Philosophy of Science in the Forest Conference

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Book of Abstracts

Keynotes

Leah Henderson (University of Groningen)
Justification by probabilities: what more do we need?

Originally the distinction between context of justification and context of discovery was made to demarcate the proper domain of epistemology as a normative enterprise. The claim was, as Reichenbach put it, that 'epistemology is only occupied in constructing the context of justification', and the context of justification deals with the question of how well justified theories are by the evidence. Assessment of how probable theories are in the light of the evidence has become a prominent way to address this question. However, over time, many different concerns have been raised about what this probabilistic model of scientific justification might leave out. Some of these concerns are well-grounded, whilst others are not. In this talk I will discuss how far probabilities can take us as an account of justification, and what might be still missing.

David Ludwig (Wageningen University)
P.S. I love you. On Criticising and Loving Science in the Face of Global Crises

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In the face of global crises from climate change to rising food insecurity to the Covid-19 pandemic, a broad intellectual alliance emphasises the need to challenge anti-science populism. Historians, philosophers, and sociologists of science have been at the forefront of defending scientific expertise and trust in science. Harry Collins has recently even argued that we need to love science in order to avert social and environmental disaster. Unfortunately, this movement underestimates difficulties of loving science not only as an epistemic but also as a social practice. The current state of the science system is deeply unlovable - scientific research is indispensable for addressing global crises while often also a key actor in producing these crises and enabling the exploitation of people and planet. I argue that this situation challenges philosophers of science to expand their reflections on the relation between science and society. Tinkering with science policy or the roles of values in science is not enough. As philosophers of science, we need a bolder program of imagining a science that is deserving of our love.

Contributed papers

Sander Beckers (University of Tübingen)
Causal explanations and XAI

Although standard Machine Learning models are optimized for making predictions about observations, more and more they are used for making predictions about the results of actions. An important goal of Explainable Artificial Intelligence (XAI) is to compensate for this mismatch by offering explanations about the predictions of an ML-model which ensure that they are reliably action-guiding. As action-guiding explanations are causal explanations, the literature on this topic is starting to embrace insights from the literature on causal models. Here I take a step further down this path by formally defining the causal notions of sufficient explanations and counterfactual ex- planations. I show how these notions relate to (and improve upon) existing work, and motivate their adequacy by illustrating how different explanations are action-guiding under different circumstances. Moreover, this work is the first to offer a formal definition of actual causation that is founded entirely in action-guiding explanations. Although the definitions are motivated by a focus on XAI, the analysis of causal explanation and actual causation applies in general. I also touch upon the significance of this work for fairness in AI by showing how actual causation can be used to improve the idea of path-specific counterfactual fairness.
You can read the full paper at: https://arxiv.org/abs/2201.13169

Federico Boem and Y. J. Erden (University of Twente)
Gatekeeping in scientific publishing: a new epistemic gear in the practice of science

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Scientific publications are an essential component of contemporary science. Journals are the central platform where the scientific community (implicitly) negotiates and discusses its standards, what is accepted as evidence and what not, which activities characterise research as such, and which ones are to be considered spurious. Moreover, publications are the way scientific discoveries are perceived, accepted, and used as ‘facts’ on which new research can be based and further grow. A simplistic view seems to assume that a scientific study is published only after scrutiny that is itself deemed ‘objective’, fair, and more or less reliable. Methods for controlling and certifying the trustworthiness of scientific outcomes, including standardised and accountable protocols and peer-review systems, have been typically considered sufficiently reliable for ensuring rigour and other essential epistemic features for scientific enterprise (e.g. validity, reproducibility, etc.). Yet there is now substantial evidence (Tennant and Ross-Hellauer 2020) across various scientific communities (from psychology to nanobiology) that such processes have failed (cf. Smith 2006; Csiszar 2016; Stern and O'shea, 2019). Reasons range from a failure to replicate, to unintentional bias (in methodology or arising from confirmation or negative bias), all the way to fraudulent methods or reporting of results (Fanelli 2009; Cowley 2015). Given the self-governing nature of scientific enterprise (i.e. scientific outcomes and procedures are evaluated by 'peers' within the scientific community), research monitoring, scrutiny, and quality control fall to key 'gatekeepers' (Coser 1975; McGinty 1999; Hojat, Gonnella and Caelleigh 2003; Sato 2012; Newhouse and Brandeau 2021; Sparks 2014; Siler, Lee and Bero 2015; Primack et al. 2019) who straddle important, powerful positions along the research path – as reviewers, evaluators, policy writers, editors, and other intellectual stakeholders. In this work, we want to analyse some aspects of gatekeepers that we consider salient from a philosophy of science perspective. Accordingly, on the one hand, we intend to show how the gatekeepers' activities constitute a central part of the epistemic cultures (Knorr-Cetina 1999) within scientific communities. On the other hand, we will outline how these figures affect the methodological components of the different systems of practice (Chang 2012, 2014) regardless of the specific research area. We explore these topics in a number of ways, including by focussing on the notion of bias broadly construed, i.e. as more than just errors of reasoning, and instead encapsulating preferences, beliefs, expectations, cultural tendencies, and implicit epistemic attitudes, all of which contribute to the decisions of individuals concerning specific topics (Andersen et al., 2019; Erden 2021). In addition, we will examine the notion of reductionism, broadly understood. Reductionism constitutes a precise meta-theoretical assumption, a way of thinking (and doing) acquired by specific education, by belonging to certain research environments and by disciplinary cultures. Being aware of the scope of these assumptions, of their advantages and limitations, making them the subject of discussion and negotiation within the scientific community, could make the system of scientific publications more open, inclusive, plural and capable of incorporating new perspectives without giving up rigour, quite the contrary.

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Daniel Cserhalmi Friedman (Stanford University) and Dunja Šešelia (TU Eindhoven)

Scientific Disagreements, Fast Science and Higher-Order Evidence

Scientific disagreements are commonly considered an important catalyst of scientific progress (Solomon, 2006, Longino, 2002). But what happens if scientists disagree while society is depending on them for quick yet reliable results? In this paper we aim to provide a normative account for how scientists facing a disagreement in the context of 'fast science' should handle it, and how policy makers should evaluate it. Starting from an argumentative, pragma-dialectic account of scientific controversies (Donato Rodríguez and Bonilla, 2013), we argue for the importance of 'higher-order evidence' (HOE), which has largely been neglected in previous discussions on scientific disagreements and controversies.

In contrast to first-order evidence (FOE), which is provided in support of, or against the truth of a proposition, HOE is, roughly, evidence about evidence, i.e. evidence for or against the truth of a proposition about the first-order evidence (Dorst, 2020, Whiting, 2020). For instance, while an experimental result is a kind of FOE for a certain scientific hypothesis, evidence showing that some of my peers disagree with me on this result (on its accuracy, significance, interpretation, etc.) serves as HOE, which may undermine the force I took our FOE to have and therefore decrease my confidence in the given hypothesis.

Following Rodriguez & Zamora Bonilla's view of scientific controversies in terms of a 'game of giving and asking for reasons', we show how HOE plays an essential role in the specification of argumentation rules that underlie scientific disagreements and their resolution. We illustrate our point with a recent disagreement on the aerosol transmission of COVID-19 virus (e.g. Jayaweera et al., 2020, Lewis, 2020) and provide guidelines for how legitimate HOE in this and similar cases is acquired.

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Nina De Boer (Radboud University Nijmegen)

Making sense of the intelligibility of network models of psychopathology

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In recent years, there has been a trend toward conceptualizing mental disorders as complex systems (Borsboom, 2017; Borsboom et al., 2019; de Haan, 2020; Olthof et al., 2021). In other words, it is argued that mental disorders are constituted by many different (biopsychosocial) factors that interact in a non-linear fashion (involving feedback loops) across different temporal and spatial scales. This trend has been accompanied by an increased use of complexity science to study psychopathology. Complexity science spans a range of methodologies that exploit and represent the complexity of their phenomenon of interest. One complexity science method that has received much attention in psychopathology research is network analysis. Network analysis comprises various (statistical) techniques that allow researchers to estimate and represent networks – i.e., nodes (variables) that are connected via edges – based on available datasets. Some have argued that network analysis has not embraced the complexity of mental disorders to its fullest extent. For instance, statistical techniques are being developed that allow network models to incorporate data coming from multiple sources and domains (e.g., (neuro)biological data, psychological data, social data, et cetera) (Bianconi, 2018; Tio et al., 2020). It is assumed that using such multisource models in psychopathology research may have epistemic benefits: they may provide us with a better understanding and/or explanation of (the dynamics of) mental disorders (Blanken et al., 2021; Braun et al., 2018; Brooks et al., 2020; de Boer et al., 2021). However, is this assumption justified?

In this presentation, I will examine how attempts to make network models of mental disorders more 'complex' will affect their ability to provide scientific understanding, i.e., understanding why something is the case based on a scientific explanation. I will explore the idea that networks models of mental disorders that do more justice to the theoretical principles of complexity may provide less scientific understanding and therefore, may have less explanatory power. First, I will argue – in line with De Regt's account of scientific understanding (de Regt, 2017) – that network models can only explain features of mental disorders if they are intelligible to the researchers using them. Afterwards, I will examine in more detail what features of a network model may influence its intelligibility, using the criteria put forward by (Chirimuuta, 2020). Compared to 'traditional' network models, network models that do more justice to the multidimensionality of psychopathology will be more metaphysically intelligible. This is because these models will be more in line with some of the principles of complexity theory and hence be more theoretically articulated. However, I will argue that such models will also be less scientifically intelligible. This is because incorporating more types of data and dimensions into a network model will introduce more kludges, thereby decreasing the model's functional transparency (Creel, 2020). I will argue that this trade-off between metaphysical- and scientific intelligibility is likely to persist.

