are 'certain' in an Aristotelian logical framework, and if not, what types of proofs ought mathematicians to use to ensure certainty. While this debate is framed as a reckoning with Aristotle, the first citation in the first sentence of the first tract of the debate (Piccolomini, 1547) was not to Aristotle but to the Muslim polymath Averroes (Latin name for Ibn Rushd, 1126-1198 CE). Moreover, much of the debate is a discussion of Latin commentaries on Averroes' interpretations of Aristotle. Is this just a philosophical or rhetorical connection? Many Ottoman scholars were training at the University of Padua at this time. How did this affect the culture and practice of mathematical learning, teaching, and research? Were there similar debates happening in Arabic commentaries of Ibn Rushd's/Averroes' work on mathematics and philosophy of mathematics that we might include in a more robust history of the Quaestio? How do the Latin traditions of Averroes' work compare to the Arabic traditions of Ibn Rushd?

Uçan, Timur

Fractals, Isomorphisms, Events, Methodology

Abstract: @page { size: 21cm 29.7cm; margin: 2cm } p { line-height: 115%; margin-bottom: 0.25cm; background: transparent } With the development of fractal geometry in the XXth century, usually unrelated objects, structures and events became relevantly relatable in unpredicted ways: precise and measurable correlations have been established between events as diverse as cloud-formations, plant-formations, organ-developments, stock market crashes, demographical evolutions. How did such development of geometry rendered possible such beneficial change of our world-conceptions? Does not such development present both historical and philosophical importance for social epistemology, philosophy of science, ecology, analytic philosophy, and early criticisms of traditional phenomenology? How to account for and evaluate the philosophical and historical importance of such change? With this paper, I propose a study of the epistemological difficulties related to the Euclidean conception of geometry whose resolutions have been rendered possible by the development of non-Euclidean geometries, notably of fractal geometry, to render manifest the natural importance of fractal geometry for social sciences. Indeed, epistemological difficulties related to the Euclidean conception of geometry have been criticized and addressed by philosophers of often unrelated traditions, as by Sartre and the early phenomenological tradition, and Wittgenstein and the early analytic tradition. Sartre criticized since his early works the difficulties raised by a form of unsocial abstractionism involved by some conceptions of Newtonian physics on the basis of Euclidean geometry, that of the "desert world" or "world without humans" (L'être et le néant, 346). Wittgenstein also criticized such misleading idealization in the 1914-1916 Notebooks. Both philosophers criticized the comprehensiveness of would-be attempts to reconstrue the intelligibility of relations, and peculiarly of human relations, on the basis of idealized Euclidean geometry and space. Wittgenstein's criticism of the substantial conception or substrate-based-conception of the proposition or phrase thusly achieves a philosophical disentanglement from the philosophical difficulties involved by the Euclidean conception of space, understandable as such against the background of new possibilities opened up by new conceptions of geometry (Tractatus, 5.5423).New philosophical and epistemological difficulties thusly emerged with new conceptions of geometries, and advances in physics based upon or involving such advances. The diagnosis of the eventual problematicity of the disconnexion of sensorial experience and abstractions was expressed and addressed in epistemology and philosophy of science during the XXth century (Lewis, 1956; Friedman, 2001). And the affirmation that perception is fractal came to be expressed (Imbert, 1952; Noë 2004). But how to exactly account and understand such claim? One central line of argument to explicit such claim is that the notion of fractal renders conceivable to think internal relations, and to rethink the distinction of the internal and the external in stronger ways, as manifested by the structural similarity of trees and lungs, whose productions mutually satisfy one another. Such reconsideration, of isomorphism, I will

argue, can serve not only to understand better the history and prospects of early analytic philosophy, but also to account without universalism and naturalism for the natural importance of fractal geometry for social sciences.

Uebel, Thomas

Carnap and the Indeterminacy of Translation

Abstract: The argument for the indeterminacy of translation (Quine 1960) has long been regarded to play a central role in the dialectic between Quine and Carnap over, among other topics, the analytic/synthetic distinction which played a pivotal role in the philosophy of science of classical logical empiricism. This paper explores the consequences for Carnap's semantics in the light of Paul Roth's helpful recent reconstruction and defence of Quine's argument (Roth 2023). In Roth's reading, which while staunchly naturalistic avoids undue behaviourist scepticism of meaning but also rejects any reduction of indeterminacy to underdetermination, the indeterminacy doctrine is understood as denying the determinacy of the meaning of statements antecedent to their translation. The question under investigation is whether Carnap was committed to determinacy in the sense attacked by this reading of Quine's argument. Roth uses Carnap as a traditionalist foil in developing his interpretation and attributes to him the "recapture" conception of translation. Here the case will be made that, on the contrary, Carnap was not committed to the pre-existence of determinate frameworks of meaning that translations then recapture. (The Ricketts-Roth point that frameworks create determinacy and do not discover it is readily granted.) Attention will be drawn first to Carnap's methodology of providing "explications" in order to make precise what had not been so previously. Its relevance to the question at hand is then brought home by recalling the anti-psychologism of Carnap's semantic theorizing. Against this background, two mistakes on which Quine's charge against Carnap was based stand out particularly clearly: the attribution to Carnap's semantics of the explanatory offices of a philosophy of psychology and the misrepresention of Carnap's "pragmatical concept of intension in natural languages" (Carnap 1955). It will be concluded that once these mistakes are corrected, Quinean indeterminacy as clarified by Roth can be seen to pose no threat to Carnap's conception of semantics and also none to any explicatory roles that may be played by the analytic/synthetic distinction in the philosophy of science.Selected references:Carnap, R. 1955. "Meaning and Synonomy in Natural Languages."Philosophical Studies6: 33-47.Quine, W.V.O. 1960.Word and Object. Cambridge, Mass.: MIT Press.Roth, P.A. 2023. "What Does Translation Translate? Quine, Carnap and the Emergence of Indeterminacy." In S. Morris (ed.),The Philosophical Project of Carnap and Quine. Cambridge: Cambridge University Press, 154-176.

