Abstracts
Résumés

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INTERNATIONAL KEYNOTE SPEAKER (OPEN SESSION)
TUESDAY, JUNE 1, 14:00 – 15:30

THE CIRCULATION OF MATHEMATICAL KNOWLEDGE IN 19TH CENTURY INDIA AND EUROPE
Dhruv Raina Jawaharlal Nehru University

The history of calculus is often treated in terms of certain canonical texts, rather than engaging with the elaboration of the procedures of calculus in its various contexts. Attention to these contexts reveals the origins of calculus across a wide variety of cultures, regions and actors. This talk argues that we are really engaging with calculus not as a 'thing' but as a set of mathematical concerns, addressed in ways that 17th century mathematicians in India and Europe might have understood the mathematical problems they were pursuing with different research programmes.

STILLMAN DRAKE LECTURE
SUNDAY, MAY 30, 14:00 – 15:30

GALILEO AMONG THE GIANTS
Anita Guerrini Oregon State University

The second day of Galileo's *Two New Sciences* (1638) discusses size, shape, and scale. He argues that there are physical limits to the size and shape of a cylinder or prism, beyond which it must collapse of its own weight. In nature, an enormous giant would require bones that were so large and heavy that he would be crushed by their weight. Galileo illustrates two bones: one a normal human femur, the other three times longer and proportionately broader and thicker.

Galileo did not choose either the example or the image by chance. Giants, particularly giant fossil bones, were a topic of discussion among the Academy of the Lynx, and Galileo's image resembles a giant bone found in the south of France 20 years earlier. This talk explores these discussions and Galileo's continued communication with members of the Lincei.

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-A-

The Role of Popular Science in the Formation of Expert Knowledge. Observations About Ludwik Fleck's Epistemology

Kamil Cyprian Aftyka Standford University

Often, modern sciences are accused of being too specialized and hermetic. Scientists are told they must popularize among laymen the knowledge they gain. In my paper, I focus on the other direction of the exchange. On how popular science and, more broadly, "exoteric" means of communication influence the development of the scientific, "esoteric" concepts. The contribution of Ludwik Fleck, Polish philosopher and microbiologist, to the reflection on this topic will be instrumental given that it was not only sociological but also a key element of the epistemology he developed before the second world war. His thesis that there is a truly bidirectional relation between esoteric and exoteric circles of knowledge is part of an inquiry into how truths and conclusive results can be reached in the reality in which "subjective experiences" and "objective facts" are interdependent. It is through social cognition, "thought-collectives," that this interdependence obtains a concrete shape. Those thought-collectives that are rational have an ability to work through and liquidate their founding errors. To do this they must have an ability to grasp and relativize the history of their concepts. In contrast, the progress in programmatically ahistorical thought-collectives occurs only accidentally, intuitively, magically. For instance, as I will argue, by the power of suggestion more exoteric circles have on the certitude scientists gain about general concepts. Importantly, the rational thought-collectives will take advantage of the cases in which the non-expert collectives guide sciences in right directions but will be aware when they do not.

The Sound of the Aurora Borealis: Discussion and Controversy in the First and Second International Polar Years

Fiona Amery University of Cambridge

The possibility of the aurora borealis transmitting sound to the surface of the earth was a contentious and much discussed issue in both the First and Second International Polar Years (IPY), of 1882-3 and 1932-3 respectively. Beginning in the late nineteenth century, survey answers, letters and oral testimonies were collected from the local inhabitants of northern Canada, the Shetland Islands and Norway, revealing that many had experienced 'swishing' and 'crackling' sounds, accompanying violent auroral displays. Captain Dawson, leader of the First IPY British Fort Rae expedition, recollected having once heard a similar noise while on auroral duty. Yet, within both international programmes the communal practice of auroral scientists was for the most part sceptical, with many linking the debate with the undetermined altitude of aurorae as well as the illusory properties of the northern lights. In the 1920s and 1930s, the contentious discussion drew the attention of Sydney Chapman, C. G. Simpson and C. A. Chant, each of whom had a distinct position on the reality of the aural phenomenon. This overlooked episode within the history of the IPYs speaks to the perceived trustworthiness of indigenous testimony in the periods, the co-operative and standardised process of transferring information across continents and the sensory register of embodied hearing utilised in the Polar regions. The problem of establishing the reality of auroral sound has been little discussed since the 1930s, either in primary or secondary literature, and only a tentative explanation for the phenomenon exists today.

Too Complex to Predict? What Complexity Theory Reveals About Natural Kind Analysis in Psychiatry

Derek Andrews Dalhousie University

In his work on psychiatric classification and natural kind analysis, Jonathan Tsou argues that some mental disorders ought to be recognized as belonging to natural kinds, owing to the fact that they represent classes of abnormal behaviour underlain by stable causal mechanisms (Tsou, 2016). The presence of the stable causal mechanisms underlying the disorders Tsou identifies, such as schizophrenia (2016) and depression (2013), enable reliable projectionable inferences to be made about members of these kinds – that is, persons with these disorders (2013; 2016). Accordingly, this move, contends Tsou, would positively impact our ability to prognosticate about and treat persons with these mental disorders (2013; 2016).

However, I argue that approaching psychiatric conditions from the vantage point of complexity theory, as developed by Bechtel and Richardson (2010), reveals a significant problem for Tsou's position. Given the immense complexity of biological systems and diseases alike, it seems that any particular factor involved in the expression of a given mental illness, such as the stable causal mechanisms Tsou identifies, can be affected by the myriad other factors in play (Phillips, 2013). This results in a picture of immense complexity, wherein no two individuals with the same psychiatric disorder will have the same causal picture underlying its expression (2013). Accordingly, it seems that Tsou's analysis does not afford us the ability to make reliable inferences about members of mental disorder kinds, as it does not take into account the full causal story at work in the expression of individual cases of mental disorders.

''Pour l'amélioration de la race humain'': The Reception of Eugenics in French-Canadian Press, 1912-1921

Vincent Auffrey University of Toronto

In September 1912, French-Canadian gynecologist Albert Laurendeau made an energetic plea in the radical Montréal-based newspaper Le Pays in defence of what he called "l'eugénique." This was the first time the newly translated term "eugenics" was used in French-speaking Canada. Interest in the topic was initially aroused when news of the First International Eugenics Congress – held in London between July 24 and 29 – reached Montréal in the summer of 1912. While Laurendeau soon abandoned his public endorsement of eugenics due to conflict with the Catholic Church, Le Pays kept publishing until the early 1920s. Its promotion of eugenics sparked intense debate with the ultramontane Catholic press in Québec City. My research sheds light on the importation and reception of eugenics in Québec by examining French-Canadian newspapers published between 1912 and 1921. If historiography on eugenics in Canada depicts French Canadians as apathetic or hostile towards eugenics due to their religious convictions or presumed scientific backwardness, this paper emphasizes the internal struggle that pitted French-Canadian eugenicists against their ideological opponents. It expands on existing historiography by illustrating why this first wave of French-Canadian eugenicists was not more successful at propagating their views in French-speaking Québec.

-B-

Big-Data and Integrated HPS: Towards a Database of Intellectual History

Hakob Barseghyan University of Toronto, Victoria College **and Nichole Levesley** University of Toronto, IHPST

In this paper, we introduce an original methodology for the extraction of data on beliefs, values, and practices of historical agents through AI-human interaction. The main output of this process will be an AI-assisted and expert-verified database of intellectual history that will present an up-to-date synthesis of the scholarly literature on historical beliefs and practices. This open access database will be publicly available by means of a website featuring a variety of tools for data visualization and analysis. By highlighting the plurality of extant scholarly views and revealing potential gaps in the current historical record, the database will provide a focus for future research. In addition, by analyzing and evaluating the concepts extracted by our deep-learning AI, we will advance our current ontology of historical epistemic agents, stances, and elements. This will provide scholars with a more precise, comprehensive, and robust framework for gathering and interpreting historical data. This big-data approach will allow us to detect patterns and test hypotheses concerning different aspects of scientific change both at the individual and communal levels. This will enhance our understanding of the structure and dynamics of scientific change with a robust body of historical evidence. Moreover, it will serve as a powerful tool for historians and philosophers to discern additional historical trends and evaluate hypotheses, fostering pluralistic frameworks and accommodating a variety of research methodologies.

Integrating-for-Purposes: The Aims-Based Approach to Scientific Integration in Biology

Alican Basdemir The University of Calgary

Scientific integration is a practice of combining various scientific activities including modelling, explaining, and data-analysis. It is fundamental to the joint scientific work that allows scientists from different disciplines, fields, and sub-fields to coordinate and communicate their activities. Integrative pluralism maintains that scientists use multiple explanations to represent complex phenomena, but scientists intend to integrate different representations to make them compatible with each other (Mitchell, 2002). Against this view, I claim that integration is not always desired or achieved in all contexts so that there is a need to examine how integrative activities succeed or fail in relation to the aims that scientists pursue. Drawing upon Brigandt (2010), I propose the integrating-for-purposes view stating that integration is not an overarching aim of science, but it is an activity that is associated with the achievement of various purposes in specific scientific contexts. I claim that integrative activities involve the use of multiple scientific activities that are associated with certain aims that cannot be achieved by individual activities. The literature falls short of emphasizing the different aims allowing, constraining, or precluding integrative activities To fill this gap, I will provide the

conditions of success and failure by examining different aims that are associated with integrative activities in structural biology. I will articulate some cases of failure in which (i) scientists prioritize aims that are not associated with integrative activities, and/or (ii) the presence of some constraints prevents integrative activities. This aim-based framework is intended to capture the practice of integrative better by examining the aims underpinning the integrative research in biology.

Justifying inferences: the case of paleodiversity measurements and the contemporary biodiversity crisis
Federica Bocchi Boston University

Estimating whether the Earth's biota is in the middle of a crisis relies heavily on comparisons between present and past data about biodiversity or biodiversity surrogates. In this paper, I compare measurements of biodiversity to measurements of paleodiversity, and the conceptual frameworks that guide such measurements. My intent is to understand whether commonly used inferences from paleodiversity measurements to biodiversity estimates are epistemically well-motivated. I claim that justifying such comparative evaluations (e.g. using paleodiversity data to show we are currently facing a biodiversity crisis) is harder than it appears. I argue that paleodiversity measurements are incommensurable with contemporary measures of biodiversity, given the different ways that biodiversity is conceptualized, and quantified accordingly. Specifically, unlike current biodiversity measures, paleoestimates rely heavily on an understanding of past diversity as species counts, like the famous Sepkoski diversity curve of marine invertebrates. But the understanding of current biodiversity is complex, dynamic, not ultimately reducible to species inventories. I call this mismatch the "incommensurability problem". I do not argue that paleodata are useless in conservation efforts, but that paleodiversity is not directly commensurable to estimates of contemporary biodiversity and loss without additional qualifications. I conclude by proposing a possible way of overcoming this incommensurability problem inspired by Santana (2014). Using Santana's strategy, the justification of claims such as "we are in the middle of a crisis" is ultimately to be found in an anthropocentric arbitrary loss of ecosystem services.

Representational Practice of Representative Sampling in Public Health Surveillance Systems
Brandon Boesch Morningside College

Philosophical discussions of scientific representation are typically offered primarily to explain how it is that a scientific model is representational (see, e.g., Hughes 1997; Bailer-Jones 2003; French 2003; Giere 2004, 2010; Contessa 2007). While these studies are valuable, there are other contexts in which representational practice is found in science. In this paper, I will analyze a case study from public health to understand the representational nature of representative samples. Specifically, I will examine the Youth Risk Behavior Surveillance System (YRBSS), used to monitor health-related activities among youth in the United States every other year since 1991 (Brener et al. 2013). I shall argue that representative samples are representational because of the ways in which they are licensed (Boesch 2017)--how they are collected and used by scientists, especially in virtue of the (1) methodology of collection, which structures the sampling practice to eliminate risk of bias and sampling errors; (2) the influence of theory in the continued development of and changes to the measurement tool; and (3) the constraint of representational aims due to awareness of shortcomings.

Molecular models as tools for gaining information about protein and DNA structure
Agnes Bolinska University of South Carolina

How did building molecular models contribute to Linus Pauling's discovery of the alpha helix and Francis Crick and James Watson's determination of DNA structure? Bolinska (2018) argues that a components-first strategy for determining molecular structure, in which bond types, lengths, and angles are generally taken into consideration before whole-molecule information derived from X-ray diffraction photographs, was likeliest to lead to the correct structure more efficiently than the photos-first strategy, which reverses this order. Further, she suggests that building molecular models was one way in which such a strategy can be applied. In this paper, I extend this suggestion. I argue that constructing molecular models gave scientists a concrete way to prioritize bond types, lengths, and angles in their reasoning. The pieces from which models were constructed physically instantiated such component-level constraints, and physical properties of the structures constructed from them precluded their violating certain stereochemical rules. Molecular model construction therefore ensured enforcement of these rules. Further, molecular models functioned as cognitive aids, enabling scientists to more efficiently apply constraints in the elimination of candidate molecular structures.

Discovery and Instrumentation: How Surplus Knowledge Contributes to Progress in Science
George Borg University of Pittsburgh

Drawing on the ideas of Marx, Engels, and more recent historical materialists, I will provide an explanation of scientific progress by relating science to the more general practice of laboring. An important fact about human labor is that it can result not just in reproduction of what it started with, but in something new, a surplus product. When the latter is a means of production, it makes possible a mechanism of change consisting of reproduction by means of the expanded means of production. "Means of production"

must here be understood in a broad sense, to include not just tools in a narrow sense, but also material means of representation and communication (Lefèvre 2005). Each iteration of the labor process can differ from the preceding one insofar as it incorporates the surplus generated previously. Over the long-term, this cyclical process can lead to the self-transformation of labor and, through it, of human societies and cultures.

In this paper, I will provide a largely theoretical argument that this mechanism of change is also at work in the history of science. More specifically, the thesis I will defend in this paper is that surplus knowledge contributes to progress in science. The basic argument is this. Labor makes progress by producing surplus use-values (objects of utility). Science makes progress as does the labor process, except that the specific use-value that it produces is knowledge. Therefore, science makes progress by producing surplus knowledge.

The paper is structured as follows. In section 2, I argue that the form taken in science by the mechanism of reproduction by means of the expanded means of production is that of a feedback loop between discovery and instrument construction. This process requires the integration, and transformation into material form, of different kinds of knowledge. In section 3, I argue that this process suggests a concept of scientific progress complementary to those that have so far been advanced in the philosophical literature on scientific progress, and defend the concept of progress as transcendence of the limitations of native human epistemic ability. In section 4, I criticize narrowly biologistic approaches to the history of science for ignoring the role of surplus generation in transforming the labor process, and discuss some problems associated with viewing science as labor. I offer concluding remarks in section 5.

How to Properly Investigate Human Cognitive Difference and Diversity?
Ingo Brigandt University of Alberta

In neuroscience and cognitive science, there already are different scientific approaches to investigate human cognitive difference and diversity. Brain organization theory is largely focused on finding out about sex-differences in the brain, which are deemed to form a biological basis of gendered behaviour. In contrast, cultural psychology and cultural neuroscience is more open to capture a large range of human cognitive diversity, and views cognitive variation as modulated by social influences. Pointing to fruitful methodological resources that already exist in science while also making further methodological recommendations, this talk will discuss how one can and should properly investigate human cognitive variation so as to do justice to human diversity.

My analysis centers on two aspect of methodology, both of which can restrict or bias, but also enhance research: (1) experimental and other practical investigative strategies, and (2) representational and analytical frameworks. Even though there are serious practical and financial limits on investigating human cognitive diversity, I discuss how various analytical categories such as sex, gender, sexuality, race, and culture can be fruitfully be employed. While many approaches study cognitive differences without attempting to offer a causal explanation for them, I argue that the kind of explanatory frameworks that one may pursue can still have a relevant impact on such research.

The Medical Nature of Personality Disorders
Danielle Brown University of Alberta

The so-called 'neo-Szazsian' critique (Zachar, 2011) put forward by Louis Charland (2004, 2006) contends that the Cluster B PDs (Antisocial, Borderline, Narcissistic) are morally-loaded in the sense that the criteria for identifying these disorders contains morally-loaded language and that effective treatment for them would amount to a 'moral transformation' on the part of the patient. On this basis, it is argued that the Cluster B PDs do not constitute properly medical constructs, but rather, moral ones, and thus should not be conceptualized according to a medical model, but rather, should be viewed as moral deficiencies. In this paper, I examine some of the proposed responses to this argument, most notably, the response from Zachar & Potter (2010), who contend that a degree of overlap between the moral and medical domains is unavoidable and that the resources of virtue ethics--in which judgements of health and proper functioning possess an intrinsically moral valence--may be of use in conceptualizing these disorders. In this paper, I attempt to untangle the implications of this response, arguing that while their diagnosis of the problem is correct and that judgements of health and functioning are intrinsically value-laden, their solution functions primarily to justify the inclusion of moral vocabulary in personality disorder diagnosis and underestimates the importance of the notion of 'harm' in diagnoses of illness. This strategy, I argue, further blurs the boundaries between disorder and social or moral deviance and has implications for other DSM diagnoses which are not as amenable to virtue-based re-conceptualizations.

Gilbert Simondon contra Marx and Hegel: Alienation and Analogical Understanding
Alex Bryant McMaster University

In On the Mode of Existence of Technical Objects (2016 (1958)), Gilbert Simondon makes a project-wide critique of the Marxist identification of alienation under industrialized capitalism with the economic relation of human beings to technical ensembles. More difficult to analyze in Simondon's text, however, is his rejection of a Hegelian notion of dialectical progress which folds in a rejection of Marxist theories by way of the dialectical progression of history found in Marx's historical materialism. In this paper,

I tease out the substance of each of these critiques and show that while Simondon has a mistaken reading of Marx that his response to the Marxist account of alienation is nevertheless valuable in light of his own work on technology.