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Christian De Weerd (University of Groningen)

Questioning the Dynamics of Reason

In "Dynamics of Reason" Michael Friedman attempts to supplement Thomas Kuhn's Structure of Scientific Revolutions in an effort to dodge the notorious charge of conceptual relativism Kuhn is known for. In neo-Kantian fashion Friedman stresses the importance of relativized a priori principles in describing the dynamics of science by claiming that paradigms consist of a priori constitutive principles. A rational scientific revolution can occur, then, because successive paradigms have more general or adequate constitutive principles, something Friedman calls inter-paradigm convergence. To instantiate a rational paradigm shift based on inter-paradigm convergence, Friedman argues that practitioners across different paradigms must agree on the conceptual space of possibilities, called inter-paradigm convergence, a term borrowed from Habermas. In essence, practitioners of the former paradigm can agree that the successive paradigm is indeed a feasible alternative. By appealing to communicative rationality, Friedman hopes to dodge the notorious charge of conceptual relativism. I argue, however, that Friedman's framework is currently incomplete by introducing a distinction between symbolic and substantial semantic preservation. This distinction shows how inter-paradigm convergence and the revisable constitutive principles (the relativized a priori) are incompatible with one another. I claim that maintaining the relativized a priori only allows for symbolic preservation of the former paradigm in the successive paradigm which is insufficient for inter-paradigm convergence. Consequently, Friedman must drop either inter-paradigm convergence or the relativized a priori to make his framework coherent again, but I point out that both solutions are undesirable. I also highlight the consequences of the distinction. Specifically, I show how Friedman's appeal to communicative rationality on its own establishes surprisingly little and ultimately falls short of rationally motivating a paradigm shift. All that communicative rationality establishes is that the former paradigm can regard the successive paradigm as a live option, but there is no rational motivation to cross this bridge. What is needed, to rationally motivate an actual paradigm shift is inter-paradigm communicative rationality coupled with inter-paradigm convergence. But the distinction between symbolic and substantial semantic preservation shows that inter-paradigm convergence is not possible. Hence, Friedman's appeal to communicative rationality ultimately does not dodge the threat of conceptual relativism, as there is nothing in Friedman's framework that explains the rationale for actually shifting paradigms. Lastly, I claim that symbolic preservation and communicative rationality do, albeit irrationally, motivate paradigm shifts in another way. Precisely because terms and principles of the former paradigm are symbolically preserved in the successive paradigm, are practitioners of the former paradigm under the false impression and illusion that the successive paradigm in fact is a continuation and improvement of theirs. This illusion might convince practitioners of the former paradigm to shift, but there is nothing about this that constitutes a rational motivation. Quite the contrary, paradigm shifts under Friedman's framework are as much a conversion experience as he accused them to be for Kuhn. Hence, for "Dynamics of Reason" to be successful, and to avoid the consequences I highlight, further conceptual work is needed to deal with the distinction between symbolic and substantial semantic preservation.

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Markus Eronen (Univeristy of Groningen)

Theory and measurement in psychology

In recent years, more and more authors have called attention to the fact that the theoretical foundations of psychology are shaky (e.g., Eronen & Bringmann, 2021; Fiedler, 2017; Smaldino, 2019). This has led to a lively debate on the "theory crisis" in psychology, which is argued to be more fundamental than the replication crisis that has received much more attention (Muthukrishna & Henrich, 2019). In this talk, I first consider why there are so few good theories in psychology, and why psychology differs in this respect from other fields, and then argue that the lack of good psychological theories also creates fundamental challenges to psychological measurement.

First, there has been insufficient attention on the conceptual clarity of psychological constructs. The same construct is often operationalized in wildly divergent ways in different fields, or different constructs are created for the same underlying phenomenon. For example, there are over 30 different constructs related to "perceived control". The result is that psychology is permeated with numerous constructs and concepts of insufficient clarity, which is a problem for theory construction, as concepts are the building blocks of theories. Moreover, this lack of conceptual clarity is also closely linked to problems of psychological measurement: It is hard to provide valid measurements of constructs that are not well defined, as the discussion on the measurement of happiness and well-being illustrates (Alexandrova, 2017). Strikingly, most studies in psychology report little or no validity evidence whatsoever for the constructs used (Flake et al., 2017).

Second, psychological states are difficult to directly intervene on, and effects of interventions are hard to reliably track, which poses great challenges for establishing psychological causes or mechanisms. More specifically, interventions on psychological variables such as affects states or symptoms are not "surgical" but "fat-handed" in the sense that they change several variables at once (Eronen, 2020). This makes it extremely difficult to infer causal relationships between psychological variables, and insofar as theories should track causal relationships, this hinders the development of good psychological theories. In addition, it is widely thought that valid measurement requires establishing a causal relationship between the attribute that is measured (e.g., temperature) and the measurement outcome (e.g., thermometer readings; Trout, 1998). Insofar as this is the case, the problem of psychological interventions is also directly a problem for psychological measurement.

In light of these issues, it is understandable that psychological theories tend to come and go, without much cumulative progress (Meehl, 1978, 1990), and that the very possibility of psychological measurement continues to be debated (e.g., Trendler, 2019). However, I will end the talk on a positive note, considering some ways of making progress in psychology: Focusing more on conceptual clarification instead of just statistics and experiments; and embracing a holistic and pragmatic approach, where measurement, theorizing, and conceptual clarification are seen as necessary parts of an ongoing iterative cycle.

Yingying Han (Institute for Science in Society, Radboud University)

Multiple historic trajectories generate multiplicity in the concept of validity

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Validity, generally defined as "the quality of being well-grounded, sound, or correct" (Merriam-Webster, n.d.), has been widely acknowledged to be essential in scientific research. However, the technical criteria that constitute such quality vary, resulting in multiplicity in the concept of validity and proliferating taxonomies of validity. For instance, Newton and Shaw (2014) listed 151 'kinds' or 'types' of validity within educational and psychological measurements and they pointed out that as a result, "it is very easy to become very confused when trying to get to grips with the concept" (p8). As an attempt to clarify the concept and taxonomies of validity, I put them back into the theoretical, practical, and historical contexts by providing an overview of various validity concepts and their development in behavioral sciences.

Behavioral sciences, such as psychology, economics, and animal research for psychiatric disorders are of particular interest. On the one hand, these research areas feature diverse research practices including measurement, experimentation, and modeling, each serving different theoretical or practical purposes in the historical contexts. On the other hand, discussions on validity theories and the application of validation procedures are abundant and highly valued in these fields. The combination of these two factors allows us to study how the concept of validity has been shaped by the research practices and purposes in different historical contexts.

Following a brief introduction (section 1), the paper outlines influential validity theories and discussions in three different practices in behavioral sciences, namely measurement and testing in psychometrics (section 2), experimentation in experimental psychology, and experimental and behavioral economics (section 3), and animal modeling in biomedical research (section 4). It then concludes with a discussion (section 5) highlighting two important aspects: validity 'of' what practices and validity 'for' what purpose, to connect the validity concepts to the rich contexts that gave rise to their specific meaning and relevance.

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Gabriel Heinrichs (University of Groningen and Avans University of Applied Sciences)

Defensible unreliability - designing aggregation and deliberation procedures for peer review panels

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Peer review is the paradigmatic evaluation model in academia, permeating many aspects of academic life (Forsberg et al., 2022). Peer review has recently enjoyed increasing attention from philosophers of science, predominantly in the context of journal and grant reviews (Heesen & Bright, 2021; Lee et al., 2013). In this presentation, I would like to shift attention to another type of peer review: accreditation reviews of higher education programmes. Similar to peer panels conducting grant reviews, programme accreditation reviews are undertaken by ad-hoc, heterogeneous expert panels that must reach a collective judgment about a programme's quality, according to predetermined criteria. As a case study, I will look into the Dutch accreditation framework, from the perspective of judgment aggregation theory (Arvan et al., in press; List, 2011). My main argument is that accreditation review panels have considerable freedom to come relatively unreliable judgments, but may have convincing non-epistemic reasons to do so.

My presentation consists of three parts. Firstly, I will give an introduction to the Dutch accreditation framework, which is administered by the Accreditation Organisation of the Netherlands and Flanders (Nederlands-Vlaamse Accreditatieorganisatie [NVAO], 2018). The framework aims to ensure the formation of consistent and reliable judgments on educational quality. To that end, it stipulates a number of general principles, quality criteria and decision rules, and recommends specific procedures to guide the judgment formation (NVAO, 2020). Importantly, accreditation review panels have considerable leeway to interpret these procedures, in accordance with the idea that peer review should involve considerable (academic) freedom for panels to determine their approach. This autonomy may significantly decrease the reliability and consistency of their judgments, depending on the judgment aggregation and deliberation procedures that the panel adopts.

Within the framework's rules, as I will show secondly, the optimal aggregation procedure for panels is premise-based (List, 2005), and weighted in accordance with the respective expertises of the four or five panel members (Pivato, 2017). For optimal collective reliability on its conclusions, the panel needs to match this aggregation procedure with a suitable social deliberation procedure (Siebe, 2022). As the framework is much less explicit about this aspect, I will assess the expected epistemic performance of several possible deliberation procedures. Variations between procedures pertain mainly to the extent and way of sharing of evidence and arguments. I will also consider a variation that reduces social deliberation between panellists to almost none: panellists vote anonymously and deliberate individually.