Vagelli, Matteo

On the Very Notion of Styles of Scientific Reasoning

Abstract: In this paper, I examine Ian Hacking's theory of styles of scientific reasoning and its implications for the merging of philosophical and historical perspectives on the study of science. Hacking's style project, which started in 1982 and spanned over 30 years, elaborated a notion of scientific styles aimed at accounting for both the historical, situated nature of scientific knowledge and practice as well as their objective and progressive features. However, these aims have generally been conceived as being incompatible with one another and critics have highlighted flaws in Hacking's attempt at holding them together through the notion of style. It has been remarked that styles, despite Hacking's intentions, a) invite a form of epistemic relativism which is incompatible with the idea of scientific progress. Furthermore, b) a tension has been detected between an early constructivist understanding of styles, and a later naturalized and realist development of the idea by Hacking. Critics have also expressed the very opposite worry that c) styles, by focusing on the intra-linguistic coherence among linguistic judgements, may be self-enclosed entities that are impermeable to empirical refutation and ultimately not accountable to the world (Rouse, 2011). In order to both take stock and intervene in these debates, I wish to focus on what Hacking calls the "truth-producing virtue of styles" (Hacking 1992) and discuss it in relation to two only apparently similar concepts: the concept of "positivity", that Hacking draws chiefly from Michel Foucault (Foucault 1969), and the concept of "truthfulness" that Hacking draws from Bernard Williams (Williams 2002). By clarifying these concepts, I wish to provide arguments to defend the styles-project from falling into the three pitfalls of a) relativism, b) intra-linguistic coherentism and c) naïve realism.

Vagnetti, Michele

The Philosophical Roots of Scientific Psychology in Germany: Herbart and Fries

Abstract: The origin of modern scientific psychology is generally associated with the names of the physiologists Ernst Heinrich Weber, Gustav Theodor Fechner, Hermann von Helmholtz, and Wilhelm Wundt. These figures combined great philosophical interests with profound expertise in physics and physiology. The concept of psychology as an experimental science was the result of a long philosophical debate in early 19th century Germany. What, then, is the role of German philosophy in this development of modern scientific psychology? The purpose of my talk is to illustrate how a debate about the possibilities of psychology as a science was already alive within early nineteenth-century German philosophy, especially through Kant, Herbart and Fries. In his Metaphysical Foundations of Natural Science (1786), Kant states that science is a mathematically ordered system of knowledge and adds that, on this basis, psychology cannot be a science because mathematics cannot be applied to the inner sense and its laws. The Kantian critique of psychology and the Kantian philosophy of science constituted a challenge to the two post-Kantian philosophers Jakob Friedrich Fries and Johann Friedrich Herbart: it served them as both a negative and positive basis for the development of their conception of psychology as an independent and mathematical science. With Herbart and Fries we witness the transition from "rational" to "scientific" psychology.This talk will attempt to shed light on the contribution of Herbart and Fries to the development of modern psychology, as an experimental psychology. Always keeping Kant in the background, I will focus on Herbart and Fries and their conception of the methods and nature of psychology and on how they elaborated the conception and philosophical justification of psychology as a natural science. The philosophical debate, which begins with Kant and continues with Fries and Herbart (among others), forms the immediate theoretical background that preceded and in part determined the experimental investigations of German psychology in the mid and late nineteenth century (E. H. Weber, Gustav Fechner, Hermann von Helmholtz, and Wilhelm Wundt).

Van Dyck, Maarten

The question of Platonism in the work of Alexandre Koyré

Abstract: Recent literature has helped paint a much more fine-grained picture of the rich philosophical background to Alexandre Koyré's seminal work in history of science (especially notable are Zambelli 2016 and Parker 2017, but see also further contributions in Seidengart 2016 and in Pisano et al. 2017). Edmund Husserl taught him how to use the phenomenological epoche as a method for the analysis of conceptual structures; Emile Meyerson showed him how the history of science could lay bare the dialectical nature of human rationality; and from Lucien Lévy-Bruhl he took comparative research as a privileged way into the importance of worldviews. In my presentation I will use this background to offer a novel interpretation of Koyré's historical research program into the scientific revolution of the 16th and 17th century. In a first step, I will show how this research developed out of an interest in the logical paradoxes of the infinite, which was part of his earlier research under the tutelage of Husserl, and which remained with him until much later in his life. In a second step, I will use this to reassess the much discussed importance of Platonism in his interpretation of the scientific revolution. I will show that this derives from a specific view of the relation of a finite, human subject to the absolute infinite. Koyré's history of science offers an anthropology of the tragic fate of such a subject, always at work to reach something that must remain beyond its grasp. Science, in his view, is the necessary fragile, instrumentally mediated stabilization of the essential tension between the real and the ideal. In conclusion, I will offer some reflection on how this puts Koyré's interpretation of the scientific revolution at a distance from that of some of his contemporaries. Ernst Cassirer, in particular, had also stressed the importance of Platonism (following the earlier work of Paul Natorp), but Koyré offered a strikingly different view of the nature of Platonism that took issue with what he saw as the overly epistemological focus of the Neo-Kantians, which missed the tragic dimension proper to finite human beings. At the same time, he also criticised Heidegger for missing the way in which human knowledge did orient itself to the infinite. In this way, we can see history of science in the first half of the twentieth century as an important battle-ground for different philosophical views on how human subjectivity grounds scientific knowledge. R.K.B. Parker (2017). "The History between Koyré and Husserl." In Pisano et al., pp. 243-275.R. Pisano, D. Drozdova & J. Agassi (eds.) (2017). Hypotheses and Perspectives in the History and Philosophy of Science. Homage to Alexandre Koyré 1892-1964, Springer, Dordrecht.J. Seidengart (ed.) (2016). Vérité scientifique et vérité philosophique dans l'oeuvre d'Alexandre Koyré. Les Belles Lettres. P. Zambelli (2016). Alexandre Koyré in incognito. Olschki, Firenze.