The question, as Simondon points out, is "What is left?" where dialectic necessity is removed as the "engine" of historical progress in a theory of technological development (176). Simondon's analogical account of the relation between the human being and the technical object, I suggest, constitutes the alternative which Simondon draws out repeatedly across the text as he proceeds from implicit treatment of the analogy to a more explicit and speculative discussion. Unlike dialectical thought, he argues, progress in understanding through this analogical method proceeds without the destruction of the preceding term (201), through the identification of structural similarity between each term. Hence Simondon's suggestion that philosophical thought must contribute to the cultivation of analogical reasoning.

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Valuing Population Health Science
Sarah Clairmont McGill University

Throughout the twentieth century Western medicine embraced – theoretically and empirically – an individualistic biomedical perspective, known eventually as the biomedical model. The influence of the biomedical model in "the West" really cannot be understated: it has developed into a system of medicine, governing everything from the production of medical knowledge to how that knowledge is implemented in clinical practice and public policy (Krieger, 2011). In fact, the terms "biomedicine" and "Western medicine" are still used interchangeably (Wikipedia, Biomedicine; NCI, Dictionary of Cancer Terms). Population health science developed in reaction to certain undesirable features of the biomedical model, namely: the model's strong emphasis on biological (as opposed to social) determinants of health and its reductionist methodology. This paper argues that the central point of entry into population health science for philosophy is through the epistemological and normative questions raised by feminist and values critiques of science (e.g., Longino, 1990; Douglas, 2004; de Melo-Martìn and Intemann, 2011).

Population health science is an emerging interdisciplinary field that studies both the macrosocial determinants of health distribution within and across populations, as well as the biological mechanisms through which these conditions manifest in the health of individuals (Keyes and Galea, 2016). As a new and rapidly developing field of inquiry, population health science faces a number of conceptual and methodological challenges—yet it has received almost no attention from philosophers (an exception is: Valles, 2018). Indeed, there are important themes within the philosophy of science that have not yet been brought to bear on population health science. First and foremost, population health science openly acknowledges its social structure and so lends itself to debates within social epistemology and the philosophy of science about the ways in which social values comes to bear on scientific knowledge.

I begin (§I) by introducing population health science, especially the population health perspective, by contrasting it with key assumptions of the still dominant biomedical model. Second, (§II) I identify and respond to some of the more pressing issues raised by the values system operating within population health science such as defining and interacting with vulnerable groups (e.g., racial or gender groups) and managing the trade-off of the values of equity and efficiency when evidence is translated into policy recommendations.

The Theory of Knowledge and the Education of the People: Unified Science as Social Epistemology
Bianca Crewe The University of British Columbia

Early 20th century logical empiricism initiated a reform of the philosophical method in service of explicitly social goals. The manner and extent to which logical empiricism and the Vienna Circle produced a political philosophy of science has been the focus of recent interpretive debates. Relatedly, the historical context of logical empiricism, and particularly its instantiation in Cold War America, has been presented in the secondary literature as shaping the political alignments and possibilities of logical empiricism in important ways.

In this literature, Hans Reichenbach's account of reason lies at the centre of an interpretive disagreement: one the one hand, it is argued to be paradigmatically indifferent to context and social dynamics in a way that is implicitly supportive of capitalist ideology. Others, however, see Reichenbach as confronting the embeddedness of political and epistemic agents in a manner unique among his contemporaries. Though I find the latter interpretation persuasive, I wish to situate Reichenbach's account of rationality and the knowing subject in contrast to the theoretical presuppositions of Marxist philosophy of science so as to address what I take to be a deeper methodological and ideological divide, and a different way of thinking about what John McCumber refers to as "the politics of reason." Specifically, Marxist philosophy of science offers a distinctive vision of rationality and the relationship between knowledge and social processes, and an explicitly political and robustly social account of science. Contrasting this with Reichenbach's account highlights some of the constraints and assumptions involved in theorizing the socio-political and non-ideal elements of inquiry from the perspective of analytic philosophy of science.

The Open Systems View
Michael Cuffaro Munich Center for Mathematical Philosophy

There is a view in philosophy and physics according to which systems are conceived of, fundamentally, as closed (i.e., perfectly isolated from their environment). This is the _closed system view_. It has become deeply entrenched, having been made more precise in the Hamiltonian and Lagrangian formulations of classical mechanics, and (via Noether's Theorem) intimately related to the existence of symmetries and conservation laws. Although not without problems, it has been extremely successful, and is even applied in cases where a given target system is actually not isolated. While we do not deny its successes, we argue against taking the closed system view as fundamental either in physics or in philosophy. Instead, we propose the _open systems view_. On this view, to fundamentally describe a thing is to describe it as coupled to its external environment. We motivate the open systems view by focusing on the role of _framework idealisations_ (idealisations made at the level of a theoretical framework that are applicable to all systems described by it) in standard quantum mechanics and in the general quantum theory of open systems (whose fundamental equation of motion is the Lindblad, not the Schrödinger, equation). We consider two notions of fundamentality: ontic fundamentality, relating to the objects of a theoretical framework, and epistemic fundamentality, relating to our knowledge of those objects. We argue that the open systems view is more fundamental than the closed systems view in both senses, and that this has important implications for physics, the philosophy of physics, and metaphysics.

Computational Pluralism
Andre Curtis-Trudel The Ohio State University

One job for a philosophical theory of physical computation is to capture how computational notions are applied to physical systems. Among other things, this involves identifying physical features relevant for individuating computational states. Recently a few philosophers have advocated for a pluralist approach to this problem. Crudely, the pluralist holds that computational states are individuated by different physical properties in different cases. There is disagreement about how far pluralism goes, however. More conservative views hold that just a few different physical properties impact computational individuation, while more liberal views allow that a wider variety of physical property can play an individuating role. This paper defends an extreme liberal view, according to which any physical property can impact computational individuation, at least in principle. There are two main motivations for extreme liberalism over its more conservative counterparts. First, it is not clear that more conservative views capture the wide variety of systems investigated in the computational sciences. Second, extreme liberalism respects deep analogies between computation and other applications of mathematics to physical systems. Just as mathematical notions quite generally can be applied to physical systems, whatever their physical properties, so too can computational notions be applied generally, whatever their physical properties. Or so I argue.

-D-

Vague Dynamical Ontological Models of Quantum Theory
Thomas De Saegher University of Western Ontario

Certain interpretations of quantum theory rely on the dynamics of quantum state ontologies, specified with a degree of fuzziness, to provide a supervenience base for determinate experimental outcomes. From this class, I isolate what I claim is common to how the empirical content of quantum theory is captured in both Wallace's Everettian interpretation and the Myrvold-Pearle approach to constructing collapse theories. While these interpretations take the quantum state to be descriptive of the physical state of a single prepared system, their common framework of approximate dynamical recovery deviates slightly from the ontological models framework used to argue for psi-ontology in the first place. After defining the new framework of "vague dynamical ontological models", I adapt the PBR argument for psi-ontology to this setting. I then adapt another argument constraining the exact representational relationship between the quantum state and the ontic state, originally made in the ontological models context by Montina (2018), to my new framework. Similar to Montina, I find a lower bound to the dimension of the vague ontic state space required to dynamically recover the behavior of macroscopic degrees of freedom. However, the ontic dimension does not depend linearly on the dimension of Hilbert space for the system under consideration and I discuss some lessons that follow for how the quantum state must represent in vague dynamical ontological models.

Relative Significance Controversies in Evolutionary Biology
Katie Deaven University of Wisconsin-Madison

Scientists engage in relative significance controversies when they investigate the importance of a cause in producing a phenomenon of interest. Some philosophers have questioned the epistemic value of engaging in these controversies. In this paper, I present a

taxonomy outlining the conditions under which questions of causal importance may arise. I then show how engaging in these different kinds of controversies may inform scientist's research. Using the historical examples of the neutralist-selectionist controversy in molecular evolution and the group selection controversy, I illustrate how these controversies help scientists form predictions about new instances of the phenomenon of interest, refine our understanding of causes of the phenomenon of interest, and improve upon explanations of causal structures.

The Case for Final Causes in Life Science

Lane DesAutels Missouri Western State University

Aristotelean final causes have long been characterized as overly teleological and as such unwelcome in post-vitalist, mechanistic accounts of life science. After all, how can something in the future cause something in the past? In what follows, I argue that there are indeed legitimate roles for Aristotelean final causes in contemporary life science. To understand these roles, however, we must first uncover the function final causes play in scientific explanation characterized as modal rather than ontic. When final cause explanations are properly understood as modal explanations, we can see that they are indispensable in evolutionary, developmental, as well as molecular biology. If this strategy is successful, it opens the door to a new way of vindicating teleology in natural science more broadly.

A More Diverse Universe: Identity and Inclusion in Modern Astronomy

Jörg Matthias Determann Virginia Commonwealth University

Astronomy is usually concerned with matters very distant from Earth. Most phenomena, whether observed or theorized, transcend human spaces and timescales by orders of magnitude. Yet, many astrophysicists have been interested not just in events "a long time ago in a galaxy far, far away," but also in their society here and now. Since the 1980s, an increasing number of them have pursued parallel careers as academics and activists. Besides publishing peer-reviewed papers, they have promoted a great variety of underrepresented groups within their discipline. Through working groups, conferences, newsletters and social media, they have sought to advance the interests of women, members of racial and ethnic minorities, LGBT and disabled people. While these activists have differed in the identities they focus on, they have come to share a conviction that diversity and inclusion are crucial for scientific excellence as well as social justice. This paper presents the biographies and institutional contexts of several key agents in the diversification of modern astronomy during the late twentieth and early twenty-first centuries. Because they are recent figures whose discoveries have not been commemorated by Nobel Prizes, they are relatively unknown among historians of science. However, they have been central to discussions about who has privileged access to giant telescopes, huge databases and other expensive resources. As such, they have also significantly shaped views of our universe.

"An Unusual Natural Laboratory": Ethnicity, Deviance and Mid-Century US Settler Colonialism in the Construction of Problem Behavior Theory, 1958-1980

Theo Di Castri Department of History and Philosophy of Science, University of Cambridge

In the late 1950s, an interdisciplinary team of social and behavioral scientists headed by Richard Jessor embarked upon a five-year field study of "deviant behavior" within Ignacio, a small "tri-ethnic" (Hispanic-, Native-, and Anglo-American) community located in Southwestern Colorado. Given the community's small size and its demographic composition, Jessor and his colleagues saw Ignacio as "an unusual natural laboratory" to study the relationship between ethnicity and deviant behavior. The research they conducted in the community laid the groundwork for what has come to be known as 'Problem Behavior Theory' (PBT), an influential theory that continues to inform adolescent behavioral health interventions to this day. This paper argues that, in a kind of imperial boomerang effect, Ignacio served as a kind of internal colonial laboratory in which Jessor and his colleagues articulated a behavioral theory of deviance that they then sought to apply to the US (and later global) population at large. I begin by situating Jessor's so-called "Tri-Ethnic Study" within its settler-colonial context and then trace the trajectory of his theory as the site of his research shifted to the predominantly White suburbs of Boulder and onward to a national sample of adolescents over the course of the 1960s and 1970s.

Abstractionism and Modality

Thomas Donaldson Simon Fraser University

The type-token distinction is a familiar philosophical tool. We might say that two token letters are instances of the same letter type. Or we might say that two similar triangles are instances of the same shape type. Or we might say that all three-membered pluralities are instances of the cardinal number three. Proceeding further into mathematics, we might say that each particular cyclic group of order six (e.g. $\mathbb{Z}/6\mathbb{Z}$ under addition, or the rotational symmetries of the regular hexagon under composition) is an instance of "the" cyclic group of order six.

"Abstractionists" such as Crispin Wright, Bob Hale, and Øystein Linnebo argue that many mathematical entities are abstract types. Fundamental to their account are "abstraction principles", such as the following:

For all x and y, the shape of x is identical to the shape of y if and only if x is similar to y.

Such abstraction principle are usually given in a non-modal form. This is problematic, because such principles are surely supposed to have some modal force, and it would be desirable to make this explicit.

I will explore the modal commitments of abstractionism. In particular, I will suggest that abstractionism is incompatible with "serious actualism" — i.e., the doctrine that only things that actually exist can instantiate properties or stand in relations.

A New, Robust, Method for Weather Event Attribution
Justin Donhauser Indiana University Bloomington

This work builds on previous work showing that the probabilistic and storyline approaches to extreme event attribution are compatible and complementary. We start with a case-study of attribution studies of the 2013 Boulder Colorado flood. Through discussion of the three successive attribution studies on this event, we reinforce the view that the probabilistic approach is good for answering some sorts of questions and making a sort of inferences that the storyline approach fares worse at, and that the storyline approach is good for answering other sorts of questions and making sorts of inferences that the probabilistic approach cannot. Our analysis highlights how each approach has different standards of evidence and leans opposite directions in admitting of Type one or Type two errors, and that using each approach as compliments thus makes up for the deficiencies of one another and provides more informative and robust models. We then look to key historical works on modeling that have shaped the development of the theory and methods used in probabilistic and storyline modeling to motivate a unifying and overarching approach, Robust Event Attribution.

Discovering Side Effects and Evaluating Efficacy in Post-Market 'Phase IV' Pharmaceutical Trials: Exploratory or Confirmatory Experiments?
Austin Due University of Toronto

'Phase IV' post-market pharmaceutical trials reveal a tension in the distinction made by philosophers of science between confirmatory and exploratory experimentation. Philosophers have been right to point out that there is no hard line between these in research programs, and that confirmation and exploration lie along continua and dimensions. However, one of the core tenets of exploration – that no hypotheses are tested – precludes singular cases of experiments jointly exploratory and confirmatory. I argue here that some 'phase IV' trials can test epistemic hypotheses and predictions about a target phenomenon – the efficacy of a novel pharmaceutical demonstrated in 'phase III' randomized control trials – while knowingly exploring for potential signals of harmful side effects. I address some initial counters about the confirmatory power of non-randomized observational trials that constitute these 'phase IV' trials, as well as considerations around whether or not 'phase IV' trials are really singular experiments. I argue that they are, and that these experiments are exploratory in virtue of their confirmatory aims. Thus, a reevaluation of what is fundamental about 'exploration,' e.g. that it does not involve the testing of hypotheses, is warranted. In doing so, a clearer, non-diachronic relationship between exploration and confirmation is revealed.

-E-

Principled Mechanistic Explanations in Biology: A Case Study of Alzheimer's Disease
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Following an analysis of the state of investigations and clinical outcomes in the Alzheimer's research field, I argue that the widely-accepted 'amyloid cascade' mechanistic explanation of Alzheimer's disease appears to be fundamentally incomplete. In this context, I propose that a framework termed 'principled mechanism' (PM) can help with remedying this problem. First, using a series of five 'tests', PM systematically compares different components of a given mechanistic explanation against a paradigmatic set of criteria, and hints at various ways of making the mechanistic explanation more 'complete'. These steps will be demonstrated using the amyloid explanation, and its missing or problematic mechanistic elements will be highlighted. Second, PM makes an appeal for the discovery and application of 'biological principles' (BPs), which approximate ceteris paribus laws and are operative at the level of a biological cell. As such, although thermodynamic, evolutionary, ecological and other laws or principles from chemistry and the broader life sciences could inform them, BPs should be considered ontologically unique. BPs could augment different facets of the mechanistic explanation but also allow further independent nomological explanation of the phenomenon. While this overall strategy can be complementary to certain 'New Mechanist' approaches, an important distinction of the PM framework is its equal attention to the explanatory utility of biological principles. Lastly, I detail two hypothetical BPs, and show how they could each inform and improve the potentially incomplete mechanistic aspects of the amyloid explanation and also how they could provide independent explanations of the cellular features associated with Alzheimer's disease.

Paulin J. Hountondji on Science, Capitalism, and Imperialism: An African Philosopher's Quest for a Sociology of Scientific Knowledge in the Peripheries

Zeyad El Nabolsy Cornell University

In this paper I argue that one can identify a coherent theoretical view of the relationship between science, capitalism, and imperialism in the works of Paulin J. Hountondji. Hountondji claimed that colonialism by bringing about a class of dependent capitalists and by creating dependent economies hindered the development of an autonomous scientific discourse on the African continent. Thus he points out that rather than attempting to theorize the relationship between science, technology, and capitalism in general, it is necessary to develop a theoretical framework that allows one to study the relationship between science, technology, and capitalism in the peripheries, where capitalism itself had a distinctive structure that differed from the "classical capitalism" of the metropolitan countries. I aim to provide a detailed reconstruction of Hountondji's claim that capitalism hindered scientific development in the formerly colonized world, and the manner in which he draws upon historical materialism as a theoretical framework in order to substantiate this claim. I also show how he deploys historical materialism in order to launch a critique of culturalism with the aim of showing that strictly speaking it is fallacious to equate "modern science" with "Western science" (or "European science"). I argue that the motivation for this attempt at making a conceptual distinction between "modern science" and "Western science" is the desire by Hountondji to articulate the possibility of an anti-colonial (and anti-neocolonial) modern science. Moreover, I argue that Hountondji's respect for modern science and his desire to develop an autonomous modern scientific discourse on the African continent is not incompatible with respect for indigenous (or, as Hountondji prefers, "endogenous") knowledge. However, it is incompatible with respect for "endogenous knowledge" as it is understood within the framework of the ethnosciences (i.e., ethnobotany, ethnomathematics, etc.), which according to Hountondji present the former as a petrified system of knowledge that is isolated from modern science. Instead Hountondji argues that it is necessary to integrate endogenous knowledge with modern science in order to develop a scientific discourse that will contribute to the emancipation of African countries from neo-colonial tutelage.

Following Climate Science Towards Greater Emissions: Framing and Subversion in the Trump Administration's Assessments for Environmental Policy

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In March 2020, the Trump administration rolled back tailpipe emission standards for vehicles, overturning the Obama administration's keystone policy to mitigate climate change. The new Safer Affordable Fuel-Efficient (SAFE) Vehicles Rule constitutes the most significant undoing of US climate policy in recent years, and will form part of a challenging environmental legacy for the next administration. Beyond the political legacy, the scientific assessments that underpin such deregulatory actions pose their own challenges to philosophers interested in the science-policy interface. In particular, familiar charges of bias and anti-science directed at the Trump administration suggest that the reason its scientific assessments align with deregulatory policy is due to epistemic corruption: non-epistemic values have transgressed their legitimate roles, leading to epistemically distorted science that suits the administration's agenda. In this paper, I argue that the dominant charge of bias leads us astray. Drawing on recent philosophical conceptions of bias, I argue that the assessment underlying SAFE does not fit the bias charge. The alternative analysis I develop focuses instead on framing effects. I argue that the framing of an otherwise reliable assessment sets up the reader to fail morally – the science communication morally subverts the reader. In essence, SAFE's environmental assessment admits that the policy will exacerbate serious climate impacts but induces the reader to discount the ethical import of this result. The complex subversive communication deployed in the assessment offers a compelling case study in the ethics of framing, and highlights an important shift in contemporary deregulatory discourses on climate change.