Thirdly, I will compare the epistemically optimal procedure to an approach that is common practice among Dutch accreditation review panels. In this approach, panels address disagreements by alternating and amalgamating premise-based and conclusionbased aggregation and deliberation. Although epistemically suboptimal, I will show that this relatively unreliable approach may be defensible in light of other principles that are set down in the accreditation framework, especially diversity, trustworthiness, and autonomy. I will conclude my presentation by considering whether this balance between reliability and other principles might also hold for other types of peer review.

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Thijs Hemme (Utrecht University)

A Physics of Inference? The Free Energy Principle & Naturalistic Epistemology

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Within neuroscience a fairly ambitious idea has emerged in recent years under the name of the 'free energy principle' (FEP), as put forth by the neuroscientist Karl Friston. The FEP is an outgrowth of and a mathematically principled way of formulating ideas in neuroscience concerning the 'Bayesian brain' and 'predictive processing'. The FEP has in recent years attracted its fair share of supporters and critics, and in the literature considerable confusion exists concerning its meaning, applicability, and merits. Friston has characterized the FEP as a principled account of 'sentient behavior' grounded in physics. He and others consider the FEP to provide something of a 'physics of life and mind', and more specifically the FEP is claimed to be a variational principle of least action for livings systems. These are still speculative ideas that are contested, but if the FEP and its purported consequences are indeed legitimate, then it might hold great potential for unifying ideas in physics, biology, and cognitive science, as well as providing something of a physical and biological foundation for a naturalistic epistemology.

In this talk, I will first discuss some of the general outlines of the FEP and whether it can be seen to provide a 'physics of inference', and in what manner. In essence, the FEP states that systems with a Markov blanket striving towards a non-equilibrium steady state with their environment minimize a variational free energy function of their defining states. The mathematics of this process can be restated in information-theoretic terms as approximate Bayesian inference with respect to environment. If living systems can be modeled as Markovblanketed systems, this may provide a physics of living systems that behave 'as if' they perform inferences, thereby suggesting a continuity between life and cognition.

Second, I will discuss what role this 'physics of inference' might play in a naturalistic theory of epistemology, or whether it may even allow us to naturalize 'the scientific method', as was recently proposed. I will conclude that the FEP does not really allow us to do that, but it can—if it is correct in the claims Friston and others make about it—play a foundational, albeit minimal, role in a naturalistic epistemology by providing a physical and biological basis for inductive and self-correcting processes. That onto itself does not tell us much about epistemic norms or about how science or 'the scientific method' is naturalistically possible or how it arises naturalistically, as that would require applications to much more complex cases that can utilize the basic process proposed by the FEP to form such things as cumulative cultural constructions and normative reasoning practices.

The FEP—if correct in its ambitious claims—may thus provide us with a physical and biological foundation for a naturalistic theory of epistemology. This foundation is however rather minimal, and putting the FEP to epistemological and philosophical use would require further work.

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Amir Horowitz (The Open University of Israel)

Intentional irrealism and selective scientific realism

Many philosophers believe that the predictive and explanatory success of attributing intentional states to people justifies the claim that we have intentional states. Though limited in force, those intentional explanations and predictions that we daily employ with respect to our fellow subjects enjoy remarkable systematic success, and this success, it is argued, can only (or best) be explained by the assumption that behavior is caused by the ascribed intentional mental states. So (most plausibly), this assumption is true, and a fortiori, we have mental states with intentional contents. The aim of this talk is to undermine this argument.

I grant the assumption that explanations and predictions of behavior expressed in intentional terms systematically succeed in predicting and explaining behavior. The move from this success to intentional realism relies on a substantial assumption, namely that explanations and predictions of a certain kind that are systematically successful are (most plausibly) true. Supporters of scientific realism (e.g., Hilary Putnam and Richard Boyd) defend this claim. But there are objections to it. According to one of them, on which I focus, we need consider true only those ('active') parts of successful theories that play an explanatory role. Selective scientific realism is thus defended (see, e.g., Philip Kitcher and Stathis Psillos).

Intentional predictions and explanations allow a distinction between semantic aspects and other aspects that are active – appealing to which may predict or explain behavior. Thus, the rationale of selective scientific realism applies to them. This is so for, first, by ascribing contents we also ascribe logico-syntactic structures, and second, logico-syntactic structures are apt for the job of rationalizing behavior. They, in themselves, can sustain not only theoretical reasoning but also practical reasoning, that is, underlie explanations and predictions of behavior, and thus be active in the sense in question. I will argue for these two claims. If they are true, then the argument from the success of content ascriptions to their truth fails.

For this objection to the success argument to be effective, the individuation of the logico-syntactic structures in question must be handled. First, the components of those structures must be constants (so that Pa and Qb are different) rather than variables (constants need not of course be understood as 'standing for' anything), and in this sense are thick, but we should specify what it is that remains constant, that is, what the individuation criterion of these constants and of the logico-sytactic structures as wholes are. As a first approximation, we may say that the logico-syntactic structures in question are the thin structures (those in which Pa and Qb are the same) further individuated by the physical realizations (or the phenomenology) of the structures' components. However, such an individuation must be understood as system relative, for the same content ascriptions are ascribed to, and can succeed in predicting and explaining behavior of, subjects whose mental states share thin logico-syntactic structures but differ physically (and phenomenology). The case of subjects who think in different languages is one example of this phenomenon. The system relativity in question is an instance of a consistency constraint: an ascription of a cognitive process (such as an inference) enjoys predictive or explanatory success if the relevant subject instantiates the ascribed thin logico-syntactic structures, and those features of these structures' components to which cognitive processes are sensitive are consistent vis à vis the ascriptions. Content ascriptions that respect this consistency constraint can predict and explain cognitive processes and action even if mental states have no contents. We may wonder what is common to those different systems – e.g., subjects whose mental states share thin logico-syntactic structures but differ phenomenology (and physically), as is the case with subjects who think in different languages – the behavior of which can be successfully predicted and explained by the employment of the same content ascriptions. Though undermining the success argument does not depend on answering this question, I will suggest an answer to it that is consistent with intentional irrealism.

Kaush Kalindindi and Víctor Betriu (University of Twente)

Scopes of Choice: Scientific Perspectivism and the Problem of Theory Choice

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This paper identifies the shortcomings of Giere's (2006) scientific perspectivism regarding the problem of theory choice and proposes a framework to address them by acknowledging the limits of human subjectivity. Giere's perspectivism calls into question science's distinctness from other ways of grasping the world and shatters the hierarchization of theories, allowing for a pluralistic and a non-linear understanding of knowledge. This introduces a certain relativism that makes it implausible for a scientist to choose one perspective over others given that every perspective has the same value. How then do we explain the ways in which scientists choose among theories? We argue that Giere (2004) assumes that scientists make use of all possible theoretical perspectives when assessing which one to build models from. Moreover, his employment of the satisficing strategy (Giere, 1985) to explain theory choice is insufficient because scientists' subjective experiences shape and limit the scope of perspectives available to them in various degrees. Accordingly, there is a need for a framework that explains how certain factors govern and influence which theoretical perspectives are accessible and are taken into consideration when determining which one to choose.

Drawing from Deleuze and Guattari's (1987) concept of "plane of immanence," we propose the framework, scopes of choice, to describe the selective accessibility of theoretical perspectives for scientists on a subjective level. The encapsulation of the totality of human knowledge of the world and the flattening of hierarchies between its constituents makes the plane of immanence a surface with multiple points of entry and one that is devoid of centers and temporalities. Upon this plane of immanence, we have conceptualized one's subjectivity as unfolding in two scopes of accessibility: the scope of awareness and the scope of satisficity. The former concerns the scientist's awareness of theoretical perspectives as shaped by material resources and traits of character. The latter concerns the set of theoretical perspectives from within the former scope of awareness that a scientist would presuppose to produce satisfactory models, influenced by her beliefs. Having laid out the framework of scopes of choice, we will employ its features to illustrate how scientists choose a theory to build models from. In particular, the framework reveals which collection of theories are taken into consideration when making this choice, and how this collection can expand to include other theoretical perspectives or contract to abandon some that are no longer relevant. Furthermore, the framework will be used to explain the formation of scientific communities and address the possibility of including non-western traditions and pre-modern worldviews into the scientific practice. Lastly, the paper highlights the implications of this framework in establishing new connections between analytic and continental philosophies of science.

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Aldo Kempen (Open University of The Netherlands)

Onto-epistemology, Poetics and Karen Barad: an Analysis of the Formal Developments of the Oeuvre of Karen Barad

In this paper, I analyse the formal developments of the oeuvre of Karen Barad — a physicist-cum-philosopher whose work is gaining prominence in a variety of fields under the header of 'new materialism'. While their philosophical output spans over three decades, the concepts and themes they work with remain remarkably similar. However, their writing style has changed fundamentally — evolving from a rather dry analytic tone borrowed from the sciences to much more experimental and poetic prose reminiscent of deconstructive philosophy. In this paper, I compare three papers published throughout these three decades and track the development of their style. In researching how their style evolves, this paper highlights how formal decisions are not accidental, but rather integral, to the praxis of the philosophy of science. Specifically, this article aims to draw attention to how certain forms and styles deal with the problematics of self-referentiality and it compares their affordances and trade-offs. Moreover, it invokes what Johan de Jong has coined 'the moment of showing' (XI) into the realm of the philosophy of science. Shifting away from what Adorno called Standpunktphilosophie (152), the analysis of Barad's poetics opens up to a praxis of philosophising that is qualitatively different to the taking of sides but involves a form of showing. Standing in a long tradition of a poetics of showing, ranging from Plato's aporia to Nietzsche's aphorisms, Barad can be seen to introduce this poetics to feminist concerns in the philosophy of science in order to engage with the self-referential problematics that haunt embodied and situated accounts of knowledge production. The comparison and tracking of the affordances of the different styles employed by Barad will result in an analysis of different styles that could be productively employed by philosophers working in different regions.