Van Gemert, Ties

On Ferdinand Gonseth's Philosophie Ouverte

Abstract: The Swiss philosopher and mathematician Ferdinand Gonseth (1890-1975) is not well-known among historians of philosophy of science. Even though his work continues to be studied in his home country and has been the subject of a few articles and one book, Gonseth's place within the development of philosophy of science remains largely unexplored. While forgotten figures abound in the history of philosophy, Gonseth's absence is somewhat surprising: in his own time, he was perceived by the community of philosophers of science as someone to be reckoned with. Not only through his organization of entretiens between philosophers and scientists but also through his writings, Gonseth influenced the debates of his time: among them, the status of intuitionist mathematics, the philosophical implications of relativity theory and the conception of scientific philosophy as presented by the Vienna Circle. It seems that if one intends to give a historically adequate account of the community of philosophy of science before the Second World War, Gonseth must be included in that picture. One of the principal reasons for Gonseth's fate is his uncompromising character. Unwavering in his effort to establish a tradition of his own, he insisted that others adhered to his approach: open philosophy (philosophie ouverte) or idoneism. To accomplish this aim, he built up a remarkable network, institutionalized through the journal Dialectica. Founded in collaboration with Gaston Bachelard and Paul Bernays, this journal published numerous significant articles in the 1940s and 1950s: among them being Albert Einstein's Quantenmechanik und Wirklichkeit (1948), Niels Bohr's On the Notions of Causality and Complementarity (1948) and Kurt Gödel's Über eine bisher noch nicht benützte Erweiterung des finiten Standpunktes (1958). Since I assume that many will not be familiar with Gonseth, I will begin by introducing his work and life, giving a brief sketch of idoneism. Next, I expound Gonseth's critique of the Vienna Circle as presented in two of his works, namely Les mathématiques et la réalité (1936) and Philosophie mathématique (1939). After that, I set out his understanding of Bridgeman's operationalism as developed in his work on the measurement of time: Time and Method: An Essay on the Methodology of Research (1964/1972). I will compare his understanding to that of Hasok Chang (2004) and Eran Tal (2016). Drawing on correspondence from the Fonds Gonseth in Lausanne, I end by outlining how Gonseth's philosophical orientation shaped the agenda and publication history of Dialectica. The objective of this talk is not only to understand Gonseth's place in history, but also to bring his philosophy of science back in dialogue with other traditions. By historically and philosophically explicating Gonseth's philosophy of science, we not only become aware of a forgotten alternative approach and so reach a better understanding of the variety of interlocutors of philosophers of science in the first half of the twentieth century but also enrich our grasp of problems surrounding operationalism and axiomatics that are still discussed today.

Van Strien, Marij

The Problem of Matter in the Nineteenth Century

Abstract: In their new book, Brading and Stan describe how in the eighteenth century, physics was a subdiscipline of philosophy, and primarily concerned with giving an account of bodies. They argue that by the early nineteenth century, this situation had changed: physics has now become separated from philosophy and giving an account of bodies was no longer the central aim of physics.In this talk, I will argue that despite this shift in the aims of physics, by the early nineteenth century the philosophical problem of the nature of matter and bodies had not disappeared from physics: in fact, physicists throughout the nineteenth century debated conceptions of matter on the basis of empirical and pragmatic as well as philosophical considerations. As Brading and Stan point out, in nineteenth century physics, there was no unified conception of matter: physicists used different conceptions, including point particles, hard extended atoms, deformable bodies, and newly developed alternatives such as the vortex atom. Empirical results posed major challenges to the attempts to develop a unified notion of matter: a particular puzzle was raised by results from spectroscopy and from the kinetic theory of gases which led to contradictory conclusions regarding the inner structure of atoms. But also conceptual, philosophical issues played a role in how physicists conceived of matter: for example, physicists such as Maxwell and Stallo discussed whether extended, deformable atoms could be taken to be foundational elements or whether such atoms would be further analysable. Furthermore, I will argue that a significant shift took place towards the end of the nineteenth century, when the philosophical problem of the nature of matter was increasingly rejected by physicists. Du Bois-Reymond, in his famous lecture 'On the limits of our knowledge of nature' from 1872, argued that the nature of matter will always remain unknowable, presenting it as an unsolvable philosophical problem. Later authors such as Hertz, Mach, Duhem and Poincaré rejected the ontological question of the nature of matter altogether, arguing that science generally does not tell us about the true nature of things but can merely yield a description of the phenomena. Their arguments were motivated in part by the conceptual difficulties encountered in developing an account of matter. It is therefore perhaps only from the 1890s onwards that the philosophical problem of the nature of matter largely disappeared from physics.