The Lone Wrangler: Dorothy Wrinch's Original Contributions to Set Theory in the 1920s

Landon D. C. Elkind University of Alberta

Besides earning first-class ("Wrangler") status on the Mathematical Tripos and writing a thesis in logic under Hardy's supervision (with Russell informally advising), Wrinch is one of the few contemporaries of Whitehead and Russell - perhaps the only one - to creatively develop of Principia's advanced parts using its notation. In the 1920s, she made original developments of Principia's Volumes II-III in four papers.

Despite her connections with influential twentieth-century mathematicians, her creative mathematical contributions are usually omitted in histories of set theory. Histories mentioning Wrinch's mathematical work treat it briefly, even though notable contemporaries like Fraenkel and Sierpinski cited and discussed her work. The result is that we lack a satisfactory explanation of what Wrinch showed, how she proved it, and her role within the set theoretic context in the 1920s.

In this paper I explain what theorems Wrinch established and how she proved them. Wrinch showed that the existence of mediate cardinals (Frege-infinite but Dedekind-finite) was equivalent to the Axiom of Choice by generalizing the theory of mediate cardinals, a result that also indicates how cardinals behave when Dedekind's and Frege's characterizations of the infinite are inequivalent. Then I discuss the significance of her work within the set theoretic context of the 1920s. I conclude by considering why Wrinch's work was omitted in later histories, and, perhaps most egregiously, why Russell, her former teacher and adviser,

never mentioned her developments of Principia in print, despite advertising the comparatively elementary work in truth-functional logic of Nicod and of Sheffer.

-F-

What Would Imaginary Ancestors Do?
Margaret Farrell University of California, Irvine

Understanding the evolution of human thinking is an important goal of evolutionary biology. Direct empirical evidence of ancestral hominin cognition is limited, and what is available is difficult to interpret. This makes reconstructing the evolutionary history of human cognition especially challenging. In his 2014 book A Natural History of Human Thinking, Michael Tomasello contends with this problem by approaching the evolution of human cognition from foundations in comparative biology. Despite some advantages, Tomasello's account has a weakness in his use of thought experiments as plausibility arguments when empirical evidence is unavailable. The plausibility of these thought experiments rests on knowledge we already have about the ease of solving familiar cognitive problems. But this is a poor foundation for assessing the thinking of hominins living before the evolution of the capacities we use to solve those problems, making the use of such thought experiments to garner plausibility inappropriate. Tomasello has shown himself to be very concerned with such anthropomorphic influences. An earlier episode in the study of primate cognition reveals that not only were he and his coauthors sensitive to the issue of human cognitive experience impacting experiments on non-human primates, they critiqued another research team on these same grounds. I argue that the persistence of this tendency to project familiar cognitive experience onto early humans, even in scholars highly attuned to this problem, along with the inherent limitations of thought experiments about extinct organisms, are grounds to eliminate their use as plausibility arguments in the evolutionary history of human thinking.

Science, Post-Truth, and Superficial Democracies: How Social Psychology Undermines Kitcher's Solution for Climate Change
Faeze Fazeli University of Notre Dame

In his "Can We Sustain Democracy and the Planet, Too?" (Kitcher, Philip. 2020), Kitcher claims that the problem of global warming can be viewed as a particular case of the problem of superficial democracy. He believes that the root of the problem of global warming denial is misinformation and ignorance. The most troublesome source of ignorance in Kitcher's view is the one when ignorance is systematically and deliberately generated by powerful individuals to serve their economic and political interests.

While acknowledging the negative role of ignorance, I argue that Kitcher's solution for addressing global warming and saving democracy is wanting. In other words, I argue that dismantling of the systematic and institutional production of ignorance in the society and distributing knowledge among the public cannot solve the problem of the superficial democracy. To do so, I rely on recent developments in the psychology of reasoning and some applications of dual-process theories (Evans, J. St. B. T., & Frankish, K. (Eds.) 2009) in the moral and social psychology to provide some solutions. I show it is quite unlikely to change many climate change deniers' opinion by providing them more information and knowledge. This is because their opinion on this matter is not the conclusion of an argument based on some false premises such that if we "fix" the premises the conclusion will change (Graham et al. 2011; Haidt 2013). Rather, it is an intuitive judgment, a gut feeling that cannot be simply changed by providing new information.

A Mummery of Bondage: Siri and the Subversion of the Master/Slave Dialectic
Jennifer Jill Fellows Douglas College

Unesco released a report in 2018 making the case that Siri, Alexa and a number of other virtual assistants were sexist. They claimed that the reason for the sexism in these programs was implicit bias on the part of program developers, who are overwhelmingly male. They called for greater diversity in AI programmers, specifically calling for the hiring of more female programmers, to overcome this sexist bias. But the whole issue, as I will argue, goes much further. I argue that 'virtual assistants' are parodying a master/slave dialectic in order to cultivate epistemic vices in the user thereby ensuring widespread ignorance. The master/slave dialectic is a long-standing model in feminist theory which posits that, while the master has all the power, it is the slave that has access t the knowledge. The master doesn't have to know or care what the slave wants. By contrast, the slave must know what the master wants in order to survive. This creates an epistemic advantage on the part of the slave. Building on this model, feminist theorists have long argued that marginalized groups might or do have epistemic advantages because of their marginalization.

But many within the feminist community note that this epistemic advantage is not a guarantee. People in power can do a number of things to prevent marginalized groups from accessing knowledge. Everything from barring them access to higher education, to making the education itself a type of indoctrination are available to those in power. I explore another tool that has emerged recently:

the master pretending to be the slave. I argue that virtual assistants are submissive to gain our trust and put us in the position of the master. In so doing, big business gains epistemic advantage that is typically found among marginalized groups, while cultivating ignorance among their own consumer base, by placing us in the apparent dominant position. Virtual assistants are not only sexist because of a lack of diversity among the programmers who designed them. They are sexist by design.

The Spacetime View in Feynman's Electrodynamics
Marco Forgione University of South Carolina

The present work offers a historical reconstruction of some of Feynman's main works (absorber theory, path integrals, theory of positron and Feynman diagrams) in light of the evolution of his philosophical intuition: the overall spacetime view. The starting point is the recognition of Feynman's rejection of the "customary view", one in which "things are discussed as a function of time in very great detail" (Feynman 1966, p.7). Afterwards, I reconstruct how the overall spacetime view (which opposes to the "customary view") applies to Feynman's early works on the absorber theory of radiation, where future and past interactions are intertwined and path integrals, where all possible trajectories ought to be considered. Finally, in the theory of positron and Feynman diagrams, all possible particle-field and particle-particle interactions ought to be considered, leading to the final characterization of Feynman's overall spacetime view.

Taking the long view on quantum field theory
Doreen Fraser University of Waterloo

At the moment in theoretical physics there is a pervasive sentiment of frustration about the elusiveness of quantum gravity. I will argue that the history of classical physics contains lessons applicable to the present moment. Drawing on recent historical work on the development of classical physics after Newton by Brading, Stan, Hepburn, and Caparrini, I point out parallels between the state of development of analytical mechanics at the time of Lagrange (late eighteenth century) and the state of development of quantum field theory today. Lagrange took physics as his starting point, but regarded his work as a contribution to mathematics rather than natural philosophy. It was not until the end of the nineteenth century that Lagrangian mechanics had developed to the point that it could be applied to physically interesting problems. One theme of the new historical work on this period is that the development of the mathematical formalism of analytical mechanics supported the development of new physical principles. Similarly, contemporary work by mathematicians on quantum field theory has not yet yielded models applicable to physically interesting systems, but could lead to deeper physical principles. The mature version of Lagrangian mechanics that had been formulated by the beginning of the twentieth century was an important ingredient for quantum mechanics and quantum field theory. By analogy, a future, further developed version of quantum field theory could be an important ingredient for quantum gravity. The history of physics counsels patience.

Agential Autonomy and Biological Individuality
Fermin Fulda University of Toronto

The individuation and differentiation of biological individuals is central to the scientific understanding of living beings. Core concepts of evolutionary biology such as 'inheritance', 'competition', 'fitness', 'growth' and 'reproduction' as well as the varieties of symbiosis such as 'commensalism', 'parasitism', 'mutualism', are predicated on the individuation and differentiation of biological individuals. But what is a biological individual? How are biological individuals individuated? Traditionally, paradigmatic cases of biological individuality include monogenomic animals, plants, and free-living single-celled organisms. Problematic cases include various forms of multicellular aggregates such as eusocial colonies, swarms, ecological communities, and various forms of close symbiosis, such as lichens, the Portuguese Man of War and more recently the host-microbiota symbiosis. Arguments for the putative individuality of some biological entity are often predicated on considerations of autonomy. But what exactly does biological autonomy consist of? I distinguish between three varieties or grades of biological autonomy, each subsuming the previous one and each more demanding than the previous one, including 'causal', 'functional' and 'agential' autonomy. I argue that although we can extract necessary conditions for biological individuality from causal and functional accounts of biological autonomy, only agential autonomy provides a complete criterion of biological individuality. To understand the notion of 'agential autonomy' and to extract a criterion of biological individuation from it, I articulate an account of what natural 'agency' is and how it is instantiated by living organisms.

-G-

On Feyerabend's Theory of Experience: The Empiricist Fluidness and Theoretical Speculation
Deivide Garcia da Silva Oliveira Federal University of Reconcavo of Bahia

This work aims to explain what Feyerabend understands by experience. Specifically, we will explore the consequences and the nature of the idea of fluidness and speculation in his theory of experience. But how are we going to do that? And, does Feyerabend has a theory of experience? By 'theory of experience', Feyerabend is not looking for a system of rules and laws which features are structural and hierarchically established, a fixed category. Notwithstanding, Feyerabend's philosophy is not just a proposal of deconstruction of empiricism. Rather, it is a reasonable account that reconstructs the idea of empirical basis. Hence, a linked second issue, which is in many senses an unknown subject, is that Feyerabend actually developed "a sketch of a new theory of experience" (Problems of Empiricism - part 1, 1965, 186). Notwithstanding, to better understand it, we must look at two fundamentally different interpretations of how Feyerabend took experience. On the one hand, especially because of the titles of some of his papers (e.g., "science without experience" [1969] or "knowledge without foundations" [1961]), which seems to add fuel to the relativistic-anarchistic fire, Feyerabend's philosophy seems to reject experience, empirical evidence to the scientific knowledge. We can say in advance that such a view is not the case. On the other hand, opposed to the previous version, experience is not only important to Feyerabend's methodology, but also to his epistemology, and at least in a practical fashion, indispensable to his ontological approach to scientific knowledge.

Conceptualizing deferential models of expert authority
Kinley Gillette The University of British Columbia

In response to perceived deficits in public trust in science, some scholars have advanced what I refer to as models of expert authority. These are prescriptive models of how candidate experts are to be authorized to function as experts. When someone is authorized to function as an expert, they become an expert authority. The role of an expert authority is to provide decision-makers with advice under specified circumstances and within specified domains, with the understanding that decision-makers will accept the advice they receive. There are different types of models of expert authority. In a deferential model of expert authority, the decision of authorization is itself confined to what Collins and Evans (2002) call the "technical phase" of decision-making and so not subject to democratic control. However, debates about deferential models of expert authority are orthogonal to debates about technocracy or epistocracy and democracy. The latter concern criteria for participation and proper decision-making in the political phase, not the governance of the technical phase. Moreover, debates about deferential models of expert authority are also conceptually distinct from, though connected to, debates about who should be authorized to function as an expert. Assuming the shared goal of authorizing only experts as expert authorities, the latter debates concern social epistemology rather than the governance of expert authority. Conceptually distinguishing models of expert authority—and deferential models of expert authority in particular—leads to a number of critical questions that are distinct from the questions that arise in other debates.

Knowledges of Geography and Geographies of Knowledge: Martino Martini's Novus Atlas Sinensis and the emergence of mathematical cosmography
Gianamar Giovanetti-Singh University of Cambridge

In 1655, as the Jesuit missionary Martino Martini was defending the Jesuits' proselytising strategies to the Congregation for the Propagation of the Faith in Rome, his atlas of East Asia, the Novus Atlas Sinensis, was published in Amsterdam by the renowned Dutch printer and official cartographer of the VOC, Joan Blaeu. This atlas, which constituted the eleventh volume of the Blaeu family's exceptionally influential Atlas Maior, appeared strikingly different from many contemporary maps of the non-European world—particularly those of the Americas. Martini's maps show a standardised iconography of topographical and administrative features such as mountains, rivers, deserts, coastlines, lakes, and cities; they exhibit geometrical details including scale-bars calibrating "Chinese stades" against "German miles," and contain cartouches that for the most part depict the peoples of different Chinese provinces as not enormously dissimilar from Europeans. While historians have rightly emphasised that the Novus Atlas Sinensis played an important role in displacing and transforming Europeans' conceptions of East Asia, this paper suggests that, through Blaeu's efforts to encourage his burgher readership to compare different parts of the world, it effected more profound changes on European conceptions of the credibility of different forms of witnessing and projecting the "Other." The paper traces a genealogy of mathematical cosmography to Martini's atlas, suggesting significant continuities between the missionary's maps and those that came to be associated with the Enlightenment's "modern" cartographic representations of Other territories.

Des hivers Belges: Projets de géo-ingénierie climatique de la Sibérie pendant la Guerre Froide
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Ce papier présente une série de projets technocratiques méconnus imaginés par des scientifiques et ingénieurs soviétiques dans le but de « terra-former la Terre » en modifiant les courants océaniques pour "améliorer" les conditions climatiques en Sibérie orientale. Une proposition consistait ainsi à réorienter une branche du Kuroshiro vers la côte pour accroître artificiellement l'afflux d'eaux chaudes dans la mer du Japon ; une autre envisageait la construction d'un barrage géant sur le détroit de Bering accompagné d'un pompage massif dans le Pacifique pour initier un processus de réchauffement de l'Arctique. A partir de plusieurs sources, ces propositions sont décrites sous leur forme de projets (prétendument) concrets. Ces cas sont ensuite analysés, entre science, technique et environnement, au regard du contexte historique et plus largement culturel de l'Union soviétique, en particulier vis-à-

vis du rôle de l'Etat promoteur de projets à grand échelle de transformation et de « contrôle » de la nature. Plusieurs conclusions sont finalement tirées de ces cas vis-à-vis de projets de modifications intentionnelles du climat global aujourd'hui.

-H-

Plant Cognition and the Re-enchantment of nature

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Over the last decade, a small group of ecologists and plant physiologists have begun to argue that plants exhibit consciousness. In The Revolutionary Genius of Plants, Stephan Mancuso tells readers that "plants exhibit unmistakable attributes of intelligence," including an ability to perceive and react to changes in their environments (Mancuso 2017, xi). In Thus Spoke the Plant, Monica Gagliano argues that "plants can communicate, are capable of learning, have memory, [and] make decisions" (Gagliano 2018, x). These claims, however, have been met with fierce criticism from other plant scientists and neuroscientists who argue that attributing awareness and cognition to plants is inappropriate. Without neurons and the complex brain structures of animals, these critics argue, it is impossible for plants to be conscious (Taiz et al., 2019).

In this paper, we seek to place this controversy and the emerging field of plant cognition into its larger historical context. Some of the scientists defending plant consciousness trace their experiments back to Lamarck, positioning their work as continuation of Enlightenment science. Gagliano, on the other hand, seems to argue for a more thorough re-evaluation of key goals and methods of modern science, calling for a rejection of materialism and hierarchy, as well as illusions of control and objectivity. She sees her research as "a collaborative effort with [her] plant associates" (Gagliano 2018, 33). At the same time, she appeals to the history and philosophy of science – for example by drawing on a particular reading of Galileo's story, as well as on Kuhn's notion of paradigm shifts – to articulate an epistemology based on empiricism, personal experience, and personal testimony. As we ask whether plant cognition constitutes a coherent field of study, we evaluate this early 21st century debate against major epistemological shifts in the past including Romanticism in the early 19th century and ecofeminism in the mid 20th C. We investigate how the history and philosophy of science enters into this debate, as well as the extent to which many proponents of plant cognition are, consciously or not, echoing past traditions and ideas in their attempts to "re-enchant" nature.

Marx and Engels: Against Positivism, for Dialectical Materialism

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The work of Friedrich Engels and Karl Marx in the history of the philosophy of science has largely been neglected in the West, particularly since the fall of the Soviet Union, despite their outsized influence in the social sciences and humanities. Far from being disengaged with questions concerning the natural scientists of their day, both thinkers were deeply interested in natural scientific developments and their relationship to social/political activity. Though it is often exclusively the work of Engels that is discussed in relation to "Marxist" approaches to the philosophy of science (often as a pejorative from those with more humanist tendencies), recently published material from the notebooks of Karl Marx on the natural sciences, with particular emphasis on ecology, have shed light on his own thoughts and contributions to theory (Saito, 2016).

In this paper, I will explore the relationship between the thought of Marx and Engels on the natural sciences in light of this emerging work. I will argue specifically against the theory that there is a fundamental disconnect between the views of both thinkers on the dialectical character of nature, which is best summarized by Terence Ball's assertion that "The idea (later espoused by Engels) that nature exists independently of, and prior to, men's efforts to transform it, is utterly foreign to Marx's radical humanism" (Ball, 1979). I will show that this view rests on the faulty assumption on behalf of its adherents that Engels' dialectical materialist conception of nature commits him to positivism, which would be foreign to Marx's qualified humanism. Both thinkers approach nature dialectically, which is opposed both to positivism as well as to subjective idealisms and humanisms.