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Maarten Kleinhans (University of Utrecht) and Henk de Regt (Institute for Science in Society, Radboud University)

Materiality and material modeling in earth-scientific experiments

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Experimentation and modeling are two sides of the same coin. Both are practices that scientists use to gain epistemic access to phenomena in the world, and despite first appearances they are in fact closely connected. This is clearly visible in the earth sciences, where scale models are material representations of target systems such as rivers, deltas and mountain ranges. Material models can be manipulated in order to uncover causal relations within the system and thereby to gain explanatory understanding of it. Material scale models lie somewhere in between experimenting on a real-world system and manipulating an abstract model. But where precisely? In our paper we investigate the nature of material modeling in the earth sciences.

Morgan (2003) has advanced a typology of experiments ranging from 'ideal lab experiments' to 'mathematical model experiments', with special attention for the role of materiality. She argues that in between these two extremes hybrid experiments exist: 'virtually' and 'virtual' experiments. While this may clarify experimental practices in physics and chemistry, where employed materials often are the same as targeted in the world, we submit that her account does not fit earth-scientific experimentation, for two reasons. First, its targets involve much longer timescales and larger spatial scales than can possibly be implemented in real-world experiments. Second, the employed materials often differ from those relevant to the target phenomena, but are chosen because they enhance similarity of relevant dynamics of the target system. Scale experiments differ from ideal lab and virtual/virtually experiments in Morgan's sense, because the similarity is not in the material itself. This raises the questions on the basis of what considerations experimenters select materials, and how these relate to the target system. We review the literature of experimental tectonics, geomorphology and civil engineering, which operate on spatio-temporal scales of 1:1,000,000, 1:1,000 and 1:10 respectively. We find that material choices are made on the basis of (at least) three scaling considerations. The first is pragmatic: the material must be available, affordable and safe to use. The second and third are that experimental scaling can be done geometrically and dynamically. Which of these prevails depends on the explanatory target, the scale, and tradition in the various subfields of the earth sciences. In particular, civil engineering follows scaling rules to model systems with the geometry and dynamics of a specific location, whereas tectonics employs analog models for much larger time- and spatial scales wherein only a more select set of variables can be dynamically scaled. As a result, only a select set of processes and properties are represented in manipulable material models. Our analysis adds a dimension of materiality, perpendicular to Morgan's typology of experiments: similarity of materials in the target system to that in the model reduces with increasing scale of experiments, in order to maintain the dynamics that scientists deem most important in the target system.

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Jan Pieter Konsman (University of Bordeaux)

Homeostasis across, and feedback from, biological systems

Although physiology as a discipline may have been declared dead, the notion of homeostasis seems to have survived the death of the discipline in which it emerged. If historically homeostasis has been mostly associated with the nervous and endocrine systems, our hypothesis is that over time it has been more and more employed in immunology and microbiology as well as regarding phenomena at lower levels of organization. The aims of the present work were 1) to characterize the use of the notion of homeostasis in the life sciences over the past decades using bibliometric and contents analyses and 2) to discuss the extent of its scope and explanations in the life sciences. In an attempt to compare how homeostasis has been used in neuroscience, endocrinology, immunology and microbiology, key word searches on the biomedical database PubMed were performed. These were then used to constitute articles corpuses that allowed to answer the following questions: 1) What are the changes in co-occurrence over time between homeostasis on the one hand, and the nervous, endocrine and immune systems and microbiota, on the other hand?; 2) Does the association of homeostasis and an adjective (e.g. endocrine or immune) refer to a whole system or its components?; 3) Is homeostasis used as a descriptor or something to be explained?
The working hypothesis was corroborated by the finding that over the past decades homeostasis has become widely used across biological systems and was not limited to nervous and endocrine systems or their components. For example, the notion of "immune homeostasis" was more frequently encountered that that of "neuronal homeostasis". Moreover, for articles identified with the search terms "homeostasis" and "immune" there was an increase over time in the proportion of articles referring to cells. Homeostasis was considered to be used as a descriptor when it was stipulated that a condition of a biological system corresponded to homeostasis and a regulated phenomenon was postulated or when it was concluded that a condition of a biological system corresponded to homeostasis after having exposed it to perturbations and a regulated phenomenon was postulated. Instead, homeostasis was considered to be explained when some independent and decoupled regulatory processes involving models (thermostat, feedback), mechanisms or selective advantages were specified. It was found that between the periods 1994-2005 and 2017-2018 the descriptive use of homeostasis in review articles has become more frequent for homeostasis and "nervous, neural or neuronal" and for homeostasis and "immune". In addition, in review articles published in 2017-2018, the descriptive use of homeostasis was more frequent for "homeostasis" and "microb*" than for "homeostasis" and "nervous, neural or neuronal". However, in the review articles considered on "immune" and "homeostasis" and on "microb*" and "homeostasis" there was some discrepancy between the description and the explanation of homeostasis by authors of these articles and our judgement of those. This latter point will be discussed by comparing emergent network stability explanations and independent and decoupled regulatory or maintenance mechanisms-invoking explanations for subcellular homeostasis.

Theo Kuipers (University of Groningen)

Truthlikeness and the number of planets

In the literature on truthlikeness, the problem of expressing the truthlikeness of a hypothetical number of planets in our Solar System, or a hypothetical set of such numbers, relative to the truth (8) is a recurring source of inspiration and intuitions (Niiniluoto (1987, 2020), Oddie (1986, 2016), Garcia (2021)).
Garcia (2021, p. 14-5) lists e.g. the following examples:
1. The number of planets is ten versus ten billion.
2. The number of planets is between seven and ten inclusive versus between one and ten billion inclusive.
3. The number of planets is 7 versus either 1, less than 1 or greater than 1.
We will all agree that the first alternative is in all three cases closer to the truth or more truthlike than the second. However, there are several other cases where our intuitions may diverge.
The following alternatives (Garcia (2021, p. 30-1) raise the question whether truthlikeness of true hypotheses (always) increases with logical strength:
4. The number of planets is eight or twenty versus eight or thirteen or twenty.
Similarly,
5. The number of planets is two versus two or two thousand.
raises the question whether truthlikeness of false hypotheses (always) decreases with logical strength.
As far as I know, the planet example has not yet been studied as a typical example of the truthlikeness of hypotheses concerning quantities of one kind or another. Moreover, a clear two stage approach seems missing, that is, first solving the problem for singular hypotheses, i.e. postulating a certain number, and then the problem for disjunctive hypotheses. As a matter of fact, the examples 2 – 5 all deal at least partly with such hypotheses, but it will turn out to be worthwhile to look first more in detail to singular hypotheses.
Besides example 1, we may question whether
6.1) The number of planets in our Solar System is nine (instead of 8).
6.2) The number of municipalities in the Netherlands (in 2022) is 346 (instead of in fact 345).
are equally truthlike (relative to the relevant factual truth), while they differ just one from the true number. Consider also
7. The number of planets is six versus ten.
Are these singular hypotheses, both differing 2 from the true answer, equally far from the factual truth? Finally, take
8.1) The number of planets in our Solar System is 16
8.2) The number of municipalities in the Netherlands (in 2022) is 690 (instead of in fact 345).
Is the second singular hypothesis more or less truthlike than the first, or are they equally truthlike, where both double the true number?
In this paper in progress we will deal in Section 2 with the truthlikeness of singular quantity hypotheses, with reference to the examples 1, 6-8. We will propose two, strongly related, normalized metrics as, in view of the examples and proposed conditions of adequacy, much more appropriate than the standardly assumed normalized absolute difference ($|x-y|/(|x-y|+1)$). In a detour in Section 3 we will introduce (based on XYZ, manuscript, conditionally accepted) a general measure for the truthlikeness of general, disjunctive, hypotheses, assuming the truth is also of a general nature, as e.g. in the nomic case, viz.

dealing with what is nomically possible. Finally, in Section 3 we will apply this general measure to the special case of a disjunctive quantity hypothesis concerning the singular truth about the relevant quantity in the actual world. We will compare the results with those of the relevant measures of Niiniluoto and Oddie, notably in view of the partly disputed conditions of adequacy. In the presentation the emphasis has to be on Section 2, which is already very challenging.

Federico Laudisa (University of Trento)

When did Locality turn into 'Local Realism'? A Historical and Critical Analysis

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According to a serious reading of the Bell 1964 theorem, any theory that is supposed to extend quantum mechanics by incorporating its statistical predictions is non-local. The attempts to deflate the revolutionary impact of the theorem, however, started in fact very early and, with different tools and aims, still continue today. The more recent version of this deflationary attitude attempts to recast the issue of the meaning and implications of the Bell theorem in terms of a vague 'realism', a condition whose conjunction with locality would be the alleged target of the theorem. Thus what is supposed to be the focus of the latter, jointly with the other (obvious) assumption that quantum-mechanical predictions are to preserved, is summarized in the expression local realism. Under the assumption of local realism, therefore, and provided quantum mechanics' predictions are taken for granted, a die-hard view takes the Bell theorem to be a result that does not establish non-locality but rather the impossibility of any 'realistic' account of the quantum phenomena (see, for instance, Zukovski, Brukner 2014, Boughn 2017). However, it can be convincingly shown that, since the seminal Bell papers of the Sixties (Bell 1964, 1966), the crucial condition was only locality, with no need of any independent 'realism' condition (Laudisa 2008, 2012). Since a large portion of presentations of the Bell theorem still refers to 'local realism' as the main target of the theorem itself, a circumstance which leads to serious misreadings, I propose a historical and critical analysis of the stages through which, starting already from important papers such as Jauch and Piron 1963, up to Clauser, Horne, Shimony, Holt 1969 Wigner 1970, Clauser, Horne 1974 and Clauser, Shimony 1978, the locality condition almost undiscernibly turned into a 'local realism' condition. This reconstruction focuses on the special role played by a condition, defined in different terms through the years: according to this condition, the original Bell enterprise embodies a sort of 'classicality' requirement, such that the hidden variable theory – taken into account as a possible completion of quantum theory – is supposed to assume from the start pre-existing values for all meaningful physical quantities for the systems under scrutiny. Finally a recent claim by Hall, relevant to the above discussion, will be taken into account: according to Hall, the locality condition in the original Bell 1964 paper can be analyzed so as to show that it is too weak, by itself, to imply pre-determination of the spin properties in the EPR argument (Hall 2020). Since the derivation of determined spin properties from locality alone is a crucial step to show that the 'local-realistic' readings of the Bell theorem are misguided, the Hall claim deserves attention. I will argue that the Hall claim assumes the original locality condition to be decomposable essentially in terms of what have become known as parameter and outcome dependence conditions, whereas the original Bell framework does not allow to justify in a straightforward way that decomposition.