Vanrie, Wim

The Bounds of Tolerance: Quine's Early Reception of Carnap

Abstract: In 1933, W.V.O. Quine met Rudolf Carnap in Prague, the beginning of an intellectual friendship that shaped the future course of the analytical tradition. As hotly debated as Quine's reception of Carnap's work is, however, there is a tendency to jump forward two decades, and take Quine's watershed critique of the analytic/synthetic distinction in Two Dogmas of Empiricism (1952) as the key document from which to recapitulate their debates. Part of the reason for this may be that Quine himself tended to say that he started out as a mere disciple of Carnap, and only later distanced himself from his views.In this paper, I draw on some crucial documents from Quine's early oeuvre to argue that this is a misconception. In fact, we can detect clear differences in philosophical outlook between Carnap and Quine from the very beginning of their philosophical interaction. My main focus is on Quine's Harvard Lectures from 1934: three lectures that were meant to introduce Carnap's ideas from Logische Syntax der Sprache (1934) to American audiences. Although understudied, these lectures offer crucial insights into Quine's early reception of Carnap. I will argue that-contrary to what Quine's later characterization of these lectures as "abjectly sequacious" (Quine, 1991, 266) suggests-they already reveal how Quine is adapting Carnap's thought to his own philosophical purposes, which are not the same as Carnap's.In Syntax, Carnap draws on novel developments in metalogic to advance his program of logical syntax, a rigorous metalogical method of constructing and analysing conceptual systems that provides us with a definitive framework for philosophical clarity, and that unmasks any residual nonlogical philosophical problems as illusory. Quine, however, takes Carnap's metalogical tools to make possible a conventionalist solution to the problem of justifying our a priori knowledge-by rendering it a product of conventional syntactical stipulations-while Carnap did not acknowledge this as a genuine philosophical issue at all. In this way, Quine transforms Carnap's program of logical syntax into a method for providing solutions to the very problems that Carnap sought to eliminate from philosophy once and for all. Consequently, Quine also misunderstands the role played by the Principle of Tolerance in Carnap's philosophy. Quine takes it to issue in an elimination of metaphysics that is grounded in conventional syntactical decisions. For Carnap, however, the Principle embodies a norm of clarity that is incompatible with metaphysics, the elimination of which is not conventional at all.Achieving clarity about Quine's early reception of Carnap's views-and how that reception immediately involves a difference in philosophical outlook-enriches our understanding of the more well-known later interactions between Quine and Carnap, which are rooted in these initial, less-explored encounters. Moreover, it helps us to better understand what was more broadly at stake in their debates: the philosophical ramifications of the novel developments in metalogic which both Carnap and Quine-each in their own way-seek to put to philosophical work.

Veldman, Michael

Seeing Du Chatelet's Methodology by the Light of her Dissertation of Fire

Abstract: Emilie Du Chatelet's magnum opus Foundations of physics (1740/42) has received the lion's share of recent scholarly attention to her philosophy. Though this is surely justified, consideration of all of her works is needed for a full appreciation of her natural philosophy and her influential philosophy of science, which until now has been based largely on one famous chapter – the fourth chapter 'On Hypotheses' – of the Foundations. Thus, I propose to examine Du Chatelet's Dissertation on the nature and propagation of fire (1739/1744), submitted in 1737 to the Essay Competition of the Paris Academy. In this paper, I discuss the historical and scholarly background before describing her project and methods in the Dissertation, through which I aim to begin to show her distinctive approach to scientific method in general, and hypotheses in particular.Without hypotheses, modern science is hardly imaginable. Yet, in the 18th century, Newton's famous dictum "hypotheses non fingo" (I don't feign hypotheses) became a slogan for a movement to reject hypothetical reasoning in toto. Hypotheses were widely viewed as unscientific. Du Châtelet's defense of hypotheses in the Foundations began the work of delineating the utility of hypotheses and setting constraints for their responsible use. I show how her Dissertation on fire (1737/1744) offers a more expansive view of her conception of methodology and epistemology in science than is available through the Foundations alone. Going beyond existing studies of Du Châtelet on hypotheses and sufficient reason, I show how Du Châtelet is committed to the applicability of mathematics and the use of calculation as crucial to the utility and rigor of hypotheses, and illustrate through examples. This clearly separates her approach to hypotheses from the standard 'Cartesian' one. Through it, she helped to make it once again respectable to use hypotheses in science while delineating plausible conditions on the rationality of particular uses.As alluded to above, one major reason the Dissertation provides a unique window onto her methodology of science is that in this text we see her engaged at length in an attempt to answer first-order scientific questions about fire and heat, rather than merely foundational metaphysical and epistemological questions. Arguably, it is also only here that we see the empirical side of her research energies, in connection to experiments conducted at her Cirey estate (by or with Voltaire; how the experiments were conducted remains somewhat unclear). The Dissertation thus illuminates her attitudes towards the relation between metaphysics, physical theory, hypothesis, mathematics, and experiment, in ways that other texts cannot. A key example is her treatment of the question whether fire can be regarded as a substance with mass, a question which she answers in the negative by mathematically drawing out the consequences of this hypothesis, using the resources of the theory of impact. Using this and other examples from the Dissertation, I build on existing scholarship on Du Chatelet's view of hypotheses by spelling out what differentiates her from

other theorists and practitioners of hypothetical reasoning in the early modern period (Paganini 2022)

Venturinha, Nuno

‘Inductive Reasoning in Wittgenstein’s Tractatus’

Abstract: In Tractatus 6.3, returning surreptitiously to a conclusion drawn in 6.031, Wittgenstein writes "Logical research means the investigation of all regularity". This is the motto to his propositions on scientific representation, which lay down the philosophy of science of the Tractatus. The core of Wittgenstein's view is the denial in 6.31 that the "so-called law of induction" – probably a reference to Lecture VIII of Russell's Our Knowledge of the External World as a Field for Scientific Method in Philosophy – can have the status of "a logical law", an assertion he elucidates by saying that "it cannot be a law a priori either". Wittgenstein emphasizes this point in 6.3211 when he forcefully claims that "the a priori certain proves to be something purely logical". Interestingly, Wittgenstein distinguishes in the next proposition, numbered 6.33, between believing and knowing a priori. His argument is that while one does not "believe a priori in a law of conservation", one does "know a priori the possibility of a logical form". Belief, by definition, falls outside the domain of the a priori since it admits the possibility of disbelief and is primarily an act of will. Believing a priori would be binding and tantamount to knowing a priori. Knowing, however, is an epistemic mode that can be either a priori, when something is necessarily the case, or a posteriori, when it is accidentally or contingently the case. Wittgenstein's point is that, rather than knowing an induction law, we do no more than believe in it. But, contrary to the prevailing interpretation, the a priori still plays a quasi-synthetic role. In 6.34 Wittgenstein talks about various scientific laws as "a priori intuitions of possible forms of the propositions of science". He further compares in 6.341 "the system of numbers" to "the system of mechanics" before highlighting the significance of geometry. This is characterized in 6.35 as a "network" that "is purely geometrical" in the sense that "all its properties can be given a priori". The ultimate aim of this paper is to explain why Wittgenstein contends that the epistemological "process of induction", alluded to in 6.363 and 6.3631, lacks a "logical foundation" but instead rests solely on "psychological" grounds. Wittgenstein's emphasis on belief at this juncture, I shall argue, is actually congruous with his earlier definition of epistemology, in 4.1121, as "the philosophy of psychology".