Publication Bias is Bad for Science if not Necessarily Scientists

Remco Heesen University of Western Australia **and Liam Bright** London School of Economics and Political Science

It might seem obvious that the scientific process should not be biased. We strive for reliable inference, and systematically skewing the results of inquiry apparently conflicts with this. Publication bias – which involves only publishing certain types of results – seems particularly troubling and has been blamed for the replication crisis. While we ultimately agree, there are considerable nuances to take into account. A scientist who is aware of publication bias can interpret the published literature so as to avoid acquiring biased beliefs. Moreover, in some specific circumstances she actively prefers the presence of publication bias as it helps her read only the most relevant results. We prove this in a model that closely approximates statistical practice. However, we also

argue that science as a social endeavor is made worse off by publication bias. This is because the social benefits of science are largely secured via go-between agents that are unlikely to be well-informed enough to account for publication bias appropriately.

Explicating Exact versus Conceptual Replication
Robert Hudson University of Saskatchewan

It has recently become a pressing concern that many verified scientific, experimental results fail to replicate. But what does it mean to replicate an experiment? A distinction is often drawn between 'exact' (or 'direct') and 'conceptual' replication. On that basis, it is suggested by some methodologists that scientists should strive for exact replications, whereas others vouch for the priority of conceptual replications. But Edouard Machery (2020) and Uljana Feest (2019) have recently argued that the distinction between exact and conceptual replication is misconceived, and should be dispensed with. Feest argues that the problem of systematic error shows that neither exact nor conceptual replication have much use in scientific, experimental investigation. For his part, Machery argues that the notion of a conceptual replication is confused, and that the only notion of replication left to be considered is captured by his 'Resampling Account', itself a form of exact (or direct) replication. My goal in this paper is to defend the distinction between exact and conceptual replication from the critiques of Feest and Machery. To this end, I provide an improved analysis of what it means to be a conceptual replication according to which one distinguishes between 'experimental' as opposed to 'conceptual' replication, with exact replications serving as special cases of the former. With this improved analysis, it becomes clear that conceptual replications and exact replications are different kinds of things that play distinct roles in the scientific process, contrary to the arguments of Feest and Machery.

-I-

The Problem of Trustworthy AI in Medicine
Paul Istasy Schulich School of Medicine and Dentistry **and Ben Chin-Yee** London Health Sciences Centre

This paper argues that under a patient-centered model of care, the notion of trustworthy artificially intelligent systems is inherently misconstrued. Trust is an essential component of the physician-patient relationship. Current models of the clinical encounter focus on shared decision-making, whereby physicians develop management plans with patients and actively elicit and integrate patients' values and preferences into clinical decisions. With the increasing use of artificial intelligence (AI) in medicine, there is a growing concern about how this will influence the physician-patient relationship. While the majority of the relevant literature examines the dynamics of human-machine relationships, the influence that these machines have on human-human relationships remains an understudied topic. The trust in a physician-patient relationships is determined by the competence and integrity of a physician— the former is self-evident, and the latter is required for identification-based trust, which is a type of trust whereby a physician identifies with and incorporate a patient's values in medical decision-making. We provide an analysis of the physician-patient trust relationship in light of the introduction of medical AI. We demonstrate that even if artificially intelligent systems show superior competence to physicians in certain tasks, they will ultimately fail to display integrity and thereby, eliminate any possibility of identification-based trust between the patient and the physician. To this end, medical AI systems cannot be considered the epistemic authority in a medical setting and if introduced without philosophical scrutiny it may adversely affect the physician-patient trust relationship.

-J-

Formal Analogies, Scientific Explanation and Understanding
Aditya Jha University of Canterbury

A Formal analogy could be drawn between two disparate target systems 'T' if their Models exhibit a shared mathematical structure 'M'. Formal analogies have been a heuristic aid in making some major discoveries in Physics (Steiner 1998). There are three prominent accounts of how one could make sense of such analogical reasoning: (a) Structure mapping approaches (Gentner 1983) that seemingly exploit morphisms between T and M; (b) Causal-condition approach (Hesse 1966) that exploits a scientifically reasoned causal representation of T in M; and c) the Articulation model (Bartha 2010) which relies on a correspondence between well-connected known similarities in T projected to hold in M. I argue that all these three approaches are flawed, in at least some respects, since they (i) presuppose that M can be reasonably decomposed into useful and surplus structures and subsequently mapped/embedded in T, or (ii) assume that it is often possible to segregate the difference-making factors from the non-difference making factors in M since it is an accepted scientific representation of T. I employ two broad strategies for this defence: one comes

from Rice (2019) who argues that a general decomposition or segregation strategy for M does not acknowledge the way mathematical representations routinely distort their target systems so as to make recovery of difference-making factors from M elusive; and two, that mathematical representations are rational reconstruction of target systems which often 'conceal' the difference-making features of T in the abstraction, if not misrepresent them. If T remains 'concealed', despite the heuristic representation M, then the philosophical problem of understanding the success of Formal analogies in Physics still remains.

-K-

Funding the future? Trust and Transparency on Crowdfunding Platforms for Research Projects
Rachel Katz University of Western Ontario

Crowdfunding is an online fundraising tool that allows individuals or groups to collect donations from individuals both in and beyond their immediate networks, usually facilitated by social media. While many crowdfunding platforms support a panoply of projects, there are others that cater specifically to researchers. In this paper, I evaluate two pressing ethical issues with the current conduct of these platforms: trust and accountability and transparency. First, I provide historical context, where I discuss Francis Bacon's Nova Atlantis and the eventual formation of the Royal Society of London. I argue that the formation of the Royal Society paved the way for the open science movement of the 20th century, which in turn set a precedent for crowdfunded research. Following this context, I turn to my ethical arguments. I argue that these platforms lack the built-in accountability mechanisms of more traditional funding avenues such as grant-based funding. As a result, crowdfunding requires the building of trust between researchers, donors, and the platforms themselves. I conclude by arguing that better transparency may provide a way for crowdfunding to become a more trustworthy style of funding. This project marks one of the first efforts to critically evaluate the conduct of crowdfunding platforms that specialize in funding for research, and I present and defend the view that while potentially beneficial for individuals and institutions, crowdfunding for research cannot continue to exist as a "Wild West".

Contra the Value-Free Ideal of Science
Aaron Kenna University of Toronto

The argument from inductive risk [A-IR] aims to demonstrate that scientists qua scientists must appeal to non-epistemic value judgements when appraising evidence and validating scientific hypotheses on account that moral, aesthetic, and social values are necessary aids to scientists in determining when a body of evidence is sufficient for accepting (rejecting) hypotheses. If sound, [A-IR] impugns the value-free ideal of science, which states that scientists ought not to allow non-epistemic judgements to inform scientific appraisals. In two recent papers Gregor Betz defends the value-free ideal of science from [A-IR], arguing that scientists can avoid having to make non-epistemic value-laden decisions about what is and is not sufficient evidence if they qualify scientific appraisals only with those comprehensive assessments of relevant uncertainties which they deem certain beyond a reasonable doubt. The upshot of his programme, claims Betz, is that scientists can maintain the proper division of labour between scientific advisers and policy-makers that a democratic society requires: scientists offer up value-neutral characterisations of uncertainties whilst policy-makers make decisions about the sufficiency of the scientific evidence for a specific decision problem.

Whilst Betz's programme holds a rather attractive normative appeal, I argue that his defence of the value-free ideal ultimately fails. Betz's argument falters, I contend, for comparable reasons Jeffrey's oddsmaker view of science defence of the value-free ideal falters, namely, scientific assessments of uncertainties, just like the probabilities assigned to scientific hypotheses in light of the relevant evidence, are themselves value-laden. More precisely, I argue contrary to most theories of choice under uncertainty that subjective probabilities and non-epistemic values (utilities) are inseparable. And if subjective probabilities are inseparable from non-epistemic values (utilities), then so too are the scientific assessments that are "certain beyond a reasonable doubt", the sort of assessments which Betz's defence of the value-free ideal requires.

Familiar Portraits: The Hidden Convergence of Explanatory Practices in Science and Metaphysics
Thomas Kivatinos Auburn University

I argue that some of the explanatory practices of science and metaphysics are not distinct, despite apparent differences. I identify the explanatory role that is both essential to and common between some metaphysical explanations, on one hand, and some scientific explanations called "mechanistic" explanations, on the other. There are instances of both modes of explanation that elucidate some phenomenon P by treating P as if it occupies a hierarchy of levels that is structured by dependence relations, and by identifying a lower-level phenomenon within the hierarchy that P depends upon. By doing so, the relevant explanations elucidate P by portraying it as a "higher-level," less fundamental phenomenon that arises out of a "lower-level," more fundamental phenomenon, and identifying the specific lower-level phenomenon that P arises out of and is fundamental to P. Thus, some mechanistic and metaphysical explanations operate via the same explanatory role. Moreover, some explanations of both kinds match in terms of the phenomenon P that they serve to explain, and in terms of what they explain P by appeal to (i.e., both their

explanans and explanandum are identical). In light of this, I argue that some scientific explanations turn out to be identical to some metaphysical explanations. For some of the mechanistic explanations employed by science play the same theoretical role and capture the same facts about the world as some explanations employed by metaphysics. Therefore, in a respect that's gone largely unrecognized, science and metaphysics converge in their methods of providing epistemic access to how the world works and why the world is the way it is.

Regularizing (away) vacuum energy

Adam Koberinski University of Waterloo

Quantum field theories famously 'predict' a divergent value for the amount of energy contained in the vacuum state. Arguments from general relativity indicate that quantum vacuum energy should gravitate, and contribute to the Einstein field equations like a cosmological constant term. The huge discrepancy between the large values predicted by quantum field theory and the measured value of the cosmological constant is termed the cosmological constant problem. In this talk, I will discuss various ways that one can tame the vacuum divergences in quantum field theory to come up with a 'prediction' for the cosmological constant. These include orthodox regularization schemes like lattice regularization and dimensional regularization, as well as more careful treatments of multiplying distributions. These various methods give very different results, and all are incompatible with a true prediction for the cosmological constant. I conclude that one should not trust quantum field theory to correctly determine the amount of energy contained in the vacuum.

-L-

Du Châtelet on Mechanical Explanation versus Physical Explanation

Qiu Lin Duke University

In her second edition of the Foundations of Physics, Du Châtelet advocates a three-fold distinction of explanation: the metaphysical, the mechanical, and the physical. While her use of metaphysical explanation (i.e., explaining via the Principle of Sufficient Reason) has received some attention in the literature, little has been written about the distinction she draws between mechanical and physical explanations, including their demand, scope, and use in physical theorizing. This paper aims to fill this void, arguing that making this distinction is a crucial piece of Du Châtelet's scientific method. According to Du Châtelet, a mechanical explanation is one that 'explains a phenomenon by the shape, size, situation, and so on, of parts', whereas a physical explanation is one that 'uses physical qualities to explain (such as elasticity) … without searching whether the mechanical cause of these qualities is known or not' (Du Châtelet 1742, 181). My analysis will focus on Du Châtelet's views regarding (1) What counts as a good physical explanation, (2) Why a mechanical explanation is not necessary for answering most research questions in physics, and (3) Why a good physical explanation, instead, is sufficient for answering those questions. In so doing, I argue that Du Châtelet is proposing an independent criterion of what counts as a good explanation in physics: on the one hand, it frees physicists from the methodological constraint imposed by mechanical philosophy, which was still an influential school of thought at her time; on the other, it replaces this constraint with the requirements of attention to empirical evidence, for that alone determines which physical qualities are apt to serve as good explanans.

Climate Denialism is Harmful Bullshit

Joshua Luczak University of Western Ontario

The harms of anthropogenic climate change are many, great, and fairly well known. The harms resulting from climate denialism, however, are greater in number and significance than are acknowledged. Standardly, climate denialism is taken to be harmful because it has repeatedly hindered governments of the world from taking immediate and drastic (or even meaningful) action on mitigating to the harmful effects of anthropogenic climate change. While this is undeniably a very great harm, it is not the only harm of climate denialism. These harms are a consequence of climate denialism being bullshit. This paper is about these harms.

This paper intends to show that a number of significant claims made by climate denialists are bullshit. It also intends to show that this bullshit is harmful. In fact, climate denialism is harmful in several ways. It is often harmful because it undermines the epistemic demands imposed on us by what we care about. It is harmful because it undermines the epistemic demands imposed on us by the social roles we occupy. It is harmful because it undermines the epistemic demands imposed on us by morality. And it is harmful because it corrodes epistemic trust.

This paper also discusses what all of this means for our individual moral duty to mobilise governments, through activism and voting, to act on mitigating the effects of anthropogenic climate change. Importantly, the normative force of this duty will be questioned in light of the damage being done by climate denialism bullshit.

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Reconstructing the history of philosophy of science by means of topic-modeling
Christophe Malaterre Université du Québec à Montréal

Much of history of philosophy is done via careful scholarly examination of individual texts. Such an approach has the merit of generating a deep understanding of the theses defended in these texts, but it also runs into difficulty when faced with large volumes of textual data. This situation is true in the more specialized field of the history of philosophy of science as well. In this contribution, we show how data-driven text-mining tools can be used as a complement to usual historical analyses. We propose to approach the history of the philosophy of science throughout the 20th century with the assistance of statistical algorithms applied to the complete full-text corpus of eight major journals of the field from 1931 up until 2017. By running text-mining and topic-modeling algorithms, we identified key research topics that span across these 87 years. We also investigated the evolution of these topics over time and their fluctuating significance in journal articles. Our results concur with known episodes of the discipline—for instance, the rise and fall of logic and language-related topics or the more recent rise of the philosophy of biology—but also highlight a diversity of topics that is much richer than what is usually acknowledged.

The Analyticity Objection to Special Science Laws
Alexandru Manafu York University

A number of authors, including Kim (1992), Millikan (1999), Boyd (1999), and Shapiro (2000, 2005) have expressed similar concerns about the possibility of a science whose kinds are functional. They have suggested that if there were laws in which functional kinds figured, these laws would be analytic, and thus uninformative. If one holds the requirement that natural sciences are by their very nature empirical investigations of the world, the worry that there can't be a science of multiply realized functional kinds becomes apparent. In this paper I offer a precise formulation and a label for this problem, by expressing the concerns that these authors have univocally, as similar instances of what I call "the analyticity objection to special science laws". Using a number of examples of multiply realized kinds appearing in various scientific domains such as chemistry and solid state physics, I argue that the analyticity objection doesn't hold for these domains, hence the credibility of the general point of the objection is much diminished.

The Nomic Likelihood Account of Laws
Christopher Meacham University of Massachusetts, Amherst

An adequate account of laws should satisfy at least five desiderata: (1) it should provide a unified account of laws and chances, (2) it should yield plausible relations between laws and chances, (3) it should vindicate numerical chance assignments, (4) it should accommodate dynamical and non-dynamical chances, and (5) it should accommodate a plausible range of nomic possibilities. No extant account of laws satisfies these desiderata. This paper presents an account of laws, the Nomic Likelihood Account, that does.

The paper begins by motivating the need for such an account. Then it spells out the account's fundamental posit, a six-place nomic likelihood relation. It then provides a novel representation and uniqueness theorem showing that this posit will yield things that look like laws and chances. It then uses these results to provide an analysis of laws and chances. Finally, the paper presents a number of consequences of this account, and addresses some potential objections.

The 'construction' of GE mosquitoes with gene drives as the solution to the high incidence of mosquito-borne diseases: An epistemically and ethically justified scientific practice?
Zahra Meghani University of Rhode Island

Laboratories and factories are not the only places where technologies are constructed; technologies are also 'created' (as solutions to particular problems) in the pages of scientific journals. This presentation will analyze a case of the latter kind of technology construction enterprises. It will examine scientific papers that 'construct' genetically engineered (GE) mosquitoes with gene drives as the solution to the high incidence of mosquito-borne diseases like malaria in certain socio-economically marginalized regions of low-income countries.

A defining characteristic of the scientific papers that will be analyzed is that they 'biologize' (or 'naturalize') the public health problem of the substantial prevalence of mosquito-borne diseases in certain poorer regions of low-income countries. Such papers do not appropriately acknowledge the structural, systemic factors that are partially responsible for the public health problems. Safe living and work environments (including a safe supply of water, waste treatment processes, screened buildings), access to adequate, nutritious food, and preventative and therapeutic health care are important determinants of the high incidence of mosquito-borne and other vector-borne diseases. The vulnerability of persons to an infectious disease of poverty, their experience of the illness, and their capacity to recover from it is not a purely biological matter.

This presentation will analyze the epistemic, and ethico-political significance of such technology constructions endeavours in the pages of scientific journals. The larger aim is to contribute to the discussion about epistemically, ethically, and politically responsible practice of technology construction by scientists.

Controversies Over the Keystone Species Concept: A Brigandt-Longino's Analysis

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In ecology, the term "keystone species" has been coined by Robert T. Paine (1933-2016) at the beginning of the 1969 year. In a "note" published in the American Naturalist, Paine uses this term to qualify two groups of top-predator species, the sea star Pisaster ochraceus and the tritons of the genus Charonia, which structural role within their historic ecological community was somehow analogous to a "keystone" in a stone arch (Paine, 1969, 1995). Over the following decades, the keystone species concept has been defined and applied in various ways and used for different purposes in ecology, in conservation biology, and more recently in ethnoecology. In this paper, I focus on the controversy raised by Ann Garibaldi's and Nancy Turner's "cultural keystone species" concept (Garibaldi & Turner, 2004). This allows me to introduce an epistemic framework, drawn from Ingo Brigandt's view on biological concepts (Brigandt, 2010, 2012) and Helen Longino's underdetermination argument (Longino, 1990), that: (1) acknowledges the necessary influence of social and ethical values in the changes undergone by ecological concepts ; (2) extends Ingo Brigandt's approach to interdisciplinary cases where only non-epistemic purposes are at stake; (3) calls for enlarging the scientific debate over the keystone species concept to ethical and epistemic issues that have to do with the aims and values attached to the various instances of the keystone species term in ecology and related fields, instead of asking whether or not there might be one and only one "good", "useful", "adequate" ecological keystone species concept.