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Oliviere Lemeire (KU Leuven)

Kindhood semantics for scientific generics

Generic sentences are unquantified generalizations such as "Ravens are black." Surprisingly perhaps, these unquantified generalizations are ubiquitous not only in ordinary conversations but also in science (Claveau & Girard 2019; DeJesus et al. 2019). Here is a list of generic sentences from various scientific disciplines: (1) Photons move at a speed of 299792458 m/s in a vacuum. (2) Americans overestimate social class mobility. (3) Dogs are definitive hosts of Neospora caninum. (4) Octopuses punch fishes during collaborative interspecific hunting events. (5) Young children police group members at personal cost.

In this paper, I defend a semantic theory for scientific generics such as (1-5) and use this theory to argue that generic sentences are of special interest to philosophers of science, not just to linguists and philosophers of language. According to the semantic theory I defend, generics express kindhood generalizations, rather than probabilistic generalizations or normality generalizations as defended by others (Nickel 2014). I argue that a generic sentence of the form "Ks are F" says that the property F is part of what makes the category K into a kind. The generic "Ravens are black," for instance, says that blackness is one of the properties that makes ravens into a kind. On this semantic theory, the notion of kindhood is part of the content expressed by a generic sentence.

I defend this semantic theory by arguing that two features that are often attributed to scientific kinds in the philosophical literature can also explain two features of the meaning of scientific generics. Scientific kinds, it is often argued, (a) are metaphysically realized in different ways across the sciences and (b) exist only relative to the epistemic and practical concerns distinctive of a scientific discipline or research program. On the hypothesis that the notion of kindhood plays a central role in the semantics of scientific generics, these two features also explain why (a) the truth-conditions of generic sentences vary depending on the domain that is described and (b) why these truth-conditions depend on the contextually operative epistemic and practical concerns.

By explicating the notion of kindhood that plays a role in the semantics of generic sentences, I provide the following truth-conditions for (scientific) generics:

A generic of the form "Ks are F" is true in a context C iff F stands in dependency relation R to the defining properties of K, where R is that dependency relation based upon which K accommodates the epistemic interests and practices operative in C.

The upshot of this semantic theory is that scientific generics are of special interest to philosophers of science, despite their meaning variability as evidenced by (1-5). Due to the kindhood semantics of scientific generics, they inform philosophers about the categories that are considered kinds in a particular discipline and inform philosophers about the way in which scientists themselves reason about the type of dependency relation that is constitutive of kindhood in their domain.

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Bert Leuridan (University of Antwerp), Erik Weber (Ghent University) and Ann Wyverkens (Ghent University and University of Antwerp)

Evidence as a Three-placed relation – contributing to the Evidence-based Management programme

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The idea of evidence-based management (EBMgt) has been developed by Denise Rousseau in a series of articles (2006, 2018, 2020) and in the book Evidence-Based Management: How to Use Evidence to Make Better Organizational Decisions (2018, co-authored with Eric Barends).

EBMgt is needed, they claim, because too many organizational decisions fail because of "managers who rush to judgement, impose their preferred solutions, fail to confront the politics behind decisions, ignore uncertainty, downplay risks, and discourage search for alternatives." (Rousseau 2018, 136) The proposed remedy is to make management decisions "through the conscientious, explicit and judicious use of the best available evidence from multiple sources [...] to increase the likelihood of a favorable outcome." (Barends & Rousseau 2018, 2) To that effect, they offer practical recommendations which managers could implement in their companies.

They distinguish 'evidence' (defined as information supporting or contradicting hypothesis) from mere 'data' (numbers, words, figures, ...) and from mere 'information' (defined as data relating to something or someone and considered meaningful or useful) (Rousseau 2018, pp. 176-177).

In their view, four sources of evidence are relevant for management decisions: 'evidence from the scientific literature', 'evidence from the organization', 'evidence from practitioners' and 'evidence from stakeholders'.

Rousseau's and Barends' project is very useful and promising, but there is room for improvement, as their conception of evidence has a shortcoming: they characterize it as a two-place relation between information on the one hand and a hypothesis on the other, while evidence should be seen as a three-placed relation between a method, information and a hypothesis. Information can only support or contradict a claim, assumption or hypothesis if it was gathered using a method that minimizes bias.

I will proceed as follows:

1. Briefly introduce Rousseau's and Barends' EBMgt-project.
2. Zoom in on their data-information-evidence tripartite and on their four sources of evidence.
3. Show that evidence characterized as a two-place relation between information and a hypothesis is problematic and a three-placed characterization is needed (using one or two toy examples).
4. Briefly discuss parameter estimation in the social sciences to show that the basic insights from 3. also apply to scientific practice; and offer a three-placed characterization of evidence for parameter estimation.
5. Distill a generic, overall approach to evidence as a three-placed relation, dubbed the 'logico-procedural approach to evidence in science', which highlights both 1° the need for an adequate logical, mathematical or statistical relation between information and hypothesis and 2° the importance of using the right (bias minimizing) procedures for gathering information.
6. Review Barends and Rousseau's book Evidence-Based Management: How to Use Evidence to Make Better Organizational Decisions (2018) and show, for three of the four sources of evidence they distinguish (from the organization, from practitioners and from stakeholders), that several of their practical recommendations are in fact bad advice; offer improved recommendations instead; and offer a proper definition of each of these types of evidence, based on the logico-procedural approach.

In short, by offering a better, three-placed account of evidence we endeavour to contribute to the EBMgt project, making it more reliable (minimizing the risk of calling information 'evidence' when it is not) and efficient (avoiding the collection of information that has to be thrown out of the window afterwards).

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Simon Lohse (Radboud University)

Mapping Uncertainty in Precision Medicine

Precision medicine is a novel, data-driven approach in biomedicine "that takes into account individual differences in people's genes, environments, and lifestyles" (Sankar and Parker 2017) to develop treatments and preventive measures for diseases. This approach promises a more fine-grained understanding of the complexity of diseases and to develop innovative therapies that are precisely tailored to individual patient groups. To achieve this goal, it relies on the analysis of complex forms of evidence, using omics technologies as well as e-health data sets and sophisticated IT infrastructure. However, there are deep epistemic concerns about precision medicine's ability to deliver on its promise. Many important concerns revolve around what has been described as the paradox of precision medicine, namely that *uncertainty* seems to be one of its key characteristics, in particular uncertainty regarding its evidential basis (Kimmelman and Tannock 2018). This observation is paradoxical because it is in tension with the main rationale of precision medicine to *increase* certainty through a more exact understanding of diseases and individualised therapies.

In a much noted commentary, Hunter (2016) describes several aspects of uncertainty in precision medicine with an emphasis on uncertainty regarding testing procedures and the interpretation of ever more large, complex and probabilistic data sets in view of their therapeutical implications. Other sources for epistemic uncertainty that have been highlighted in the biomedical literature include unclear evidence thresholds for stratifying diseases into subtypes, opaque machine learning algorithms for predicting therapeutic outcome, and different ways to deal with genetic variants of unknown significance in clinical settings (e.g. Pollard et al. 2019). Uncertainty is also discussed as a feature of precision medicine in the philosophy of science and science studies literature. For example, the unclear

causal status of genetic markers and the unreliability of genomic testing have been cited as contributing to uncertainty in precision medicine (Kerr et al. 2019; Green at al. 2019).
In summary, uncertainty is highlighted as a structural feature of precision medicine in practice. While uncertainty is a typical feature of many new developments, in particular in biomedical research, technology development and medicine, in precision medicine, more specific questions of uncertainty seem to become relevant too. In particular, there seem to be systematic links between uncertainty in precision medicine and other much-noticed aspects of this field – such as its complexity, its reliance on big data technologies, and its aim to reorganise disease taxonomies – that are in need of further exploration. However, there neither exists a comprehensive overview of factors that may contribute to uncertainty in precision medicine nor a description of its main characteristics (or forms). This talk will take first steps to remedy this situation. Drawing on work in a large precision medicine consortium, I will provide a critical overview of sources of uncertainty in precision medicine on an ontological, epistemological/methodological and practical level, thereby generating a high-resolution picture of the uncertainty paradox. Based on this picture, I will discuss implications for the theoretical and practical assessment of translational research and therapy in the context of precision medicine.