Verhaegh, Sander

How American Philosophy turned Analytic: A Tale of Three Departments

Abstract: American philosophy rapidly changed in the 1950s and 1960s. Pragmatists, realists, and idealists had dominated the discipline for decades but they were quickly overrun by philosophers who advocated a more scientific or analytic approach. How did this happen? And what factors contributed to this sudden transition? Standard accounts focus on the migration of the logical empiricists and the reception of their work by a small number of young American pragmatists-e.g. Morris, Nagel, and Quine-but it seems unlikely that a handful of refugees and a few U.S. students had sufficient institutional weight to spark an intellectual revolution. In 1945, the American Philosophical Association had 870 members and few of them had been trained in conceptual analysis, symbolic logic, or philosophy of science. In this paper, I aim to shed new light on the transformation of American philosophy through a study of the institutional archives of three major philosophy departments: Princeton, Yale, and Columbia. I reconstruct changes in their hiring policies, their tenure decisions, and their curriculum designs between 1940 and 1970 in order to distill some of the factors that contributed to the analytic turn. First, I describe how philosophers actively attempted to keep the positivists out. I argue that there was widespread agreement that the positivists were a fundamental threat to the discipline and show how this opposition influenced decision making in all three departments. Next, I show why the quasi-boycott of positivist philosophy was eventually unsustainable and I argue that this led philosophers to shift gears. In the 1950s, a new generation of chairmen tried to craft more 'balanced' departments, noting that an ideal curriculum should pay equal attention to "the historical and humanistic aspects of philosophy on the one hand and the logical and scientific on the other". Though this new policy, for some, was a strategy to protect traditional approaches to philosophy, I argue that it had the opposite effect. The increased demand for analytic philosophers and the shortage of American graduates educated in this tradition, created a fierce competition between prestigious departments. Within a decade, representatives of the new movement were flooded with job offers, thereby tipping the balance in the other direction. Though there were some departments that kept emphasizing balance, I show how these mechanisms stimulated policy makers to put most of their eggs in one analytic basket.

Vida, Grigore

Pierre Borel and Descartes' Democritean Physics

Abstract: Much has been written about Descartes's two physics: metaphysical and mathematical. In this paper I argue in favor of a third one, which I will call, building on the writings of Descartes' early biographer Pierre Borel, a "Democritean physics." It is the physics of Descartes' Meteors, rewritten into parts III and IV of the Principles of Philosophy, featuring eel-like particles of water, needle-shaped corpuscles of salt, and branchy structures of oil. This gallery of bizarreries is certainly not mathematical, neither does it look metaphysical. Apart from the cases when it was simply ignored, this physics was often relegated to the rather vague domain of "hypotheses" – useful explanatory devices, formed by analogy with objects of the macro-world, but with no serious ontological import: a construction of an invisible world in which the imagination could play quite freely. However, an unexpected way of overcoming the usual interpretative dead-ends concerning this part of Descartes' physics is provided by Pierre Borel, a much-understudied medical practician of the 17th century with interests in natural philosophy, alchemy, and microscopy. In his biography of Descartes, Borel constructs an image of Descartes as a Democritus redivivus, set up to find out "that Democritical Truth [...], which lies hid, as it were in the bottom." He uses this assimilation with Democritus to bypass the metaphysical arguments against atoms from Principles II.20, as well as to treat Descartes' corpuscles not as mere hypothetical entities, but as the true constituents of reality. Sometimes Borel even seems to imply that, with the newly-invented microscope, these particles might become open to empirical investigation, thus completely shifting them from imagination into reality. Borel's reading also opens the question of a real debt of Descartes to Democritus, to which some of his contemporaries pointed out, and some later Cartesians tried to disprove.