Quantum Determinables

Michael Miller University of Toronto

Hawthorne has advocated that physical properties such as mass and charge be treated as determinables, with particular values of the properties being determinates of the determinable. Wilson has used determinables and determinates to provide an account of metaphysical indeterminacy in general, and Calosi and Wilson have used this account of metaphysical indeterminacy to provide an account of the indeterminacy that arises for quantum mechanical properties in particular. According to their view, a state of affairs is metaphysically indeterminate just in case a quantum mechanical system has a determinable property such as spin along the x direction, but no determinate of that determinable. The views of Hawthorne, Wilson, and Calosi have recently come under criticism of three distinct varieties. The first holds that quantum mechanics does not involve indeterminacy at all, the second that determinables and determinates do not appropriately capture the nature of quantum mechanical indeterminacy, and the third, that determinables and determinates are ill-suited to treat the nature of physical properties more generally. In this paper I respond to each of these lines of

criticism. I proceed by generalizing the account of Calosi and Wilson so that it affords a complete logic of quantum metaphysical indeterminacy. This generalization shows that there is a clear sense in which quantum mechanical property ascriptions exhibit metaphysical indeterminacy, and it does so in a way that is perspicuously captured using determinables and determinates. This success at treating quantum mechanical property ascriptions provides evidence that Hawthorne's suggestion that physical properties be treated in terms of determinables and determinates in general is in fact well-founded, thus responding to the third line of criticism.

Descartes, Model-based Scientist

Ryan Michael Miller University of Geneva

Descartes' break with Baroque scholasticism is often framed as a move towards certitude in simple and mathematical methods instrumental to the founding of modern science. While Descartes' development of analytic geometry and its application for significant breakthroughs in optics bears this out, however, most of his scientific explanations are now viewed as dated rather than certain, and many of his claims contradict the more accurate understanding of Galileo and others. I suggest that Descartes' breakthrough is not in mathematical or error-free method but rather in a broadening of the notion of assumption to include multiple independent and contradictory models. Perhaps counter-intuitively, Descartes finds this broad notion of assumption in Ptolemaic astronomy, and first applies it in algebraic kinematics. Descartes' use of models reaches its zenith, however, in his optics, where he disclaims any attachment to the truth of his contradictory physical models for the transmission, reflection, and refraction of light while insisting that each is necessary for explaining the observed phenomena. These multiple contradictory models used by Descartes go well beyond the idealization of Galileo, since the latter insisted that he only neglected minor phenomena rather than engaging in contradiction. This broad Cartesian view of modeling is now considered critical to predictive science in many disciplines and gives Descartes a genuine place in the foundation of modern science independent of his physical views.

An Armstrongian Defense of Dispositional Monist Accounts of Laws

Mousa Mohammadian University of Notre Dame

There is an important debate about the relationship between laws of nature and their corresponding regularities within the non-Humean camp of theories of laws of nature. In his "The Ultimate Argument against Armstrong's Contingent Necessitation View of Laws" (2005), Bird reveals an important problem at the heart of Armstrong's theory of laws of nature: to explain how a law necessitates its corresponding regularity, Armstrong is committed to a vicious regress. In his very brief "Reply to Bird" (2005), Armstrong gestures towards a response that, as he admits, is more of a "speculation" than an argument. Later, in their "The Ultimate Argument against Dispositional Monist Accounts of Laws" (2012), Barker and Smart argue that a very similar problem threatens Bird's dispositional monist theory of laws of nature and he is committed to a similar vicious regress. In this paper, first I construct Armstrong's would-be argument in response to Bird. I show that in this very brief reply, Armstrong's strategy is to carry out an interesting maneuver to respond to Bird's criticism in terms of the instantiation of universals, especially the nomic relations or laws of nature. Second, I argue that his response causes more problems than it solves for his accounts of laws of nature and natural properties, especially because it makes them inconsistent with "a posteriori Realism" about universals according to which what natural properties and laws are in the world is to be decided a posteriori, on the basis of total science. Finally, I argue that Armstrong's strategy to address Bird's criticism can be used, quite ironically, to defuse Barker and Smart's argument against Bird.

Values in Science: Pragmatist and Feminist Perspectives
Parysa Mostajir University of Chicago

Philosophers Heather Douglas, John McCumber, and Don Howard have observed that, from the 1960s, philosophers of science largely ignored socio-political factors, opting for a confirmation-theoretic approach that accepted the value-free ideal of science. In the 1990s, to undo this depoliticization and challenge the notion of science as a process that reveals pure, objective facts, feminist scholars such as Longino and Rooney demonstrated the implausibility and untenability of a clear-cut divide between the operation of epistemic/constitutive, truth-oriented values and the operation of contextual, socio-political values.

Taking a broad look at 20th century philosophy of science, the growth of the value-free ideal coincided roughly with the recession of the pragmatist tradition in American philosophy. Bridging this gap, the insights of late-20th century value-conscious feminist philosophers of science can be read as consistent with and strongly implied by theories of science found in early-20th century value-conscious pragmatist philosophy; and potentially, there is a highly fruitful theoretical conversation to be had between them.

In Dewey's Logic, he suggests that the methods of scientific inquiry we institutionalize are not absolute methods by which we reveal the objective nature of reality, but are historically developed and adapted to meet specific biological and socio-cultural needs. Dewey was reacting to logical positivists, but if we interpret his statements on institutionalized methods to correspond to Longino's "constitutive values," then pragmatism contains a finely-developed framework for engaging feminist theories on the social and cultural purposes for which science is undertaken, and social and cultural influences on our concepts of objectivity and rationality. Additionally, pragmatism's consideration of science's adaptation to relatively universal biological needs (as well as highly contingent social ones) may resolve some questions left by feminist epistemology about the existence of a degree of objectivity—albeit limited—in scientific methods as they stand.

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Cui Bono? The Continental Critique of Teleonomy
Auguste Nahas University of Toronto

Recent years have seen a resurgence of interest in the idea of biological purposiveness, agency, or teleology, along with a flurry of debate regarding the exact meaning of these terms. The aim of this paper is to bring some clarity to these debates by retracing them to the conceptual bifurcation which 'teleology' underwent during the middle of the 20th century. The origin of this split might be traced back to Rosenblueth et al.'s landmark 1943 paper, Behavior, Purpose, and Teleology, which proposed a new, scientifically legitimate, 'non-Aristotelian' conception of teleology. This new conception was popularised by Colin Pittendrigh, who first named it 'teleonomy', and Ernst Mayr who connected it to the idea of the genetic 'program'. This attempt to naturalise teleology was quickly critiqued by continental philosophers such as Georges Canguilhem, Raymond Ruyer and especially Hans Jonas, who unanimously saw teleonomy as a way of eliminating teleology rather than naturalising it.

Beyond the need for a historical reconstruction of this episode which emphasises the importance of teleology as an topic of inter-disciplinary and inter-continental debate, it is my contention that a better understanding of this period can help us make headway in contemporary debates on the role of purposiveness as a biological concept. For what the continental critique of teleolonomy reveals is a disagreement over the kind of problem which naturalisation of teleology is, and the way one ought to go about it. These very same disagreements, which often remain implicit, continue to influence contemporary debates.

From Static to Dynamic: A Historical Account of the Emergence of the Dynamic View of Proteins

Jacob P. Neal University of Pittsburgh

There has been a recent shift in molecular worldview from static to dynamic. Changes in scientific representations of proteins in the late twentieth and early twenty-first centuries exemplify this shift. Whereas the static view of classical molecular biology held that proteins were rigid, compact, and largely static molecules, the newer dynamic view represents proteins as undergoing constant structural fluctuations. In this paper, I develop an account of the history of protein science that focuses on the emergence and rise to prominence of the dynamic view of proteins. My account aims to answer two outstanding historical puzzles: (1) why did the dynamic view of proteins emerge as a challenge to the dominant structural view? and (2) what explains the relatively slow uptake of the dynamic view? I argue against anomaly- and technology-driven accounts of this shift. Instead, I show that pre-existing theoretical commitments led a handful of scientists to develop and defend the dynamic view of proteins. These scientists were committed to treating proteins as small thermodynamic systems, which showed that protein dynamics were an inevitable consequence of the laws of thermodynamics. This theoretical analysis convinced them to accept the dynamic view and led them to seek out anomalous cases of protein behavior that could only be explained using the dynamic view of proteins. Although the discovery of anomalies played a part in ultimately convincing the scientific community, I conclude that theoretical considerations about protein dynamics were the primary driver behind the emergence of the dynamic view.

The Grammar of Bird Cognition: Wittgenstein, Corvids, and Nonlinguistic Concepts
Erik Nelson Dalhousie University

Comparative psychologists are usually willing to attribute conceptual capabilities to nonlinguistic animals if they are able to abstract a relationship and then apply it to novel stimuli (e.g. Castro and Wasserman 2017). In contrast, many philosophers have argued that scientists who attribute conceptual capabilities to nonlinguistic animals have failed to understand what it actually takes to grasp a concept. Holists, such as Dennett (e.g. 1996), Brandom (e.g. 2009), and Rosenberg (e.g. 1997), argue that grasping any one concept depends upon the ability to grasp the inferential connections between it and other concepts. These inferential connections are only available through linguistic practices. It is the alleged failure to recognize this requirement that has led Brandom to claim that "analytic philosophy has failed cognitive science" (2009, 197). In this paper, I will assess the claim that grasping a concept is only possible through the medium of language by comparing the claims by holists about the capabilities (or lack thereof) of nonlinguistic animals to recent empirical work on the ability of Clark's nutcrackers and black-billed magpies to solve same/different transfer tasks (Magnotti et al. 2015, Magnotti et al. 2017, Wright et al. 2017). I argue that success at same/different transfer tasks is only possible for subjects that can grasp (what the later-Wittgenstein called) the internal relations between the concepts of same and different. Two concepts are internally related if it is only possible to understand one by also understanding the other. The success of Clark's nutcrackers and black-billed magpies at these tasks demonstrates that grasping the inferential connections between (at least some) concepts is not dependent upon linguistic capabilities.

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Climate model comparisons: history and philosophical implications
Ryan O'Loughlin Indiana University Bloomington

Since 1989 climate scientists have pursued coordinated model intercomparison projects involving multiple complex general circulation models (GCMs) simulating future climate under projected CO_2 increases. Today such intercomparisons involve dozens of GCMs investigating an abundance of topics ranging from aerosol chemistry, to regional climate changes via downscaled models, to sea ice changes, among others. Importantly, these are predated by less formal model comparisons in the 1970/80s which involved: early investigations of climate sensitivity using a GCM and simpler models; the first climate model intercomparison conference; a White House commissioned study on possible future climate change; and an in-depth analysis of two GCMs running long range experiments. There is therefore a continuity of scientific practice involving model comparisons as an epistemic and pragmatic strategy that involves analyzing both model agreement and discordance to learn about earth's climate system, to provide data for climate projections, and to inform model development. Thus, I contend that model comparisons are a fruitful philosophical unit of analysis, important for several reasons. First, understanding model comparisons is necessary for understanding current uncertainty about the climate system and how scientists address it (e.g., model intercomparisons explore but do not sample uncertainty). Second, current scholarship in the philosophy of climate modeling may be expanded (Parker 2006; Lloyd 2015) or productively critiqued (Lenhard and Winsberg 2010; Katzav 2014) by thinking about modeling in these terms. Finally, model comparisons feature in other sciences (e.g., economics, epidemiology) and so this work can pave the way for future philosophical inquiries across the sciences.

Novel Methodology in Reconstructions of Quantum Theory
Jessica Oddan University of Waterloo

Traditional approaches to quantum theory are generally focussed on how the complex mathematical formalism of the theory should best be interpreted, particularly what we should take quantum theory to tell us about the world. Reconstructions of quantum theory, such as those argued by Hardy and Coecke, are a new branch of theoretical physics which challenge this traditional approach. These reconstructions aim to reformulate quantum theory from a base set of physical principles in order to derive the key postulates of quantum theory (the von Neumann postulates), thereby illuminating the correct interpretation of the formalism. I will argue that reconstructions of quantum theory reveal novel methodology not previously employed within quantum theory or physics. This new methodology proves particularly enlightening because it has the ability to contend with physics in the context of discovery via conceptually driven axiomatization that aims to determine the salient mathematical and/or physical features of quantum theory. I will argue that not only do reconstructions employ axiomatization, but they do so in an innovative way, insofar as they indicate a conscious use of different mathematical frameworks in order to facilitate concept formation. I draw insight from Bokulich regarding the utilization of different mathematical formalisms in quantum theory, comparing the reconstruction program to her examples. The result of these reconstructions is a pointed move toward pluralist approaches in theory development in quantum theory.

On the Meaning of the Wave Function: Ettore Majorana's Lectures Notes on Theoretical Physics
Andrea Oldofredi Université de Lausanne

Ettore Majorana was one of the most prominent Italian scientific figures during the thirties of the XX century, and his contributions to physics are studied and discussed by several commentators to this day. However, very little has been said about his interpretation of the wave function ψ, the central object of Quantum Mechanics. Referring to this, some authors claimed that Majorana anticipated the physical content of Feynman's path integral approach, grounding their thesis on a draft of a conference held by him in January 1938.

However, considering Majorana's lectures notes on theoretical physics - a text not yet subjected to philosophical analysis - it is possible to propose a novel interpretation of his thoughts regarding the meaning of the quantum state.

Indeed, I will argue that Majorana endorsed an epistemic view of ψ, interpreting it as a mathematical tool encoding the knowledge about the state of a system as one can deduce it from previous experimental experiences, a conception very far from Feynman's. On the physical side, he held a semi-classical view about QM, where physical objects instantiate definite properties, although their measurability is constrained by the rules of quantum theory. The main novelty of the new mechanics, according to him, is to be found in the inherent stochastic nature of the quantum laws.

Finally, I will show that Majorana's interpretation of ψ finds its roots in Heisenberg and Dirac's works on QM, concluding that this alleged anticipation of Feynman's work contrasts with the views contained in his lectures on quantum theory.

Irreducibility, Novelty, or What? Towards a Property Cluster Theory of Emergence
Erica Onnis University of Turin

In the last decades, discussions about emergence have become pervasive in several areas of philosophy and almost every branch of science. This fact exacerbated the difficulties in the formulation of an appropriate definition of emergence – a problem that has plagued the debate since its origins. During the years, in fact, many criteria have been identified, and several models and schemas have been formulated, but no account of emergence seems now able to appropriately accommodate all the phenomena that the philosophical and scientific literature usually recognizes as emergent (e.g., quantum decay, spacetime, molecular geometry, complex patterns of behaviours, consciousness, etc.).

Models of emergence are typically developed focusing on just a few well-selected representative cases, but this approach results in formulations that lack generality. This attitude depends upon the idea that emergence can be identified by a number of stable criteria usually including ontological and epistemological irreducibility, and causal novelty. The examination of a fair number of examples, however, highlights that many alleged emergent phenomena do not meet these requirements.

While several authors downgrade these cases as non-genuine cases of emergence, my suggestion is to widen the models we have and replace the quest for necessary and sufficient conditions with the identification of an open cluster of properties including a variety of different features characterizing emergence in a contextual and non-exclusive way. This approach suggests that emergence is a highly heterogeneous phenomenon that exhibits different properties depending on the ontological domain in which it appears.

A Scientific Centennial: The 1921 Toronto Meeting of the American Association for the Advancement of Science
David Orenstein Danforth CTI

In December 1921, the American Association for the Advancement of Science (AAAS) held its Meeting at the University of Toronto (U of T). This was the first international scientific congress to be held in Canada after World War I, the last one previously being held in 1913, the International Geological Congress (IGC), also at U of T. It also allowed the University to be well-prepared

to host both the International Mathematical Congress (IMC) and the British Association for the Advancement of Science (BAAS) in August 1924.

It was a crucial year both in Canada and the United States. Warren Gamaliel Harding had been inaugurated President of the United States on March 4, bringing in an era of conservative laissez faire "normalcy", while William Lyon Mackenzie King and his Liberal Party had just won the mould-breaking Canadian federal election on December 6, naming the cabinet in mid-Meeting on December 29. Insulin had been discovered at U of T by Banting, Best, Mcleod and Codlipp that summer, while the Communist Party of Canada had been founded on May 29. At this very Meeting there was established the Committee for the History of Science, the precursor of the History of Science Society (HSS).

Using primary sources, this talk shows how the 1921 AAAS Toronto Meeting illuminated this changing era at scales both big and small.

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Degrees of Harm and Legitimacy: A Defense of the Inductive Risk Account of Epistemically Detrimental Dissent

Tyler Paetkau University of Alberta

This paper aims to defend and build on the inductive risk account (IndRA) of identifying epistemically detrimental dissent (EDD) proposed by Justin B. Biddle and Anna Leuschner. While dissent is essential to the scientific process, Biddle and Leuschner argue that it can also have negative epistemic consequences. Since instances of detrimental dissent often share specific characteristics, Biddle and Leuschner propose the IndRA, composed of four jointly sufficient criteria for identifying EDD. In contrast, Inmaculada de Melo-Martín and Kristen Intemann argue that the account is of little use and fails to identify EDD reliably. Although I propose that many of these criticisms can be mitigated, I suggest that they reveal structural weaknesses of the IndRA. As such, I argue that the IndRA must be modified. By eliminating the need for the four criteria to be jointly sufficient and requiring that each criterion be measured as a matter of degree rather than category, I argue that the IndRA is a powerful tool for identifying EDD. Furthermore, I suggest that even when any or all of the criteria fail to be fully met, the account serves as a reliable indicator of EDD.