Matteo Michelini (TU Eindhoven)

Epistemic Vigilance as a Driver of the Emergence of Truthful Communication: An Agent-based Modeling Approach

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The reliability of human communication constitutes one of the many puzzles of evolution. Although lying may seem advantageous for individuals, our communication is mostly honest and we effectively learn from other people's words (Harris et al., 2018; Scott-Phillips, 2008). Sperber et al. (2010) provide a solution for this puzzle, claiming that humans developed a suite of epistemic tools, which they call epistemic vigilance, that is responsible for keeping communication reliable. In particular, they describe epistemic vigilance as a filter preventing the listener from believing false information. Such a filter relies on four tools (Michaelson, 2018): a past record of the listener's experiences with the speaker, a social assessment of the speaker's reliability (obtained combining the individual experiences), a consistency check of the new information against the listener's previous beliefs and the ability of the listener to detect non-verbal cues that the speaker may produce. In Sperber et al.'s proposal, the combination of these tools is responsible to keep communication reliable. Yet, alongside the many applications of it (Heintz and Scott-Phillips, 2022; Mercier and Sperber, 2017), this idea sparked some critiques (Michaelian, 2013; Michaelson, 2018).
The aim of this paper is to examine Sperber et al.'s hypothesis by means of a computer simulation. To do so, we operationalize the concept of epistemic vigilance and we study how it influences the evolution of communication. We simulate agents who can either share true information, lie or ignore the others. Although sharing true information is beneficial for everybody, agents may lie (or refuse to communicate) to get to know other people's information without giving up on theirs. Following Sperber (2013), epistemic vigilance is formalized as a set of tools that helps agents to detect lies and punish liars through a reputation system.

In order to understand if epistemic vigilance is responsible (i.e. necessary) for reliable communication, we compare runs where agents have access to it and runs where they do not. Our contribution is twofold. In addi1 tion to testing Sperber et al.'s hypothesis, we propose the first computational model providing a how-possible explanation of how epistemic vigilance may have generated honest communication (Gr¨abner, 2018). This is a fundamental step to be able to integrate epistemic vigilance in evolutionary models of cooperation, which Giardini et al. (2021) consider necessary for our understanding of altruistic behaviour.

Broadly speaking, the epistemic vigilance hypothesis is corroborated by the results of our model, as in most of the runs resembling real conditions epistemic vigilance is necessary to obtain reliable communication. Yet, we find that this is not always the case: if agents are able to independently verify the information they receive often enough they may not need epistemic vigilance. In addition, our results also underline the conditions that make the combination of reputation and epistemic vigilance insufficient for the evolution of honest communication. For example, when the information agents learn independently from the others is too little honest communication is never established. Finally, the effectiveness of the different tools of epistemic vigilance is tested, and we find that the ability to detect behavioral cues plays no positive role and that the so-called consistency check is responsible for most of the results.

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Aydin Mohseni (University of Pittsburgh)

HARKing: From Misdiagnosis to Misperscription

In a 2019 article in Nature, the author, psychologist Dorothy Bishop, describes HARKing as one of "the four horsemen of the reproducibility apocalypse," along with publication bias, low statistical power, and p-hacking (Bishop, 2019, p. 435). The practice of HARKing---hypothesizing after results are known---is commonly maligned as undermining the reliability of scientific findings. There are several accounts in the literature as to why HARKing undermines the reliability of findings. Scholars have argued that HARKing undermines frequentist guarantees of long-run error control, (Rubin, 2017) that it violates a broadly Popperian picture of science, (Mayo, 2019) and misrepresents hypotheses formulated ex post to observing the data as those formulated ex ante (Kerr, 1998). I argue that none of these accounts correctly identify why HARKing can undermine the reliability of findings, and that the correct account is a Bayesian one. Further, I show how misdiagnosis of HARKing can lead to misprescription in the context of the replication crisis in the social and biomedical sciences.

I will show that HARKing can indeed decrease the reliability of scientific findings, but that there are conditions under which HARKing can actually increase the reliability of findings. In both cases, the effect of HARKing on the reliability of findings is determined by the difference of the prior odds of hypotheses characteristically selected ex ante and ex post to observing data. To make this precise, I employ a standard model of null hypothesis significance testing in which I provide necessary and sufficient conditions for HARKing to decrease the reliability of scientific findings.

Understanding HARKing is important on at least two counts. Historically, HARKing is closely tied to questions regarding the relationship between prediction and accommodation. These questions have engaged philosophers at least as early as Mill (1843), were made central in the philosophy of science by Popper (2005) and continue to be of concern in contemporary discussions in scientific epistemology (cf. Hitchcock & Sober (2004), Douglas & Magnus (2013), and Worrall (2014)). HARKing is also imputed to be among the questionable research practices contributing to the crisis of replication in the social and biomedical sciences. A better understanding of HARKing sheds light on both these issues.

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F. A. Muller (Erasmus University and Utrecht University)

What is science?

The demarcation problem, as Popper called it, has fallen on hard times. Popper tried to demarcate scientific from non-scientific propositions. But science is so much more than propositions. Of course Popper also had definite views on the-so-much-more, which is hereby acknowledged.
I argue that the "things" to which the predicate `science' applies are praxes. After characterising what a praxis is, I go on to characterise what a scientific praxis is. Science is then taken to be the plurality of all and only scientific praxes. This is not yet the desired Carnapian explication of the concept of science, meeting Carnap's conditions. But before admit defeat and throwing my hands in the air shouting "Family Resemblance!", I try not to give up yet on finding a Carnapian explication. I then argue that fundamental philosophical principles will influence the explication of science inevitably, which will make an explication of science that will happily be embraced by every philosopher and scientist alike as unfeasible as one for, say, the concepts of truth and politics. Consolation is that this the generic case in philosophy and the problem of demarcating science from non-science (e.g. philosophy, religion, pseudo-science) is no exception.
Yet the way paved here to think about how to demarcate science is novel, I submit, and thereby provides a novel way to think about what science is. Hail to the Demarcation Problem!

Vincent C. Müller (TU Eindhoven and University of Leeds)

Digital Philosophy: A Methodological Proposal

Philosophy is methodologically in the air after the silent death of analytic philosophy and ordinary language philosophy – but we still do “conceptual analysis”, mostly separate from empirical insight. We also pretend that there is such a thing as ‘merely applying’ philosophy to other areas such as technology or science.
There is a way out: We can do classical conceptual analysis in a methodologically sound way, without the need for a new philosophical ‘foundation’; while reviving a proper way of ‘applied’ philosophy. We can do this by identifying conceptual problems and trying out solutions in digital technology, moving away from the ‘human case’: Call it “digital philosophy”. I will introduce the concept, then sketch some examples from the more distant or more recent past, and finally provide an outlook for the use of this method.

Ittay Nissan-Rozen and Amir Liron (The Hebrew University of Jerusalem)
Triangulation, Incommensurability, and Conditionalization

According to Methodological Triangulation there is an epistemic value in using several methods to establish the same scientific claim. Hessen et al. (2016) presented three formal results that can be viewed as expressing two different senses for the term “epistemic value” in the above characterization. The first sense is that of independent reliability: when several reliable methods support the same claim independently, the overall (objective) probability of the claim to be true increases with the number of methods. The second sense is that of reliability likelihood: when at least two methods, with unknown levels of reliability, agree, the probability that some of these methods are reliable increases with the number of methods.
After discussing some limitations of these two senses, we suggest a third one that applies to cases in which different methods rely on partly incommensurable theories, that - nevertheless – give predictions regarding the same phenomena. We model such cases using different probability distributions that are defined over partly overlapping algebras. We assume that although all theories assign a probability value to some hypothesis, H, none of the theories assign a probability value to the claim that one of the other theories predicts H with a given probability. We argue that this nicely captures the situation in a wide range of interdisciplinary scientific discussions. We also argue that at least in many such cases, it is natural to demand that after learning that a method, associated with a given theory, supports H to a given degree, a rational agent should not change her conditional credence in the reliability of other methods, that are associated with other theories, given H. We show that this last assumption amounts to a commitment to using Jeffrey’s conditionalization as an updating method and that when all methods are reliable, rational credence in H increases with the number of methods used. We also provide a general formula for computing this credence and show that the order of using different methods does not matter.
We then demonstrate how our account can be applied to the case of using different methods to gain knowledge about causal mechanisms. We do that by exploring the example of the research regarding gendered pathways to women's incarceration. The relevant scientific literature identifies three different pathways to women's incarceration. However, some of the measures (e.g., substance use) used in the

research regarding each one of these pathways are endogenous to the pathways. Thus, it is not always possible to assess the exogenous effect of one pathway given another one. We argue that this should be understood as a type of partial incommensurability between the theories that describe each one of the pathways and so our account can capture this case. Specifically, we argue that even though no statistical evidence regarding the effect of one pathway given another is available, the fact that a given woman follow more than one pathway should increase one's confidence in her future incarceration. We point to some implications to rehabilitation policy.

Hans Radder (VU Amsterdam)

Empiricism must, but cannot, presuppose real causation

The philosophical and scientific significance of the notion of causation has been frequently questioned, primarily by philosophers in the empiricist tradition. The basic assumptions of empiricism are that sensory experience constitutes the foundation of all knowledge and that belief in the reality of unobservable entities, properties, events or processes cannot be epistemologically justified. Claims about unobservables may have some pragmatic function in human discourse or human affairs, but they tell us nothing about the real world.