Voelker, Robert

Normativity of Quine's Kinds

Abstract: Quine's views of natural kinds are idiosyncratic among naturalistic philosophers (Quine 1969). They cohere well with attempts to make natural kinds naturalistically acceptable, emphasizing them needing to support scientific inductive inferences. Quine also gives a naturalistic evolutionary gloss in how natural kinds are formed in our scientific and linguistic practices, somewhat similar to (Kornblith 1995). Quine's views conflict with most naturalistic glosses of natural kinds because Quine thought natural kinds are at best a stop-gap before mature sciences render them superfluous. This conflict is over what is properly naturalistic. Quine thought that similarity relations are not naturalistically respectable, and that the project of giving a fully naturalistic gloss of natural kind simpliciter, let alone natural kinds in mature sciences, was futile. Quine thought that natural kinds are a useful tool which we use to facilitate inductive inferences before we have the better vocabulary to explain what is really happening using the language of our best scientific theories, thus are an example of Quinean explication.Some natural kind defenders such as (Khalidi 2013) eschew brute similarity requirements, and attempts at giving rigorous glosses of natural kinds such as grounding them in homeostatic property clusters or underlying causal structures seem to suggest Quine's pessimism about the naturalistic respectability of natural kinds was unwarranted (Boyd 1999; Khalidi 2013). In particular, views which focus on the underlying causal relations examined by special sciences suggest Quine set a false dichotomy between natural kinds versus special sciences. The ontology of such views of natural kinds is suspect, with any plausible view looking to be necessarily conventionalist (Chakravartty 2023). I argue that if these views are necessarily conventionalist then this makes both Quine's dichotomy between natural kinds and mature sciences and his pessimism about the naturalistic respectability of natural kinds plausible. If such views are conventionalist then their ontology is necessarily tied to our normative practices, both linguistic and scientific, and not inherent to the kinds. I also argue Quine's view of natural kinds as part of Quinean explication offers a way of keeping the rigor of contemporary glosses of natural kinds while giving a normative background for us to use them. Each argument requires I explain and motivate Quine's views of natural kinds, convention, and normativity, which are oft-misunderstood and in conjunction seem incompatible. In particular, I show how Quine's antagonism towards normativity in language is compatible with minimal commitments of normativity in our linguistic practices and in Quinean explication.ReferencesBoyd, Richard. 1999. "Kinds, Complexity and Multiple Realization." Philosophical Studies 95:67-98.Chakravartty, Anjan. 2023. "Last Chance Saloons for Natural Kind Realism." American Philosophical Quarterly 60:63-81.Khalidi, Muhammad A. 2013. Natural Categories and Human Kinds: Classification in the Natural and Social Sciences. N.p.: Cambridge University Press.Kornblith, Hilary. 1995. Inductive Inference and Its Natural Ground: An Essay in Naturalistic Epistemology. N.p.: MIT Press.Quine, Willard v. 1969. "Natural Kinds." In Essays in Honor of Carl G. Hempel A Tribute

on the Occasion of his Sixty-Fifth Birthday, edited by Nicholas Rescher, 5-23. Dordrecht: Springer Netherlands.

Vrahimis, Andreas

Stebbing against anti-realism in the philosophy of science

Abstract: Within the recent revival of interest in Susan Stebbing's work, she has been commonly interpreted as a representative of the Cambridge analysts defending a Moorean conception of directional analysis. Nonetheless, Janssen-Lauret (2022) has challenged this hypothesis by examining a series of papers from the 1920s which demonstrate that Stebbing should also be credited with developing a variety of original positions in the philosophy of science. Janssen-Lauret characterises Stebbing's overall outlook as a defence of a form of (non-naïve) realism. This paper will trace the development of Stebbing's realism in her critical engagement with different positions in the philosophy of science, where she systematically opposes different varieties of anti-realism. As far back as 1912 in her Pragmatism and French Voluntarism, Stebbing criticised both Pragmatism's instrumentalist tendencies, as well as the French Voluntarists' methodological appeal to intuition, e.g. for both failing to provide a satisfactory answer to the question of whether Euclidean geometry can describe reality. This earlier work will inform Stebbing's critical reception of Whitehead's philosophy of nature during the 1920s, in which context she would begin to develop her own brand of realism. While she grows increasingly dissatisfied with the trajectory followed by Whitehead after 1929, the realistic attitude she had defended in her early papers is maintained throughout subsequent work, including e.g. her account of the significance of logic for scientific methodology, her various defences (and eventual abandonment) of directional analysis, and especially her critique of scientists' anti-realist muddles in Philosophy and the Physicists.

Walter, Scott
Corentin, Fève

Closed theories in physics: from Poincaré and Hilbert to Heisenberg

Abstract: The two great scientific revolutions of the early 20th century -- relativity theory and quantum mechanics -- inspired new outlooks on theories of physics elaborated in the 18th and 19th centuries. A leader of the quantum revolution, Werner Heisenberg drew from first-hand experience of theory change an epistemological lesson: classical mechanics, like quantum mechanics, is both unsurpassable and true. Both mechanics are what he termed "closed" theories, the creation of which he believed, furthermore, to be the objective of theoretical physics. Heisenberg's epistemology of closed theories, cast in a neo-Kantian framework by a number of writers, including Chevalley (1998), Bokulich (2004, 2006), and Schiemann (2009), is considered here in relation to Poincaré's conventionalism, on the one hand, and to Hilbert's axiomatization program for the exact sciences (Hilbert's Sixth Problem), on the other hand. Heisenberg's epistemology of closed theories will be seen to incorporate elements from the programs of both Poincaré and Hilbert.

Wells, Aaron

Du Châtelet’s Evidence for Simple Substances

Abstract: In this talk I take a closer look at the evidence for simple substances in Du Châtelet's Institutions physiques, which Brading and Stan's Philosophical Mechanics in the Age of Reason (PMR) describes as the middle eighteenth century's "most developed attempt to provide a philosophical mechanics of bodily action." I argue that to give evidence for simples, Du Châtelet draws on rules of induction laid out in her chapter on hypotheses. If so, a central plank of Du Châtelet's metaphysics is inductive. Furthermore, at least in the order of discovery, she takes an account of matter to be prior to an account of simples. Du Châtelet's simple substances are not observed directly. As PMR explains, an endorsement of simples therefore faces the empiricist objection (from 's Gravesande, for example) that properties of body cannot be discovered a priori, and there's no reason to believe simples are epistemically better off. Although Du Châtelet in this context invokes her principle of sufficient reason, which is a priori, I don't take her to treat that principle as sufficient to establish the properties of either bodies or simple substances. Instead, her argument is in the form of a conditional: if all bodies have certain properties, such as extension and active and passive force, then these properties must be grounded in underlying simple substances. Her principle of sufficient reason is supposed to establish that the conditional as a whole is true, and at least on the reading in PMR, it also gets her further conclusions, for example that each body and its properties supervene on a finite collection of simples.But what provides evidence for the antecedent in this conditional? Du Châtelet tells us that experience or observation gives us properties instantiated by all bodies. But why think experience of particulars generalizes to all bodies? Here it's helpful to observe that Du Châtelet only presents her argument for simples after a detailed discussion of hypotheses. There, she suggests that (1) if a property is shown to hold in all known experimental circumstances, it can be taken as holding of bodies generally, and that (2) this well-founded hypothesis can only be dislodged if some contrary experiment contravenes it. (1) and (2), in her historical context, are not trivial assumptions: it was common to limit the scope of induction. They have affinities with, respectively, Newton's third and fourth Rules of Reasoning. So while I agree with PMR that Du Châtelet relies on both the principle of sufficient reason and empirical considerations to make her case for simples, I contend that she relies on other principles as well-namely rules of induction. I close by posing two questions that, as far as I know, remain open. First, is Du Châtelet's justification for the rules of induction themselves basically theological, or does she have other grounds? And second, to what extent does her account of hypotheses and induction support her laws of motion?