Morphine, alcohol, and the victorious body: how intoxicants intersected bodies and minds in the development of the biological subject

Matthew Perkins-Mcvey Dalhousie University

Drunk, inebriated, besotted, boryeyed, muddled, and exhilarated—intoxication of many kinds, and even more names, is at the heart of an untold story about shifting perceptions of "embodiment" found toward the end of the 19th century. Substances of intoxication can be found quietly skulking in nearly every nook and cranny of our society. However, little thought has been given to their formative role in the shaping of the modern body. This paper explores the formative role of substances of intoxication in the social and scientific establishment of the biological subject in late-19th century Germany. Beginning with the reformation of the European pharmacy and the eighteenth century project of medical system building, this narrative identifies the emergence of substances of intoxication as 'vital substances' and tracks the influence of this concept on the development of scientific physiology and philosophy. Particular emphasis is placed on late 19th century psychological research on the effects of intoxicants on the mind as the site of a dynamic encounter between bodies, theories of mechanism, and 'vital substances'. Here, one finds the emergence of a biological subject which remains fundamentally anti-vitalistic and yet is conceptually distinct from the neo-mechanism of Helmholtz, Du Bois-Reymond, and their followers. This not only calls into question the established narrative of how biology supplanted conceptions of mechanism, vitalism, and the soul but also recontextualizes the place we accord to substances of intoxication in the modern world.

Social Roles in Scientific Collaboration

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Collaboration in science is often distinguished by members' relations of epistemic dependence (Hardwig 1985; Scheman 2001). Andersen and Wagenknecht (2013), for example, outline different sorts of epistemic dependence relations, which play a significant role in how scientific groups function such as how they divide cognitive labor. I expand this conversation by analyzing another, though underexplored, type of dependence relation: collaborators' social roles. In this discussion, social roles refer to the assigned position a collaborator occupies in the context of a joint endeavor, such as one's role in a co-authored paper (e.g., first author, last author). Practices like co-authorship reveal that social roles confer group members differential degrees of authority. Collaborators are often not equal to each other and their interactions are mediated in part by hierarchical relations. If my analysis of social roles is right, what implications might this have for epistemic dependence relations? I argue that we should understand the dynamic

between social roles and epistemic dependence to be an interactive one, but one that can sometimes lead to tension or conflict among collaborators. I then consider whether Longino's (2002) conditions for scientific inquiry could be applied to collaborative groups in science to promote well-functioning collaboration. My analysis shows, however, that the conditions are not sufficiently sensitive to the social structure and organization of these groups. I end by proposing some tentative alternative conditions that promote well-functioning collaboration, which takes as its starting point the inequality of power among collaborators.

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C.I. Lewis, F.P. Ramsey, and Scientific Theory Change

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Recent scholarship in the history and philosophy of science highlights the importance of 20th century Kantian philosophers to theories of the relativized a priori elements of empirical knowledge. C.I. Lewis' conceptual pragmatism stands out in particular to practice-oriented philosophies of science: his notion of the pragmatic a priori is promising to understand the dynamic aspects of scientific practice in terms of the needs and interests of an intellectual community. In this vein, this paper seeks to refine a Lewisian account of scientific theory change concerned with scientific practice. I argue that Lewis' view of the pragmatic a priori nicely accounts for major change to a theory's conceptual scheme – where an entire theory is abandoned for another – but struggles with minor changes that don't involve theory abandonment. The problem turns on Lewis' view that the a priori elements of a theory are fixed: any change in conceptual scheme entails wholesale change of meaning and thus change of theory. Yet this does not do justice to the fact that scientific theories are sometimes stable under conceptual change. I look at the history of the electron in the 20th century to help establish the existence of such dynamic but stable theoretical practices. I then propose that we can address this problem by looking to the work of F.P. Ramsey. Ramsey and Lewis' respective species of pragmatism share enough affinities to make their comparison well-founded, and moreover, we find in Ramsey an insight applicable to Lewis' account of theory change. The key lies in Ramsey's notion of an 'existential judgement' – roughly, that the excess theoretical content of a theory can be expressed as an existential assertion about theoretical entities. I argue that existential judgements are compatible with Lewis' view of conceptual entities as a priori elements of a theory, and offer us a way forward in developing a Lewisian account of scientific theories that can sustain dynamic conceptual change without forcing theory change.

Evaluating STEM Outreach Programs: What Works Best and How Would We Know?

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STEM (Science, Technology, Engineering, and Math) outreach programs, especially those oriented towards youth, have become very common in Canada. This is evidenced by the hundreds of individuals and organizations that have been awarded NSERC (Natural Sciences and Engineering Research Council of Canada) PromoScience grants. Despite the ubiquity of STEM outreach programs, the shared goal of providing meaningful and foundational STEM experiences for youth, and the frequency with which these programs undergo self-evaluation, little systematic information is available about best and common practices in STEM outreach evaluation. To address this gap, we set out to collect information about the self-evaluation tools used in Canadian STEM outreach programs by sending email inquiries to representatives for all English-language NSERC PromoScience programs. We contacted 200 programs and heard back from about 100 of them, for a response rate of 50%. Of those 100 programs, about 90 of them used some sort of formal evaluation tool. The results led to some useful typologies for categorizing approaches to evaluation, such as: output vs. outcome, quantitative vs. qualitative, metrics vs. surveys, and general vs. specific. Synthesizing the approaches in this way allowed us to observe that low-resource methods (i.e., simple metrics recording, brief post-event surveys) are very common, and few programs make an effort to measure their long-term impacts, despite the level of importance placed on them in general. We conclude by highlighting some potential cross-cutting solutions that attend to both short-term and long-term impacts while respecting resource limitations.

The Reichenbach Scare: Cold War Reason and Closing the Gemeinschaft Gap

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Hans Reichenbach is a curious figure in the history of analytic philosophy. In the standard histories of analytic philosophy offered by analytic philosophers, logical empiricism has pride of place but for reasons (verificationism, logical analysis) largely unrelated to Reichenbach's concerns; hence, while Carnap always and Schlick often make important appearances, Reichenbach is often marginal to the historical accounts given. There is, however, a genre of history of analytic philosophy offered by those who do not wish to self-describe as analytic philosophers in which Reichenbach, by contrast, plays an outsize role: Both Philip Mirowski and John McCumber offer accounts of the notion of reason in logical empiricism that connect logical empiricism to rational choice theory and Cold War reason, and both give Reichenbach pride of place in this story. It is an irony of history that a German Jewish

social democrat came to the USA and changed philosophy into a cog in the Cold War machine of the USA, but this is an irony that, on their accounts, we must live with.

In this paper I rely especially on McCumber's articulation (following S.M. Amadae) of rational choice theory in Chapter 3 of his The Philosophy Scare to argue that, contrary to McCumber's account of Reichenbach's Rise of Scientific Philosophy (in his Chapter 4), Reichenbach does not endorse all the elements of rational choice theory. Indeed, Reichenbach's Rise denies both the fixity and givenness of preferences and, in so doing, denies the implicit individualism of rational choice theory. Moreover, while the preferences of others might not be directly criticizable on ethical grounds for Reichenbach, he does stipulate that the crucial political and ethical questions of community arise from the differences between and the need to harmonize preferences. I trace the hybridity of Reichenbach's actual views to their sources in his early (1910s) attempts to provide an ideology to democratic socialism and to his general engineering conception of scientific philosophy. My story also has an irony in it: it reveals that the very sort of democratic socialism that American cold war theorists could not adequately theorize (usually assimilating it to communism) remains untheorizable in at least some forms of contemporary American criticism of Cold War reason (which, by contrast, assimilate it to capitalism).

Representation and Representational Explanation
Andrew Richmond Columbia University

Cognitive science understands the brain in representational terms. E.g., the role of the hippocampus in spatial navigation is understood by appealing to the kinds of spatial properties that hippocampal cells represent, and the operations hippocampal circuits perform on those representations (Moser, Kropff, & Moser, 2008). Philosophers concerned with representational explanations like this want to provide them with philosophical elucidation and grounding. They have tended to approach that task by first giving a metaphysics of representation: a definition of the property of representation, or a set of constitutive conditions for that property.

I make a distinction between that approach—the Metaphysics First approach—and an alternative: the Explanation First approach. The latter has the same goals of elucidating and grounding representational explanation, but approaches this task by investigating the role representational notions play in cognitive scientific explanations, leaving aside any property those notions might refer to. I motivate the Explanation First approach and use it to build an account of representational explanation. That account elucidates and grounds representational explanation solely by describing the kinds of explanatory resources that representational notions bring with them, and how those resources serve cognitive science's goals. I show that we need not even consider the metaphysics—or even the potential existence or non-existence—of a property of representation.

I then put this account of representational explanation to work, intervening in two debates: how to characterize the hippocampus's representations; and how to understand the debate between representational and non-representational approaches in cognitive science.

Les créoles comme modèle de changement scientifique
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Thomas Kuhn a construit une historiographie de la science très influente sur une erreur: l'incommensurabilité comme faute de traduction. Plus de 50 ans d'analyse critique des travaux de Kuhn par Feyerabend, Hacking, Hattiangadi, Barnes & Bloor et d'autres et, étonnamment, tous ont été aveugle à l'erreur.

Inspiré par les créolistes (en particulier Mufwene et DeGraff), cet article propose que le débat est marqué par une confusion entre le raisonnement diachronique et synchronique. Contrairement à une traduction incomplète entre des théories scientifiques qui sont historiquement liés, c'est la formation du langage qui offre le bon modèle pour le progrès scientifique. Plus précisément, l'étude de la formations des langues créoles offrent une sortie de l'impasse entre progrès comme accumulation de connaissance et progrès comme rupture avec le passé, un dilemme que Kuhn et ses interlocuteurs ne parviennent pas à résoudre dû à la confusion selon laquelle la thèse de l'incommensurabilité traite une étape passée de sa langue et une langue étrangère comme étant de façon équivalente différente de sa langue actuel.

Dans cet article, je voudrais revenir sur le passage de la mécanique newtonienne à la mécanique relativiste, un exemple canonique de révolution scientifique, pour évaluer ce nouveau modèle de développement scientifique comme créole. Cela ouvre des portes. Par exemple, la démonstration que la relativité restreinte est une étape moins révolutionnaire dans le contexte de l'électrodynamique qu'il le semble lorsque comparé à la mécanique newtonienne. La science en tant que créole met en évidence le fait que le célèbre article sur la relativité d'Einstein en 1905 n'était pas en rapport avec Newton ou même Maxwell mais avec la théorie de Hendrik A. Lorentz. C'est une position qui mérite plus d'attention.

Reconceiving modeling: From representation to enaction and construction
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In philosophy, we use the term "model" to refer to a computational, mathematical or concrete object that scientists use for learning about some system of interest: a "model" is a representation of the "target" system, and this representational relation is what makes the model informative about its target (e.g., Weisberg 2013, Morrison 2015). This sense of "model" is in direct opposition to how we use the term in ordinary situations. In educational contexts, for instance, the behavior exhibited by an expert (e.g., a parent) is the "model" that the apprentice (e.g., a child) is to emulate (rather than, in the philosophical-scientific sense, calling the learner's behavior a "model" of the expert target). In ordinary life, a "model" is something we draw inspiration from for learning, not the product of the learning process.

This paper proposes a shift away from the dominant philosophical conceptualization and toward the ordinary sense of "modeling." The proposal frames model-based science in terms of enaction and construction. Scientists identify a model system in the real world (i.e., the "target" of investigation) and, through building artifacts ("models" in the traditional philosophical sense), scientists enact their understanding of the real-world system. This process is constructive in the literal sense of involving building artifacts, but also in the sense that scientific understanding arises (i.e., is constructed) through engagement with those artifacts. This shift reorients inquiry away from puzzles about representation and toward questions about embodied learning in continuity with non-scientific instances in ordinary life.

Exploring the Networked Subject Through Reality Augmenting Technologies
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This is an age of networks: information networks, networks of actors — human and non-human alike — and through these networks, the networked subject. The networked subject is defined as "a subject adrift in a field of mediation" (Coleman, 2012 79), where the subject is conceptualized by Foucault as a social construction (Foucault, 1995) and by N. Katherine Hayles as "an amalgam, a collection of heterogeneous components, a material-informational entity whose boundaries undergo continuous construction and reconstruction" (Hayles, 1999 3). Working through these three definitions, this paper then considers the embodied post structuralist, post human, networked subject and its relation to wearable technology through its reality augmenting capabilities. Notably, this paper takes to Hayles' argument that the human cannot be defined as solely information patterns, but that it is constituted by its very material being that allows for interaction with other objects, subjects and information. As such our interactions with wearable technology offer processes of subjectivation not only through the close proximity of the physicality of the wearable technology that binds it to the human body, but also through the informational processes that it mediates. Specifically looking at smart glasses — Heads Up Displays (HUDs) or Head Mounted Displays (HMDs) — as a locus for the interrogation of processes of subjectivity, this paper seeks to address how the networked subject is produced and made amenable to embodied information technologies. Largely looking to differentiate between the human computer interactions between the subject and wearable technology and the subject and the non-wearable technology, this investigation will argue that by wearing the technology that embodies a network of information the subject no longer simply participates in it but becomes the network. Exploring what it means to be a post human subject in an era of what Katherine Hayles calls informatics, which is constituted by "a capitalist modes of flexible accumulation," "the integration of telecommunication and communication technologies, the increased significance of big data, and the "reconfiguring" of embodied behaviours with information technologies (Hayles, 1999 313). To determine the role of the smart glasses in creating processes of subjectivation, this paper will consider its augmenting reality (AR) capabilities as the manifestation of an infrastructure for information flows. As an example, this paper will consider the role of AR as an infrastructure in labour and manufacturing.

How to Put the Cart Behind the Horse in the Cultural Evolution of Gender
Daniel Saunders University of British Columbia

In The Origins of Unfairness, Cailin O'Connor develops an evolutionary game model to show how gender might have emerged. She suggests it was originally a device for coordinating the division of labour. Without social roles, it can be difficult to coordinate on the issue of who should perform which tasks. Sexual differences are one salient feature in early human societies that could provide a basis for the division of labour. Once endowed with social significance, sexual difference can transform into the autonomous cultural force of modern systems of gender.

Her models are illuminating but have a difficulty. She assumes that agents engage in gendered social learning as the mechanism by which successful strategies spread through a population. But this seems to put the explanatory cart before the horse. It's not clear how early humans could have a well-developed system of gendered social learning before the gendered division of labour. One possibility is that gendered social learning and the division of labour incrementally co-evolved. But no formal model of such a process is currently available.

This paper closes that explanatory gap. I construct an agent-based model that represents an evolving population faced with coordination problems. I show this model replicates the core results O'Connor found using an equation-based modeling framework. However, the agent-based model also provides additional structure to explore more complex social learning behaviours. I show that, under a variety of conditions, gendered social learning and the gendered division of labour can co-evolve.

Policing in the Age of Algorithms
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Science and technology greatly influence public life, and artificial intelligence (AI) is changing the political and legal landscape of Canadian society. Algorithmic policing practices are becoming more pervasive in Canada and now, more than ever, there is an urgent need to examine the use of AI in policing to determine the ethical underpinnings of these technologies, as well as the political and legal consequences of putting them into use. Emerging literature discusses the use of algorithms in policing in the context of criminal justice and human rights, which is a valuable framework for analyzing the current scenario. However, in this paper, I will argue that we ought to place the use of artificial intelligence (AI) in policing within the broader discussion of settler colonialism. Framing the discussion within an analysis of settler colonialism more adequately reflects the level of discrimination and racism that is inherent in these technologies.

To demonstrate, I will look to Indigenous movements such as Idle No More to argue that surveillance is a form of settler colonialism that deliberately discriminates against minority groups. I will argue that if we are to truly appreciate the problems of algorithmic policing, then this context—of settler colonialism—is crucial for our analysis.

In outlining these background conditions, I hope to reveal the underlying power structures that mediate the use of artificial intelligence, demonstrating why AI is not neutral or objective. I will show how AI is being used in Canadian policing practices, focusing in particular on the surveillance algorithms being used by the Calgary Police Service and other Canadian policing agencies. I hope this analysis sheds light on the complexity of artificial intelligence while also offering some insight into the ways that settler colonialism informs our view of science and technology.

Cassirer on Representation, Representationalism, and Physical Knowledge
Benedetta Spigola University of Roma Tre

In this paper I will argue that Cassirer makes a case for a nonrepresentationalist model for physical knowledge. Roughly put, representationalism (R) is the view that a proposition p (belonging to a certain physical model) holds for a part w of the world iff p appropriately depicts or accurately reproduces w. It is then arguable that R takes the mathematical formulas to be the most appropriate or accurate representations of what there is in the world. However, R has been seriously undermined by quantum mechanics. For instance, Heisenberg's matrix mathematics shows that the object or set of objects of this kind of mathematics is in fact nothing representable in any cognitive sense. Cassirer's main agenda in the philosophy of science is to make the general claim that R is no good theoretical explanation of physical knowledge. Cassirer argues that physical knowledge obtains as a process of idealization (rather than of representation) through which the very object of physical knowledge can be first grasped. By Cassirer's light, the mathematical formulas do not correspond to anything given sensu stricto as a part of the world, but, rather, intervene as constitutive elements of what can in principle count as the object of physical knowledge, and thereby correspond to nomic structures. These nomic structures are, in Cassirer's idiom, 'symbols', which as such do not denote objects properly, but systems of instructions for operatively ordering the worldly domain of objects. I will then argue that Cassirer advocates an operationalist model for physical knowledge.

Metaepistemology of Tools that Extend the Power of the Human Scientific Imagination
Michael Stuart University of Tubingen

There is a set of epistemic tools used in science that should be characterized as tools of the mind, in Francis Bacon's sense. These tools operate by providing impetus to the imagination, and guiding it in certain ways. There is a lot written about the use and epistemology of several of these tools, including thought experiments, mental models, diagrams, analogies, metaphors, and formal languages. But little has been said about the way we should go about doing epistemology of these tools, considered as a set. That is, how can we define a good tool of imagination if we cannot define a good imaginative act? I argue that a good imaginative act can be understood in at least three different ways, following epistemological consequentialism, deontic epistemology, and virtue epistemology. If we leave things here, "a good imaginative act" is ambiguous, and this ambiguity spreads to tools of imagination: a tool can also be "good" in at least these three senses. To judge between them, I see how well each is able to capture what scientists themselves say about imagination. I conclude against deontic epistemology of imagination, and for both consequentialist and virtue epistemology, though these two frameworks are used for very different purposes by scientists: the former to evaluate past imaginings, and the latter to improve imagination for the future.

Deduction and Induction: False Dichotomy?