A typical feature of empiricism is the belief that the notion of the empirical is philosophically unproblematic and can therefore be used as the basis for ordinary and scientific knowledge claims. This explains why empiricists do not pay much attention to the study of empirical practices in science or ordinary life (Radder 2006, chap. 2). Fortunately, many other philosophers have provided in-depth and sustained studies of such practices. During the last four decades, many detailed studies of scientific experimentation and observation have been made available (see, e.g., Hacking 1983; Kosso 1989; Galison 1997; Janich 1998). Similarly, many accounts of ordinary empirical practices have been provided through a variety of specific studies of embodied and embedded cognitive processes (see, e.g., O'Regan and Noë 2001; Anderson 2003). What these studies have demonstrated is that empirical knowledge, both in science and in ordinary life, is the result of complex practices. For this reason, such knowledge cannot be conceived as philosophically unproblematic or neutral. The central insight is that obtaining experience may fail as a matter of course and not merely in the case of individual illusions or hallucinations. With respect to science the point can also be phrased somewhat differently, as follows. Experimental and observational evidence is not 'given' but has to be correctly realized. This entails that epistemology needs to address the question of the justification of evidence. The epistemology of science cannot be limited to the issue of how 'given' evidence justifies theoretical claims.

The focus of the article is on scientific experience (but an extension of its conclusion to ordinary experience seems plausible). I do not presuppose the specific views of the authors cited in the preceding paragraph. I merely use their general point that in-depth study of empirical practices in science and ordinary life may lead to novel philosophical insights and may undermine traditional views and doctrines. If we take into account this point, the empiricist critique of the notion of causation proves to be untenable.

In this article, I put forward a basic philosophical claim: empirical scientific knowledge, that is, knowledge generated in experimental and observational practices, presupposes real causation. My discussion exploits two core notions from the philosophical analysis of scientific experimentation and observation: the aim of realizing object-apparatus correlations and the required control of the relevant interactions between environment and experimental or observational system. The conclusion is that, without the notion of real causation, acquiring epistemically sound empirical knowledge is impossible. Several empiricist objections to this conclusion are discussed and refuted. As a consequence, empiricism faces an unsolvable dilemma: either it cannot account for empirical knowledge or it should accept the existence of unobservable but real causal interactions.

Sam Rijken (Erasmus Univerity Rotterdam)

Science and Imagination

Imagination is gaining increased attention in philosophy of science. One of the most notable recent developments is the construction of a full-fledged philosophical account of scientific models that places the concept of imagination center stage: the fiction view of models. The fiction view of models promises to provide increased insight in the epistemic role of imagination in science and, in particular, in the practice of model-based reasoning. It achieves this by building the account explicitly on a conceptual framework for imagination from (Walton 1990). But the fiction view of models is a family of non-compatible rival accounts, not a single view. The differences between rival accounts arise mostly from the specific ways in which the conceptual framework for imagination is employed. To settle these debates, careful bottom-up conceptual analysis is required.

In this talk, I analyze the concept of imagination as employed in the fiction view of models. I discuss prevalent definitions of “imagination” and, partially following (Stuart 2021), I distinguish between “imaginative mental states” (the set of mental states that are distinctively imaginative) and “acts of imagination” (the cognitive process of having a sequence of imaginative mental states). It is the latter, not the former, that is the target of epistemological analysis. I then critically evaluate the common distinction between two types of imaginative mental states: “objectual” and “propositional” imaginative mental states. I argue that both types of mental states are present in epistemically significant acts of imagination (e.g. model-based reasoning à la fiction view of models), but that for the purpose of epistemological analysis such acts of imagination can be reconstructed exhaustively as sequences of only propositional-imaginative mental states (i.e. imaginative mental states with propositional content).

With these clarifications in hand, I follow (Meynell 2014) and argue that an answer to the question “how can we gain knowledge about the world just by using the imagination?” necessarily requires a two-step analysis: (1) analyze the imagined content and identify what generates and constrains it, (2) identify the knowledge about the world that the act of imagination is supposed to provide and analyze what justification, if any, the imagined content provides for that insight. I indicate, specifically, that only in step (2) we must answer the question: “where exactly do we stop imagining and start believing, and are we justified in doing so?” I indicate that the necessity of this two-step analysis has negative consequences for the viability of one version of the fiction view of models, and positive consequences for the viability of another. I also indicate that, for the fiction view of models, an answer to step (2) is exactly an answer to the question: "does this model accurately represent its target system?" I conclude by indicating that the fiction view of models, in conjunction with improved conceptual clarity on "imagination," straightforwardly suggests a new account of scientific thought experiments, which is a promising direction for future research.

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Jan-Willem Romijn and Hanna van Loo (University of Groningen)

Thick descriptions in psychiatry: Geertz meets Kraepelin

Owing to new research methods and the availability of ever larger data sets, psychiatric research is becoming increasingly data-driven. Our lead example is the I-SHARED project, in which clinicians use machine learning methods to predict the efficacy of interventions, share these predictions with their patients, and involve them in clinical decision making. To arrive at personalized predictions, psychiatrists build up a profile that represents the patient, based on a given catalogue of variables.

A well-known concern about data-driven predictions is that the data are infused with theoretical suppositions that do not get properly adjudicated. The opposite concern, namely that the data do not carry enough information to bring psychiatric illness into view, has received far less attention in the philosophy of science. Our paper directs attention precisely there. Our worry about the turn to data-driven research in psychiatry is that, while rich in implicit theory, the data are still too poor for adequate modeling and predicting. There are many aspects to the doctor-patient interaction that do not get represented in the predictive system, while they are vital to an adequate understanding of the patient's situation and potential recovery.

To make this claim concrete, we scrutinize the practical context in which psychiatric illness is modelled and predicted. When diagnosing the problems and determining therapeutic interventions psychiatrists routinely build up a narrative for their patients. These narratives include variables that can be found back in the psychiatric data structures but they often contain much richer content. Several aspects of the narratives are difficult to convert to research data. First, the narratives often indicate the causal background of the patient's problems. Second, the narratives provide specific meanings to data items about the patient. In the conversion from clinical practice and the narratives that capture doctor-patient interactions to an SPSS file, these finer semantic and causal details about the patient are at risk of being lost. Furthermore, it is simply impossible to convert the vast number of features and aspects of the clinical reality that may have some relevance to the case – curious behaviours, abnormal speech, disproportionate reactions, and so on – into a manageable data set.

Our primary conclusion is that, if we want to rely on data-driven predictions in the clinic and in research psychiatry, we are in need of what Clifford Geertz would call "thick descriptions", i.e., highly detailed data that includes intentions and meanings, over and above the direct observational facts. Perhaps surprisingly, some thickening of clinical descriptions is relatively easily accommodated. The key is a recommendation to designers of medical data infrastructure, namely: "keep good contact with the clinical context". There are, on the other hand, limits to this, e.g., variation in what the personal specifics mean to a patient is much harder to accommodate in database infrastructure. Our paper concludes with some indications on how far we might get in converting thick descriptions into data sets, against the background of a much broader discussion on the nature of explanation and understanding in the human sciences.

Rosa W. Runhardt (Radboud University)

Reactivity and Stability of Human Kinds: A Framework for Gender and Race

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Reactivity in the human sciences occurs when measurement or classification of some social phenomenon affects research subjects' attitudes and behaviour to such an extent that subsequent measurement or classification results are affected (cf. Jiménez-Buedo, 2021; Runhardt, 2021). Reactivity may, amongst others, consist of an individual's reinterpreting of some of the social concepts involved or of recalibrating their own position relative to others in a reaction to the measurement itself. For example, a research subject may gain self-knowledge about their mental health as a result of an initial take-in interview, and therefore report a different level of e.g. some depressive symptom in a second interview (cf. Ahmed & Ring, 2008). Reactivity can also take place at the group level, when an entire community changes in response to measurement. For example, racial categories imposed by a census measurement may develop into real categories binding together individuals (cf. Bonilla-Silva, 1997; Loveman, 2014), leading to subsequent changes in how the census is best set up in the future.

Reactivity is a relevant topic for both philosophers of science and social ontologists. Philosophers of science may consider the ways in which reactivity in the human sciences interferes with prediction and intervention, given that reactivity makes comparing different measurement results difficult. One could ask, for example, how and to what extent scientists can distinguish the effects of their own interventions from the effects of subject reactivity when trying to explain some change in a phenomenon of interest . For social ontologists, the proper framework for reactivity and its effects on the stability of so-called 'human kinds' remain topics of debate since Hacking's influential paper on looping effects (cf. Hacking, 1995; Laimann, 2020; Tsou, 2007).

In the previous, we saw that reactivity could be seen as an effect to control for in methodology, or a threat to stable social metaphysics that must be conquered. In this paper I will argue for a more positive interpretation: for certain types of phenomena, reactivity is legitimate, i.e. for those phenomena it does not undermine the accuracy of the measure. Specifically, research subjects' reinterpreting and recalibrating is legitimate for those phenomena which are not constituted or caused by simple biological regularities, but which instead combine social and biological aspects. Hallmark cases of such phenomena include 'race' and 'gender' (cf. Haslanger & Saul, 2006). The first part of the presentation provides an inventory of types of legitimate reactivity and focuses on the stability of race and gender concepts under legitimate reactivity. The second, more exploratory part of the presentation questions a strong candidate solution to the instability of race and gender concepts. Based on work in philosophy of mental disorder, where reactivity is also a widespread matter of debate, one may be tempted to 'stabilize' prediction and intervention by referring to the biological mechanisms that underly phenomena alone (cf. Tsou, 2019). However, I will argue that this solution is based on particular value judgements that could damage legitimacy as set out in the first part of the presentation.