West, Peter

Philosophy is not a science: Margaret Macdonald on the nature of philosophical inquiry

Abstract: Although there are signs of a growing interest in the work of Margaret Macdonald (1903-1956) (e.g., Kramer 2021, Vlasits 2021, Whiting 2022), she remains a somewhat peripheral figure in the history of twentieth century philosophy. Yet, there is a case to be made for thinking that she was in fact one of the central figures in the development of the analytic tradition in Britain and a key figure in twentieth-century philosophy of science. Along with Susan Stebbing (her PhD supervisor) and Gilbert Ryle, Macdonald founded the journal Analysis, which she edited from 1948 to 1956. Along with Alice Ambrose (a fellow student), she was also responsible for the publication of Wittgenstein's Blue and Yellow (or Brown) Books. What might the story of the origins and development of analytic philosophy look like if Macdonald were rightly recognised as one of its main characters (alongside the likes of Russell and Moore)? An analysis of Macdonald's writings on scientific and philosophical method suggest that the answer is: Very different. In her 1953 paper, 'Linguistic Philosophy and Perception', Macdonald uses the case study of (analytic) philosophical debates about perception to demonstrate that the kinds of hypotheses defended by philosophers are quite different from those of scientists and mathematicians. While a mathematical hypothesis ought to be trivially (i.e., analytically) true, a scientific hypothesis, according to Macdonald, is the kind of proposition that can be verified or falsified by experiment. But crucially philosophical hypotheses, Macdonald argues, do not fall into either of these camps. It is not possible to carry out an 'experimentum crucis' to verify or falsify the claims of various philosophers of perception. For that reason, philosophical hypotheses are neither scientific nor mathematical. What's more, Macdonald claimed that philosophers engaged in a particular debate (e.g., the philosophy of perception) do not actually disagree about the phenomena (e.g., all parties agree that there are veridical and illusory experiences), they simply disagree on how best to articulate those phenomena. On that basis, Macdonald arrives at the surprising conclusion-surprising, that is, for those well-versed in typical narratives about the aims and method of analytic philosophy-that philosophy is much more like an art than it is a science. In her own words, "Philosophical theories are much more like good stories than scientific explanations." And philosophical disagreements, according to Macdonald, really boil down to different ways of articulating phenomena, or what Macdonald calls different "emotional attitudes."In this paper, I will argue that an analysis of Macdonald's metaphilosophy reveals that at least one figure at the forefront of the 'analytic' movement did not accept the image of analytic philosophy as something akin to a science. Furthermore, I conclude that revisiting Macdonald's views on philosophy and science, and undoing the neglect of her work, might open up new approaches to the question of what philosophy is – and what it could be.

Wiesenfeldt, Gerhard

Husserl among the Geometers: Transcendental Phenomenology Meets Artisanal Epistemology

Abstract: Edmund Husserl's role in the History and Philosophy of Science seems to be predominantly confined to a figure lurking in the shadows. While he may have been informative to canonical writers - from Koyré to Foucault - he is only rarely considered as a direct source to aid in epistemological analysis, notwithstanding one major attempt to refocus the attention of HPS scholars back on Husserl (Hyder & Rheinberger, Science and the Life-Word, 2010).It is easy to deplore this state of affairs, not only because Husserl's magnum opus - The Crisis of European Sciences and Transcendental Phenomenology is an extensive analysis of the role of science in the modern world, but because the concept of life-world established therein could be utilised to help historians of science in attempts to get a more sound methodological foundation for the widely used concept of context. It is also easy to see why Husserl has often been neglected. He holds a deep ambivalence towards modern science that has often be interpreted as hostility and he is more interested in what science has done to the world than what constitutes science. Most importantly his historical reconstruction of scientific development - in Crisis and elsewhere - seems problematic in its apparent focus on idealist interpretation of Galileo and Descartes only.This talk aims to reassess the value of Husserl's phenomenology for the history of science and engage with Husserl's reading of history from the perspective of current historical research. It will focus on geometry as the discipline at the core of Husserl's interest. His history of geometry has a mythological character, placing the origin of modern science in a few key transitions: conceptualising measurement, developing the art of surveying, and turning that art into abstract geometry. His historical reconstruction thus points towards three fundamental questions relevant to all historical epistemology - on the origin of knowledge, how knowledge becomes tradition as part of the life-world of those immersed in these traditions and how knowledge changes when it becomes tradition. The talk will apply at least the last two of these questions to early modern geometry to explore the benefits of a Husserlian reading of history.