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Since Sextus Empiricus, we understand inductive inferences as non-deductive. Sextus was right to believe his notion of induction was non-deductive, since he understood inductive inferences as universalizations over a potentially infinite number of entities. Our modern understanding of induction, however, is more subtle. We now think of inductive inferences as projections from one set to another (Goodman 1955). Given sufficient information about one set and the relation between the two sets, a projection can be deductive. This brings the question: is the dichotomy between induction and deduction outdated?

I build a case for the affirmative. My desiderata for our notion of induction are that, (1) if possible, it should (pragmatically or epistemically) support our use of induction in science and, (2) the resulting notion of induction should correspond to our technical use in science. First, with the (logical) problem of induction (Hume 1739, 1748; Popper 1972) and the Popper-Miller theorem (1983), we have good reasons to believe that any non-deductive notion of inductive inference will result in unreliable inferences or inference whose reliability is without satisfying explanation. Neither option does justice to the apparent reliability of induction or the faith we put in it. As for (2), the material theory of induction (Norton 2003, 2014) proves itself to be a relatively accurate description of how we use inductive inferences. I argue that a deductivist version of Norton's theory would do a better job at identifying how we understand underdetermination and how we deal with it.

On the Structural Nature of Entanglement
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A central question in the philosophy of quantum mechanics (QM) is that of which ontology the theory is committed to (Ney and Albert, 2013), (Lewis, 2016). In this paper I challenge this question: my claim is that by looking at the case of entanglement it emerges that the role of QM as a physical theory is rather to be a structural framework that provides the formal categories to model nature, and thus questions about ontology should be discussed separately. I focus on the experiments that allow to observe the peculiar features of the correlations among entangled subsystems (e.g. Bell tests (Genovese, 2005)). I highligh the fact that entanglement embeds the observational data in a coherent picture but, at the same time, these latter remain on a separate level from the notion of entanglement as formalized in QM.

On one hand, entanglement is the general form of a quantum state, defined via the notion of non-separability (Horodecki et al., 2009). On the other, what is observed is always a set of particular, locally accessible states of aair, analysed through suitable theories that refer to a specific ontology. Certainly, measurement outcomes displaying quantum correlations can be understood only if interpreted within the framework of entanglement, but this latter remains a pervasive, structural feature that is not tied to any particular class of physical objects. Symptom of the separation between entanglement and observational data is the fact that one ought to refer to additional theories in order to devise an experiment, such as, e.g. for photonic experiments, quantum optics and QFT. In my view, this is because entanglement is a theoretical structure that comes both ahead and independently of any particular ontological claim.

-V-

The Circuit and the Bath: Cultures of Energy Modelling at the University of Cambridge, 1960-1980
Theodora Vardouli McGill University **and Salmaan Craig** Mcgill University

In the 1960s, aligning with priorities of government funding agencies, building scientists and architects in UK research institutions developed mathematical models of energy in buildings. This area of research also attracted applied mathematicians and theoretical physicists researching turbulent flows in the 1980s. This paper sheds light on proximities of these research communities catalyzed through specific modelling practices. With focus on the University of Cambridge, a key hub of energy modelling activity, the project examines two intellectually, operationally, and materially distinct modelling paradigms: the "circuit" and the "bath". Circuit models conceptualized energy as electricity flowing through a network of discrete building components. Bath models approached energy as continuous and irregular flow, like a liquid flowing through a mass. Through oral history interviews and primary source research on the groups that developed the "circuit" and the "bath" at the University of Cambridge Department of Architecture and the Department of Applied Mathematics and Theoretical Physics respectively, the paper maps the historical contexts and intellectual lineages of these two approaches. It brings forward cultures of abstraction and empiricism among the models' makers, and interprets them in relation to disciplinary commitments and institutional conditions. By extrapolating on the visual and material attributes of the two models, it also contributes an inquiry of models as agents of convergence: despite their distinctive differences, the circuit and the bath helped shape a middle ground of abstraction in which physics equations acquired a concrete, material body and architectural bodies acquired calculable network skeletons.

Representing Non-Actual Targets?

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Scientists seek to learn about targets of interest by representing them with models. This supposes that we have an account of how models represent. Typically, such accounts have been concerned with real-world targets or, more precisely, actual targets. However, some models appear to not have actual targets (Weisberg 2013, sec. 7). This raises a puzzle: how could models represent non-actual targets?

This paper aims to answer that question using Frigg and Nguyen's (e.g. 2016) denotation-exemplification-keying up-imputation (DEKI) account of scientific representation. The argument proceeds in two steps.

First, illustrating with the case of the perpetual motion machine (Feynman, Leighton, and Sands 2010, ch. 46), I argue that models with non-actual targets targets do not, contrary to what we might believe, necessarily fail to denote. Denotation failure implies failure to be an epistemic representation. This is at odds with a naturalistic interpretation of scientific practice. My proposal, which avoids postulating ontologically problematic entities (e.g nonexistent objects), is that these models simply denote properties of the actual world.

Second, I argue that that the properties we key up and impute with these models are modal properties of actual targets. I illustrate with a case from economics (Arrow and Debreu 1954) and show that the general equilibrium model exemplifies the properties a system needs to have in order for the equilibrium to exist. Then, modal properties are imputed on actual economic systems.

This accounts for how, within the DEKI framework, models with ostensibly non-actual targets may represent.

Experience as expertise: Clara Park's call for valuing a mother's knowledge
Marga Vicedo University of Toronto

First-hand experience has been considered crucial for obtaining knowledge in many realms. Yet, personal experience has also been suspect since the knower's subjectivity could bias what many sciences saw as necessary for objective knowledge: detachment from one's object or subject of study.

In the field of child development, the first-hand experience of parents in childrearing has had little impact. In the early twentieth century, researchers welcomed the observations provided by mothers. However, as the field became professionalized, male researchers dismissed patience, sympathy, and other traits associated with maternal care as interfering with scientific investigation.

This paper examines how Clara Park challenged that stance in her writings about raising her autistic daughter Jessica. In her 1967 book The Siege and other writings Park called upon scientists to recognize that daily contact with their children allowed parents to acquire "deep knowledge of the child in context." She argued that experience was one type of expertise that could complement clinical and research work. Park fought to have a mother's voice recognized as a legitimate source of expertise.

In this paper, I situate Park's work within a larger historical and philosophical discussion about experience and expertise.

-W-

Accommodating Horizonal Inheritance in Phylogenetics
Aja Watkins Boston University

Phylogenetics is the study of evolutionary relationships between different populations. A phylogeny is a "tree" that shows how the populations are related, i.e., in what order the populations diverged from each other. Currently, all phylogenetic trees have a strictly branching structure: two lineages which have diverged can never merge back together (e.g., Sober 1988). Horizontal inheritance in general, and lateral gene transfer in particular, challenges this assumption of phylogenetics. Microbial organisms, at least, most likely have a phylogenetic history with a reticulate structure, one where the branches can reconnect. Even some macroorganisms, especially plants, have been known to hybridize, which would result in the merging of two distinct lineages (Velasco & Sober 2010). Algorithms used in other areas can provide guidance for revising phylogenetic algorithms to accommodate for horizontal inheritance. For example, recent research on human evolutionary history has shown that our ancestors often met and interbred (e.g., Reich 2019). This research on human population mixing events uses algorithms that allow for a reticulate tree structure, which I believe may be applicable to the much larger Tree of Life. In this paper, I use these other algorithms to propose that phylogenetic algorithms be adjusted to accommodate for lateral gene transfer, hybridization, and other forms of horizontal inheritance.

Kant on Mental Association and the Limits of Physiological Explanation
Aaron Wells University of Notre Dame

In the Anthropology from a Pragmatic Point of View (1798), Kant states that "it is futile to demand a physiological explanation" of "the law of association" that governs mental representations. I will argue, first, that this claim about the limits of physiological explanation is independent of Kant's deflationary attitude towards various versions of the mind–body problem (such as the

metaphysical problem of mind–body interaction). I then lay out an apparent tension between this claim about the limits of physiological explanation of association, and more optimistic remarks Kant makes on physiological hypotheses. Finally, I argue for a reading that defuses this tension: physiological hypotheses can account for some necessary conditions for association, but are not sufficient to explain the rules that produce associative phenomena. Along the way, I hope to situate Kant's claims in the context of early modern work on anatomy and psychology (e.g., Boerhaave, Reil, Bonnet, Tetens, and Sömmering). I also draw conceptual connections to more recent debates about the feasibility of localizing mental activities in the brain (and in particular, the issue of how this task might differ for different kinds of activity). And I bring out overlooked links between Kant's views on the inscrutability of association and his account of prejudice—which is often caused by association and can be fruitfully compared to some contemporary models of implicit bias.

Singing Immunological Songs: An Exploration of Holobiont Individuality
Alistair Whittle University of Bristol

Several biologists/philosophers have proposed that macrobes and their associated microbes should be considered as biological individuals called holobionts. Pradeu (2012; 2020) proposes that physiological individuality is determined by immune system (IMS) processes. This essay argues that, because IMS processes are dependent on microbial partners, credence is lent to considering holobionts as collectively produced physiological individuals. However, the microbial taxa with which many putative holobionts form functional partnerships varies greatly, intra-individually over time, inter-individually, and intergenerationally. As such, it does not seem straightforward to account for the persistence of such holobionts under standard models of evolution by natural selection, the weak partner fidelity producing a disintegration of reproductive lineages such that the traits produced by these partnerships do not seem to be heritable. Addressing this issue, the essay draws on the recently formulated 'it's the song not the singer' (ITSNTS) theory of evolution (Doolittle & Booth, 2017) to explain the persistence of such partnerships. We start from the ontological position that biological processes exist and that these are characterised by underlying, immaterial interaction patterns between entities. These interaction patterns are cast as units of selection that recur over time by being recreated by the various entities that enact a biological process. Finally, the essay suggests that viewing immunological processes under the ITSNTS framework can, to an extent, bring together notions of physiological and evolutionary individuality in holobiont theory.

A Turn at the Wheel: The three lives of the Rota Aristotelica
Jennifer Whyte University of Pittsburgh

It should come as no surprise to historians that a paradox is a difficult thing to understand. A paradox is fundamentally a problem in understanding, not a product of it. Though it is common to write about paradoxes as free-standing puzzles and brainteasers, I will argue that this divorcing of paradoxes from the contexts in which they are actually used by science and philosophy obscures their meaning. In this paper, I will present the well-known case of Aristotle's Wheel, the Rota Aristotelica, and argue that modern interpretations of it in the literature about Thought Experiments do it a disservice by separating it from its context. In particular, I will argue that there are three possible ways to interpret the Rota Aristotelica, each proper to the goals of the context in which it was written. Each of these ways of understanding the Rota Aristotelica is different from the others in two ways: each answers the problem in a different way, and each endows the answer to the puzzle with a different significance. In particular, only one of the interpretations of the Rota Aristotelica can make sense of its role in the 17th century debate over the status of the vacuum. I will argue that by glossing over the philosophical and mathematical context in which the Rota Aristotelica is being presented, we lose the ability to understand its meaning.

The Ghost in the Machine: Structural Metaphors in the 'Golden Age' of Artificial Intelligence Research, 1956-1976
Joseph Wilson University of Toronto

This presentation is based on an analysis of the language used in a corpus of academic papers from 1956-1976 that are generally considered to be the foundational documents of the field of artificial intelligence. Thirty-one papers were assembled and examined for evidence of the use of structural metaphors (Lakoff & Johnson, 1980), first manually with an adapted version of the Metaphor Identification Procedure (Pragglejaz, 2007), and then through key-word-in-context searches (Deignan, 2008) with online corpus analysis tool Sketch Engine. Concordance data shows that the scientists frequently used metaphors to make sense of their work, some of them eventually coming to define the entire field of study. Some structural metaphors used imagery from the same source domain, suggesting underlying root metaphors (Pepper, 1972), evidence of particular perspectives that comes to constitute the academic field. Root metaphors such as A MACHINE IS A BRAIN or RESEARCH IS A JOURNEY were extremely successful in communicating non-observable phenomena between scientists. Other structural metaphors appeared briefly in the literature but soon disappeared from discourse such as Oliver Selfridge's suggestion that COMPUTER PROCESSING OCCURS IN HELL (specifically, the version of Milton's hell from Paradise Lost he called Pandemonium). There is a current renaissance of the spirit of AI's Golden Age that employs different metaphors: today we speak of record-high investment in 'deep learning' and 'neural nets'. The choice of metaphor when communicating science is important as it affects how scientists think about their work and how the public comes to understand what is going in R&D Departments and computer labs around the world.

The Dialectic of Aristotle's De Divinatione per Somnum

Adam Woodcox Saint Mary's University

In his 1900 work The Interpretation of Dreams, Sigmund Freud praises Aristotle for naturalizing the phenomenon of dreaming. According to Freud, Aristotle's predecessors regarded dreaming as something supernatural and god-sent, the product of divine inspiration. Aristotle, by contrast, approached dreaming as a problem of psychology, the product of the dreaming psyche which is 'subject to the laws of the human spirit'. This paper has three goals. First, I will discuss in outline the supernatural accounts of dreaming and divination in ancient Greece before Aristotle, in order to establish the relevant intellectual background to Aristotle's naturalized account. This will involve, in particular, a cursory analysis of the views of Homer, the Presocratic natural philosophers, the Hippocratics, and Plato. From this analysis, it becomes clear that the belief in prophetic dreams was widely held in ancient Greece, although we find no sustained investigation of the phenomenon before Aristotle. This will set the stage for the dialectic of Aristotle's trilogy on the subject: De Somno et Vigilia, De Insomniis, and De Divinatione per Somnum. The second goal of this paper is to show that although Freud's praise is not necessarily misplaced, Aristotle's views on dreaming—and more specifically the phenomenon of divination in dreams—remain to the modern ear very strange and (at least quasi-) supernatural. Aristotle's work marks an improvement over his predecessors, but it is not the wholly naturalistic account that Freud suggests. Finally, I hope to offer some insight into Aristotle's methodology and show, in particular, that his short treatise De Divinatione per Somnum offers a clear-cut case of the dialectical method at work. (Briefly, I assume that an argument occurs in a dialectical context when its premises are endoxa and serve as premises under that very aspect or description, i.e., qua endoxa.) I argue that Aristotle's explanation of divination in sleep is guided in large part by certain endoxa—most especially the significance assigned to dreams by all (or most) people and the attention of the practising 'scientific' physicians. Accordingly, this paper illustrates the influence and authority that Aristotle assigns to endoxa and the role of dialectic in his natural science.

-Y-

The Epistemology of Scientific Explanation

Haomiao Yu University of Guelph

In this presentation, I shall present a virtue epistemology for scientific explanation. In so doing, I compare several models of scientific explanation in the literature and identify two ways of theorizing scientific explanation -- the structuralist approach and the epistemic approach. I argue that the structuralist way of modeling scientific explanation is aiming at finding a better structure of scientific explanation. They fail to explain why scientific explanation explains to humans. The leading figures of the structuralist approach include Hempel (1965), Kitcher (1989), Woodward (2003), and Bokulich (2011, 2012). On the contrary, the epistemic approach takes into account epistemic virtue and thus captures the explanatory element in scientific explanation. The leading figures of the epistemic approach include de Regt (2017), Khalifa and Gadomski (2013). Furthermore, I develop the epistemic approach into a virtue epistemology for scientific explanation. I build the connection between epistemic virtue and scientific explanation through understanding. First, I divide epistemic virtues into agent-virtues (virtues possessed by agents/scientists) and explanation-virtues (virtues possessed by scientific explanation). Second, I discuss how our understanding of the phenomena can be achieved by the collaboration of agent-virtues and explanation-virtues. The explanation-virtues I discuss include intelligibility and visualization proposed by de Regt, unification downplayed by Morrison, and several others (e.g., simplicity, scope, mechanism, analogy, testability, fruitfulness) listed by Khalifa and Gadomski. I argue that these explanation-virtues, coupled with agent-virtues (e.g., perception, reasoning, imagination, generalization, etc.) are the key to scientific understanding and thus the reason why scientific explanation speaks to humans.

Biology and Its Tale of Two Reductions

Sun Kyeong Yu Minnesota State University, Mankato

Reduction of biological phenomena has been attempted on the basis of the layered model of properties. On this model, properties are construed in a hierarchical fashion where a macro-property on a higher level is in a mereological relation to a micro-property at a lower level. When a macro-level entity is decomposed into micro-level entities, a property of a macro-level entity is reduced to the properties of micro-level entities and their relations.

I argue that, although the layered model has at times been useful, it is not the only available form of a comprehensive ontology which reductionism in biology should be based on. Inspired by Jaegwon Kim's analysis of second-order properties and reductionism, I introduce the 'no-level' ontological framework in which biological properties are analyzed in terms of base/higher-orders of properties. On this new model, all the different orders of properties exist at the same level if in the micro/macro-level hierarchy of properties.

I propose that this new ontological framework leads to a fresh model of reduction for biological phenomena, 'no-level reduction', which provides a better understanding of what reductionism in biology needs to be like. The integrated 'level and no-level' framework offers an ontology that classifies biological properties in a more fine-grained way. It also explains more appropriately the two kinds of reduction, level reduction and no-level reduction, for biology.

-Z-

The Importance of (An Account of) Empirical Adequacy
Helen Zhao Columbia University

On a standard view, empirical adequacy is a requirement of scientific theories. While moral and political values may determine which and whose empirical evidence to hold a theory accountable to, every scientific theory must be accountable to some body of evidence (Nelson and Nelson 1994, Anderson 1995, Longino 1995). Empirical adequacy is a "minimal" criterion, essential "if we are to be assured we are on the right track" (Douglas 2013).

Recently, Sindhuja Bhakthavatsalam and Nancy Cartwright (2017) have mounted a forceful challenge. They have argued that some scientific aims are best served by theories that "need not be anything like empirically adequate," and, more urgently, there are contexts of theory choice where "empirical adequacy is a positive vice."

I show that Bhakthavatsalam and Cartwright's disagreement is founded on unacknowledged disputes about two features of empirical adequacy. First, the authors dispute fit: the relation a theory must bear to the empirical evidence in order to count as adequate to it. For Heather Douglas and others (e.g. Laudan 2004), a theory must explain the evidence; for them, a theory must be true to the evidence. Second, I show that Bhakthavatsalam and Cartwright disagree about coverage: which phenomena an empirically adequate theory must fit. While others take the phenomena to be circumscribed by the intended domain of application, on their view, it is the domain of application implied by the theory that counts. I argue this exchange demonstrates the literature's need for a more structured and precise account of empirical adequacy.

Two statistical approaches to simplicity: a comparison
Shimin Zhao University of Wisconsin, Madison

Simplicity is often considered as a theoretical virtue and cited in debates about theory choice. But what does simplicity mean, and how to justify the preference for simplicity? This paper will analyze and compare two statistical approaches to simplicity: Akaike's Information Criterion (AIC) and Formal Learning Theory (FLT). AIC justifies a preference for simplicity in model selection: when multiple models fit the data equally well, models with fewer adjustable parameters will have a greater predictive accuracy (Forster and Sober 1994). FLT justifies a preference for simplicity in finding the correct hypothesis: if one always chooses the simplest hypothesis compatible with the evidence, one would converge to the truth reliably and efficiently (Kelly 2007). Given this similarity, it is natural to compare AIC and FLT and ask which provides a better justification for simplicity.

By trying to take AIC out of its original context and applying it to the kind of problems that FLT typically deals with and vice versa, I will show that

1. A meaningful application of each approach demands specific assumptions about the evidence and the theories under consideration. And

2. The assumptions that one approach requires are often unsatisfied in a different context.

Therefore, despite their loose connections with simplicity and theory choice, AIC and FLT are approaches that are designed in substantially different contexts; they do not compete, and the question "whose justification is better" does not arise.

Abstracts for sessions / Résumés pour les séances
in order of programme schedule

SESSION – DECENTERED SCIENCES: BEYOND CENTRE/PERIPHERY STORIES

Session organizer: Ernst Hamm, York University
Chair: Gianamar Giovanetti-Singh, University of Cambridge

The Discovery of Binary Numeration: Leibniz, Caramuel, and the New World
Maria Amuchastegui York University

Leibniz claims, in his 1703 Explication de l'arithmétique binaire, that he invented binary numeration, a claim that has remained largely unchallenged. Most histories of mathematics and computer science credit Leibniz with the discovery of the numbering system that would later form the basis for modern computers. Although the priority dispute between Leibniz and Sir Isaac Newton over the invention of calculus has been extensively studied, the dispute over the discovery of binary numeration has received little attention. Juan Ares, María Aurora Martínez, Juan Alfonso Lara, and David Lizcano contend that Leibniz plagiarized the Spanish scholastic philosopher and mathematician Juan de Caramuel y Lobkowitz (1606-1682), who was in turn inspired by New World accounts of alternative numbering systems, in particular the counting practices of the Guaraní Indians of Paraguay. A more nuanced interpretation of Ares et al.'s allegation is needed, one that takes into account 17th and 18th century norms for attribution, the relationship between Spain and the northern Europe, and the role of institutions in legitimating Leibniz's priority claim. It was the Académie Royale des Sciences that first published Leibniz's Explication de l'arithmétique binaire, an act that instantly conferred legitimacy on his claim. In addition, the status of indigenous numbering systems as knowledge has not been sufficiently acknowledged. Ares et al.'s formulation assumes that it was Caramuel who "discovered" the numbering systems described in his 1670 Mathesis biceps, a formulation that denies the status of knowledge to the various numbering systems used in the New World.

Science, Empire, Centre and Periphery: Considerations via the Caucasus, Tartu, St. Petersburg and Paris
Ernst Hamm York University

Science and empire is a booming industry and for good reasons. The long reach of nineteenth century imperialism was going strong through most of the twentieth century and, arguably, lingers into the present. And science has never been far from the centre of imperialism. But the empires in question for many historians, including historians of science, have been largely Atlantic, but even there the Iberian, Dutch and even the French empires are overshadowed by the focus on the Anglo-Atlantic world. But British and American, or French, empires were not the model for all others. This paper will consider the ways in which the expedition to and ascent of Mt Ararat in 1829, an expedition involving natural philosophers from the University of Dorpat (Tartu), Russian soldiers and diplomats, an Armenian cleric, and any number of go-betweens from the Caucasus and Transcaucasia, was a project of Russian imperial science. Despite this Russian language and institutions were not at the centre of the expedition. This paper argues, along with other recent work by historians of science and empire, that too much of our thinking about science and empire is structured around rather dated ideas about British imperialism. Places like Tartu, Armenia, the Caucasus were part of an empire but not wholly defined by empire. Likewise, the natural philosophical questions that were addressed by the expedition were related to imperial concerns, but not entirely defined or limited by such concerns.

Tracking Decolonisation: The Case of the Journal of Genetics Going East
Gordon McOuat University of King's College

In 1957, the Journal of Genetics, the oldest English journal in genetical science (founded by William Bateson and R. C. Punnett in 1910) ripped up its roots at University College London and followed its colourful editor, J.B.S. Haldane, to a new home in India. The move was a political one, tracking Haldane's anti-imperialist passage to India, and strongly resisted by its European former editors and contributors. Haldane's goal in moving the J.of G. was to diversify science and its flagship journal, de-colonising both the centre of authority of science and, most importantly, its contents. Using archival and extensive bibliometric data drawn from Haldane archives and the papers of the journal itself, we will

examine the shifting ways in which the journal succeeded or failed in setting a standard of a decolonised science. Was this effort at decolonisation successful? What does it say about our perceptions of modern science being predominantly "Eurocentric"? Can science be decolonised?

J.B.S. Haldane: Charting a New Genetics Through Language and Place [Talk cancelled]
Arden Rogalsky University of King's College

In the early 1950s, eminent 20th century biologist, mathematician, and science popularizer John Burdon Sanderson (J.B.S.) Haldane began to trace the outlines of a new program in genetics around two axes of interests: the genetics of animal language, and the unique research possibilities promised by newly-independent India. This paper traces the convergence of those interests, and brings to light the common political, ethical, and methodological concerns that generated their mutual co-formation in Haldane's work. In a wide-ranging series of studies concerning animal communication, Haldane attempted to articulate an evolutionary mechanism that could explain the obscure process through which learned behaviours become inherited instincts. The kinds of research and genetic interpretation that he thought necessary were, much to his frustration, at odds with the prevailing tendencies of British genetics, then plagued by limited field study possibilities and stifling politicized demarcations in the wake of the Lysenko affair. Haldane's dissatisfaction with these limitations arose simultaneously with, and partly in response to, his encounters with a nexus of uniquely local possibilities for the study of animal behaviour in India. Among his principal concerns were India's long-standing ethical-religious practices of non-violent observation, an abundance of animal and plant life in large "unaffected" regions of the country, locally-relevant methods of population analysis, and India's political independence from the hardened binaries of genetic theory upheld by Cold War combatants. This harmonious interplay of research activity and place played an important role in motivating Haldane's move to India several years later in 1957, a transitional period which has recently received several detailed reassessments. More broadly, studying Haldane's early engagements with Indian science contributes a strong challenge to centre-periphery analyses of science in the last century, and is therefore relevant to decolonizing global histories of genetics, ethology, and knowledge transfer.

SESSION – ISSUES IN SCIENCE FUNDING POLICY

Session organizer: Jamie Shaw, University of Toronto
Chair: Rachel Katz, University of Western Ontario

Feyerabend, Pluralism, and Socially Responsible Science Funding Policy
Jamie Shaw University of Toronto

In Science in a Free Society, Feyerabend critically appraised the Bauman amendment which sought to increase congressional oversight on budget proposals for the National Science Foundation. He welcomed the Bauman amendment as a "step in the right direction" towards a socially responsible science that prioritizes research that services the general public. In this paper, I argue that Feyerabend's appraisal of the Bauman amendment reflects a failure to learn lessons from his own work on the freedom of science. Specifically, Feyerabend hastily endorse the Bauman amendment because of Senator William Proxmire's 'Golden Fleece Awards' which mocked NSF funded projects that seemed overly esoteric and bereft of practical significance. Proxmire's arguments, I argue, should not have impressed Feyerabend as they run counter to his stance on pluralism. Specifically, I contend that Feyerabend's pluralism does not allow us to coherently identify which projects are "frivolous" outside of the context of science needed for urgent situations. I go on to show that Proxmire would have been unable to satisfy this condition and, therefore, could not have given a convincing argument to a Feyerabendian

Social Science for What? Wasting Taxpayers Dollars, Winning Golden Fleece Awards
Mark Solovey University of Toronto

In 1975, U.S. Senator William Proxmire began to issue a monthly Golden Fleece Award, given to a government-funded research project deemed to have little practical value, implying that the American taxpayer was being "fleeced." This paper examines Proxmire's campaign against wasteful spending, with special attention to the case of the U.S. National Science Foundation (NSF) and its social science program. One of the nation's premier science agencies, the NSF received a number of Proxmire's unflattering awards, including one for a grant to Sherry Ortner, a rising star in Anthropology, for her project "Himalayan Mountaineering, Social Change, and the Evolution of Religion among the Sherpas of Nepal." Proxmire asked: why was the U.S. government allocating scarce resources to send Ortner half-way around the world to carry out such an esoteric study? Ortner responded by suggesting Proxmire was an ignoramus whose misguided efforts had dangerous implications for American science and foreign policy. Drawing on my forthcoming book Social Science for What? this paper

examines the heated exchange between the politician Proxmire and the scholar Ortner. As I will explain, this episode embodied fundamental and long-standing science policy challenges, which became particularly acute during the 1970s, and especially for NSF social science.

Science Funding, Unpredictability, and New Inductions
Kareem Khalifa Middlebury College

Why should we fund basic science? This question, despite its obvious practical value, has attracted meager philosophical attention. Within science policy circles, the most powerful argument for funding basic science can be called the 'unpredictability argument' where basic science should be funded because it frequently facilitates technological gains. This argument was first outlined by Vannevar Bush in Science – the Endless Frontier in 1945 and has been enormously influential ever since. Most importantly, it was crucial in the founding of the National Science Foundation. In this paper, we offer a careful reconstruction of the unpredictability argument and highlight its most contentious premise that has, thus far, escaped the attention of commentators. Specifically, this argument relies on the historical induction that basic science has demonstrated its technological fecundity though the fruits of particular basic scientific discoveries could not have been known at the time. We argue that technological progress may have no invariant causes; sometimes it is the result of unexpected sources; other times, it is not. Whether the unpredictability argument is sound depends on the relative frequencies of these different causes of technological progress. Moreover, we compare and contrast this argument with historical inductions more familiar to philosophers of science: namely, the pessimistic meta-induction and the problem of unconceived alternatives. This reveals a new set of challenges for philosophers of science that can contribute towards well-informed science funding policies.

SESSION – SCIENCE, TECHNOLOGY AND NORTHERN EXPLORATION

Session organizer: Antony Adler and Sarah Pickman
Chair: Fiona Amery, University of Cambridge

Diving Under Ice: Saturation Diving and Canadian Sovereignty Claims in Arctic Waters
Antony Adler Carleton College

Underwater habitat development during the 1960s and 1970s deserves greater attention than it has yet received as it highlights a peculiar confluence of military, scientific, and popular interest in the colonization of the seafloor characteristic of the period. Existing accounts have focused on American habitats, notably Sealab and Tektite. But this approach overemphasizes a Cold War narrative in which the sole protagonists of the habitat programs are the United States and the Soviet Union. At least 65 habitats were built between 1962 and 1991. Some were statesponsored, with significant programs run by French, German, Japanese, and Canadian teams. This paper takes as a case study the Canadian Sublimnos habitat as well as the underwater exploration programs it helped launch in Newfoundland (Lora-1) and in the Arctic (Sub-Igloo). The Canadian case demonstrates that technological expertise and public enthusiasm for underwater exploration should not be solely understood with reference to Cold War interests of the two superpowers. Rather, the international range of habitat programs of the 1960s and 70s reveals an expanding interest in the vertical underwater dimension fueled by numerous national scientific aims and territorial claims in the Arctic Ocean. Unlike habitat programs developed elsewhere, Canadian diving expeditions carried out technological trials and physiological studies specifically applicable to underwater exploration in northern environmental conditions.

"A Maximum of Nourishment with Minimum of Bulk": Arctic Exploration, Experimental Failure, and the Pemmican Campaigns of Vihjalmur Stefansson
Sarah Pickman Yale University

Canadian-American explorer Vilhjalmur Stefansson (1879-1962) is remembered today as a prolific Arctic explorer of the early twentieth century. However, he also spent the decades between 1920 and his death in 1962 collaborating with university and government scientists in the United States and Canada, eager to use his Arctic experiences to position himself as an authority on cold weather survival. Arguably, no example better illustrates this facet of Stefansson's career than his passionate crusade to promote pemmican - an Indigenous North American food made of dried meat and fat - as a military ration. This paper will examine Stefansson's long-running work on pemmican, focusing on the explorer's involvement with the Harvard University Fatigue Laboratory. In the early 1940's the Laboratory worked on contract for the U.S. War Department, researching ways to prepare soldiers for combat under polar conditions. Drawing on correspondence from the Fatigue Laboratory archives, this paper will trace how Stefansson provided his research on pemmican and his first-hand experience

with Indigenous Arctic diets to the Laboratory's physiologists. Acting on the explorer's advice, the Fatigue Laboratory organized trials of pemmican-based diets on soldiers; trials that failed spectacularly, though no one involved could definitively pinpoint why. Stefansson's collaboration with the Fatigue Laboratory is revealing as a study in the difficulties of translating experiential-based knowledge into standardized laboratory practice. However, this case study also speaks to larger issues of knowledge transfer between Indigenous cultures and Western science, especially who, historically, Western scientists have understood to be a credible expert.

Northern Depictions: Military Science and Inuit Knowledge on Film in Cold War Canada
Matthew Wiseman University of Waterloo

Between 1948 and 1954, the National Film Board of Canada produced and released two civil service training films about the "friendly Arctic": Going North (1948) and Vigil in the North (1954). Designed to educate civil servants about the geography, climate, and winter living conditions of the Canadian Arctic, the two-part film series depicted the importance of "modern" science and engineering to military service and social development in the northern regions of the country. As historical artifacts, the films provide insight into southern perceptions of coldweather science, northern expansion, civil-state relations, and Indigenous knowledge in Canada during the early Cold War. Furthermore, historical films shot in northern Canada are valuable for understanding the interplay between Western scientific epistemology and local knowledge systems, and these two films are particularly useful for assessing Indigenous-newcomer relations in an important and non-static region of the country.

Using previously classified records from Library and Archives Canada, this paper examines both films as visual representations of "modern" science in early postwar Canada. It argues that exploitation and appropriation of Indigenous knowledge was central to the high modernist goals and military preparations of the Canadian defence establishment, and that "Western science" acted as a cultural marker for southern visitors to the North. In so doing, this paper situates Inuit knowledge within the context of broader federal policies and shifting postwar attitudes towards the ethical treatment of Indigenous peoples in northern Canada during a significant period in world affairs.

SESSION – TEACHING HPS THROUGH EXPERIENCES

Session organizers: Isaac Record, Michigan State University
Chair: Molly Kao, University of Montreal

Reimagining First-Year Writing for STEM Students as Inquiry-Based Learning in Science Studies
Marisa Brandt Michigan State University, Lyman Briggs College

How can a first-year writing course help to create 21st century STEM students with foundations for interdisciplinary inquiry? Could such as curriculum engage STEM students in knowledge production in ways that help to acculturate them as collaborative, ethical, and empathetic learners? Bringing together insights from writing pedagogy, Critical Cultural Communication studies, and Science Studies, the author is leading an effort to rethink the first-year writing course required of all students at a residential college for STEM students at a large midwestern university. A major goal of the curriculum redesign is to develop science studies-inspired writing assignments that foster reflective experiential learning that demonstrate the value of science studies inspired inquiry to STEM education. The talk describes how the curriculum team designed the first-year curriculum and shares data on lesson lessons learned by the both the instructors and our students.

"Make the Students Do the Work": Designing a New Space for Learning HPS
Isaac Record Michigan State University

In early 2018, I gained access to a windowless room in the basement of my building and I received $40,000 to spend on equipment to support student learning. Over the course of the spring semester, 60 students in my HPS classes used these resources to design a new learning space for our college. I'll share the story and share some lessons from this experience that can apply to smaller experiential learning exercises. In class, students learned from guest speakers about ethnography, sociology, classroom design, makerspace culture, and many other topics. They immediately put these skills to use: they interviewed fellow students, faculty, and staff; completed site surveys of dozens of sites of learning ranging from coffee shops and lounges to makerspaces and classrooms; and read literature on experiential learning and classroom design, all in pursuit of the background they would need to create the new learning space. New interests emerged for them during this process, including the links between learning and failure and learning and games, and we had to work to incorporate these new ideas into the plans for the space and for the course. Having the college itself as an authentic "client" for the major project of the

class worked well as a practical motivator: students took the project seriously and learned a lot. This was a life-changing experience for me and for my students. Managing a design project of this scale with 60 students was certainly a challenge for me, and there were some bumps along the way, but I am proud of the outcome: a student-driven learning space.

Teach with "GORP" for Better Experiential Learning in HPS

Eleanor Louson Michigan State University

Experiential learning opportunities can improve student engagement and reflection in HPS courses. As a learning experience designer at a large midwestern university, I facilitated prototype experiential, interdisciplinary, problem-based courses. We designed these courses based on Heinrich et al.'s GORP framework, which leverages Gravity, Ownership, Relationships/Roles, and Place as elements promoting transformative, student-driven learning and engagement. At the same time, as an instructor in the History, Philosophy, and Sociology of Science, my own students' engagement in classroom experiences was enhanced when I began applying the GORP framework at the assignment level. In this talk I describe a series of three team-based experiential HPS assignments from my 1st-year undergraduate course. I introduce the Black Box project, Lab Study site visit, and Scientific Controversy stakeholder analysis assignments, and show how each of them benefit from GORP. I also describe strategies for prompting students' written reflections on these experiences and for giving students the tools to manage their own teams.