Wulz, Monika

Edgar Zilsel’s Correspondence: Tracing the Relation of Epistemology and Politics

Abstract: In this paper, the relationship between epistemological and political aspects in Edgar Zilsel's scholarly writings will be discussed in the light of recently discovered letters by Zilsel to Thomas Mann, Paul Taussig and others (Wulz 2022). In these letters, Zilsel himself reflected on the relation between his philosophical work, his methodological approach in the humanities, and his engagement as an "Austro-Marxist" intellectual and member of the Social-Democratic party. The letters add a long-time perspective to Zilsel's previously discussed methodological concerns regarding his failed habilitation thesis 1923/24 (Raven/Krohn 2000; Taschwer 2022). They show that Zilsel's methodological approach for the period of the 1920s and 1930s was not only connected to his interest in the natural sciences but at the same time reflected his political position as a Marxist intellectual with an indeterminist understanding of historical development. The paper will address the role of Zilsel's correspondence with his contemporaries as a medium for substantiating his intellectual approach in light of his political conviction.ReferencesRaven, Diederick, and Wolfgang Krohn. 2000. "Edgar Zilsel: His Life and Work (1891–1944)". In Edgar Zilsel. The Social Origins of Modern Science, ed. D. Raven and W. Krohn, xix–lix. Dordrecht/Boston/London: Kluwer Academic Publishers.Taschwer, Klaus. 2022. "The Circumstances of Edgar Zilsel's Failed Habilitation. A Case-Study on the Instigations of Anti-Semitic and Conservative Academic Networks in the 1920s at the University of Vienna". In: Edgar Zilsel: Philosopher, Historian, Sociologist. Vienna Circle Institute Yearbook 27. Cham: Springer, 45–60.Wulz, Monika. 2022. "On Thermodynamics and Society: Edgar Zilsel's Epistemology and Politics Across Disciplinary Boundaries". In: Edgar Zilsel: Philosopher, Historian, Sociologist. Vienna Circle Institute Yearbook 27. Cham: Springer, 61– 89.

Zellmer, Jacob

La Peyrère's Polygenism and Human Species Hierarchy

Abstract: Isaac La Peyrère (1596-1676) was an early anthropologist who popularized the pre-Adamite thesis-the claim that God created people prior to creating Adam and Eve-and the related thesis of polygenesis-the claim that God created multiple first human progenitors in separate acts of creation and multiple such progenitors were created at a single time before Adam. La Peyrère argues that Genesis contains two creation accounts. In the "first creation" of Genesis 1, God made many original humans across earth as progenitors of the Gentiles (or pre-Adamites). In the second creation in Genesis 2, Adam and Eve were created as progenitors of the Jews (or Adamites). La Peyrère claims the humans produced by the first and second creations are different in "kind [genere]" and "species [specie]" (TS 121-2, 153). The dominant interpretation maintains that La Peyrère's polygenism does not itself imply a hierarchy of human species or races despite being taken up by later Americans to defend scientific racism and slavery (Popkin 1987: 46; Smith 2011: 224, 247). Commentators claim that La Peyrère saw all people as biologically equal or "made up of the same flesh and blood" (Popkin 1987: 46). Justin E. H. Smith suggests that La Peyrère takes no interest in creating a hierarchy of lower and higher racial types: "preoccupation with 'racial difference' is more or less absent" (2015: 102).This paper reconstructs La Peyrère's polygenism and argues that it does set up a species hierarchy. I argue that the two human species are differentiated hierarchically by their material composition, mode of creation, and form. La Peyrère maintains that the Adamites are made of more refined material (TS 136-7). Concerning the mode of creation, La Peyrère argues that the first humans were created with the rest of creation and animals (by God's "word"), whereas the Adamites were created in a special act of creation (by God's "hand"). Although La Peyrère maintains that both species of humans are created in God's image, he claims they are created in different images of God: the pre-Adamites are made in God's "external" image, whereas the Adamites are made in God's "internal" image and are therefore more divine or perfect (TS 18-19). The pre-Adamites were made of more primitive matter and through a lesser mode of creation. Further, La Peyrère closely connects the non-Adamic human species to animals (TS 112, 122, 144). Hence the "framing" (formatione) of the two species was "altogether different" (TS 135).These differences between the two human species, I argue, underlies a metaphysical andnormative hierarchy. The Adamic humans were made "higher" and are Sons of God by "fabric [formatione], which is nigher to nature" (TS 112). I contend that the hierarchy gives human species distinct essences or natures that ground normative properties-as subsequent racialist concepts of race do-and therefore La Peyrère creates a proto racialist human hierarchy.Works cited:La Peyrère, Isaac. 1655. Systema Theologicum ex Prae Adamitarum Hypothesi. Leiden: Elzevier.Popkin, Richard. 1987. Isaac La Peyrère (1596–1676).Smith, Justin. 2015. Nature, Human Nature, and Human Difference.

Zhang, Guihong

The Forming of Information Worldview and Its Value Dilemma

Abstract: When Norbert Wiener was thinking about the social impact of cybernetics, he constructed a world view based on cybernetics and information theory: the information world view. He focused on analysison the social and ethical implications of cyberneticsafter World War II. As early as 1948 in his "Cybernetics", he not only described the main ideas of his new science, but also described the social significance of these ideas, and published the book "The Human Use of Human Beings" in 1950. Additionally, in conferences, public lectures, and interviews, he has extensively discussed the social and ethical issues that may arise from cybernetics and computer. With extraordinary foresight, he predicted many features of today's information age. From today's perspective, we can see that Wiener is not only a major participant in the information technology revolution, but also the founder of the field of "information and computer ethics." He inadvertently constructed a far-reaching information worldview, a new explanation of the nature of human nature, society, robots, and even the nature of the universe. Driven by later philosophersof information, the information worldview theory is gradually improving. However, in explaining human values and ethical issues, theyhave encountered problems such as how to explain the origin of value. With in-depth discussions on these issues, value issues have become a new importantdebate in information ethics. Although Wiener explained the information nature of the world and proposed "natural evil" based on entropy increase. . However, when it comes to the issue of human values, it cannot explain wherehuman values comes from. Facing with this dilemma, inter-culturalists have proposed more practical solutions, which aremore useful strategy at present.