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Federica Russo (University of Amsterdam) and Emanuele Ratti (Johannes Kepler Universität Linz)

Science and values: a two-way relation

‘Science and values’ is the name of a rich field, which includes debates (just to name a few) on inductive risk, scientific communication, feminist philosophy of science, etc. In (2021), Ward analyzes the diversity of terminology used to express the idea that science is value-laden, and in particular the different ways in which it can be said that a choice is value-laden. First, she highlights that values can either motivate or justify reasons for choices. Second, she describes how values can be causally linked to choices, being either causes or effects or choices. Ward points to several authors that notice how in playing a role in experimental design, the influence of values “filters the rest of research process, making downstream decisions like hypothesis appraisal value-laden” (p 56). This may look like the uncontroversial thesis that, down the road, choices have consequences that can be ethical. But Ward assigns an importance to this view - “pervasive but often hidden” (p 57) - that seems to suggest that there is more to this thesis than just the uncontroversial point that choices have moral consequences. The importance is also noticed by Elliott who, en passant in (2017), makes the interesting point that scientists’ choices “support some social values while weakening others” (p 13).

In this paper, we show the sense in which there is more to this thesis, and that the importance assigned by Ward is justified. We make this pervasive dimension of the relation between science and values explicit, by claiming that there are aspects of science (concepts, methodologies, etc) that are compatible with (and hence can possibly promote) certain values, whether scientists want it or not.

The central thesis of our paper is that the relation between science and values can have ‘directions’. In particular, there are two main directions which, as we will see, are not mutually exclusive; rather, they sometimes work in iteration, and sometimes independently. A first direction is from values to science, in the sense that values shape the concepts, the methodologies, and the choices made by scientists throughout the entire scientific process. The literature on inductive risk exemplifies the first direction; it is shown that science cannot be value-free, and that choices made on the basis of (non-epistemic) values are not only ubiquitous, but they cannot be avoided. A second direction is from science to values, in the sense that methodologies or scientific concepts constrain the values that we can possibly promote, because some methodologies or concepts are more compatible with certain values rather than others. The second direction has not received enough attention (especially if compared to the first); here we further develop the argument formulated in (Russo 2021) for the ‘from-science-to-values’ direction, and we explore some of its consequences. In particular, we formulate this thesis by developing two examples, one from the health sciences, and one from the use of algorithmic tools in the justice system.

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Tom Sterkenburg (LMU Munich)

The theoretical justification for machine learning algorithms

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A substantial research effort in machine learning is devoted to laying a theoretical basis for the great empirical success of machine learning methods. The field of machine learning theory is concerned, in particular, with deriving mathematical learning guarantees that purport to show that standard procedures, like minimizing error on a given training data set, are conducive to attaining epistemic goals, like successful classification of new instances (ShalevShwartz and Ben-David, 2014). As such, machine learning theory can offer an epistemic justification for machine learning methods.

Yet the usual interpretation of the influential no-free-lunch (NFL) theorems of supervised learning (Wolpert, 1996; Schaffer, 1994) is that we cannot formally justify our machine learning algorithms. That is, we cannot formally ground our conviction that some learning procedures are indeed sensible: that we have reason to think some possible algorithms will perform well in attaining the epistemic goals that we designed them for. In Wolperts original interpretation, "all learning algorithms are equivalent," so that, for instance, a standard learning method like cross-validation has as much justification as a ludicrously-looking method like anti-cross-validation (Wolpert, 2021). Consequently, philosophers have read these results as a "radicalized version" of Hume's argument for inductive skepticism (Schurz, 2017).

This raises a puzzle. How can there exist a learning theory at all, if the lesson of the NFL theorems is that learning algorithms can have no formal justification?

In this talk, I investigate the implications of the NFL results for the justification of machine learning algorithms. The main tool in my analysis is a distinction between a conception of learning algorithms as purely data-driven or data-only, as instantiating functions that only take data, and a conception of learning algorithms as model-dependent, as instantiating functions that, aside from input data, also ask for an input model. I argue that the NFL theorems rely on the former, data-driven conception of learning algorithms; and that there is here an important parallel to the philosophy of induction.

Namely, discussions surrounding the NFL theorems share a questionable presupposition with Hume's original argument for inductive skepticism: the idea that the performance of our inductive methods must be grounded in a general postulate of the induction-friendliness of the world. I side here with philosophical work that denies the cogency of such a principle, and that advances a local view of induction (Sober, 1988; Okasha, 2005). This leads me to a local view of learning algorithms: the model-dependent perspective. Many standard learning methods, including empirical error minimization and cross-validation, are best seen as taking two inputs: data, and an explicitly formulated model, which constitutes a choice of bias. What we can reasonably demand from such model-dependent algorithms is that they perform as well as possible relative to any chosen model. Consequently, learning-theoretic guarantees are relative to the instantiated models the algorithm can take, and it is in this form that there is justification for standard learning algorithms.

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Krist Vaesen and Wybo Houkes (Eindhoven University of Technology)

Active normative pluralists can contribute more to diversifying science

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In this paper, we develop a new form of socially engaged philosophy of science: a version of active normative pluralism on which philosophers of science can contribute extensively to ongoing efforts to diversify science. For this, we use insights from innovation science to analyze existing pluralists' arguments for the epistemic benefits of diversity. The analysis reveals non-trivial conditions for scientific communities to reap these benefits, and suggests epistemically effective strategies of re-organizing science, which philosophers of science could help implement.

Many philosophers of science have advocated versions of scientific pluralism, i.e., "a normative endorsement of the multiplicity of knowledge systems—theories, models or research approaches—in each area of science" (Van Bouwel 2015: 151). This normative endorsement is grounded in potential epistemic benefits of diversity to scientific communities, such as organized criticism (Longino 2001), the division of scientific labor (Kitcher 1993), and integrative explanations of complex phenomena (Mitchell 2002). Active normative pluralists, such as Hasok Chang (2012), therefore call for the deliberate diversification of science, i.e., increasing, fostering or minimally maintaining the diversity within a scientific community.

We argue that this strategy can be conceptually refined such that the focus on epistemic benefits of diversity offers a broader foundation for identifying and assessing interventions aimed at diversification. As for refinement, we show the need to systematically incorporate in pluralist arguments for epistemic benefits a distinction between types of diversity – variety, balance and disparity – that has been reconstructed for various disciplines by Andy Stirling (2007). Focusing exclusively on variety – as Chang's version of active normative pluralism does – fails to identify how balance of and disparity between knowledge systems affect the realization of epistemic benefits in an area of inquiry. We identify a number of non-trivial conditions for benefits to obtain, which are associated with balance and disparity as well as variety. In terms of deliberate diversification, we argue – negatively – that the exclusive focus on variety risks suggesting interventions that are ineffective or even counterproductive; and – positively – that considering all three types of diversity identifies a broader suite of interventions. Chang directs a call for action towards researchers in integrated history and philosophy of science, to recover, conserve and justify alternative knowledge systems. By contrast, we focus on current and potential efforts at diversifying scientific research undertaken by various gatekeepers – policy makers, funding bodies, and scholarly publishers – and on ways in which philosophers of science can engage with these gatekeepers to further these efforts. We offer various illustrations of efforts aimed at producing particular epistemic benefits, which can be made more effective by considering carefully their effects on variety, balance, and disparity.

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Martina Valković (Hannover University, Radboud University)

Cultural evolutionary theory and its limitations

The last decades have seen a rise in the application of concepts and methods from biological evolutionary theory to human cultures and societies in an attempt to explain their complexity, much like biological theory aims to explain the complexity of living beings. Some of the most influential proponents of this approach include Cavalli-Sforza, Feldman, Boyd, Richerson, Bowles, Gintis, and Mesoudi. In these theories, culture is conceptualised as information which is mainly stored in individual humans and, to a lesser extent, external artefacts. At the more radical end of the spectrum, it is primarily seen as neural patterns (Mesoudi 2011). Culture is broken down in distinct cultural traits or types which are transmitted from one individual to another. These traits vary in respect to their "fitness", which is sometimes understood in biological, and sometimes in cultural terms, and the cultural change is then the resulting change in the frequency of cultural traits.

This view fails to appreciate the complexity of human sociality and culture. Since cultural evolutionary theories espouse a particularly strong version of methodological individualism (Fracchia & Lewontin 2005), they are oblivious to the consequences of the fact that human societies are not just aggregates of individuals (Lewens 2015,), but rather structured wholes irreducible to individual psychology and behaviour. While social groups may be constituted by people, this does not mean that people determine all the facts about groups, their actions and intentions (Epstein 2015), or that the social world completely depends on individuals, their actions and mental states. Social structures are not reducible to individual actions nor merely emergent from them, since they can themselves be causes. A simplistic view of societies as little more than aggregates of individuals also leads to overlooking the importance of power inequalities in social and cultural change, making this a significant blind spot. In addition, cultural evolutionary theories do not show sufficient engagement with the developments in evolutionary biology. For instance, the proponents of the Extended Evolutionary Synthesis have recently highlighted the importance of constructive processes in evolution and the reciprocal nature of causation between the organism and its environment. Such approaches are hardly a novelty: Richard Lewontin, for instance, has long written about the interpenetration of organism and environment, as has Oyama.

Cultural evolutionary theory, as it stands, fails to fully appreciate the complexity of and the complex interaction between various processes which are at the core of how cultures and societies change. The flawed ontological assumptions in these sorts of accounts limit them in explaining social complexity; they pose significant limitations to their explanatory scope and relevance. The implication is that this approach to studying human culture and its change is, at best, incomplete and applicable only to limited cases, which makes it less relevant in studying human sociality. In a worst-case scenario, cultural evolutionary theory is a distortion, rather than a simplification, of human social processes (Cofnas 2018, p.316), making it unfit to explain most or even all cases of human sociality.

Ioannis Votsis (New College of the Humanities)

Theory Change through a Logical Lens

A familiar pattern can be seen in the history of science. Not long after a theory becomes established, the seeds of its demise are sown. In time, those seeds germinate into a full-blown rival theory, which supplantsthe earlier theory and resetsthe whole process. How long this pattern continuesis unknown as the dynamics of theory change are somewhat opaque. In this talk, I endeavour to throw some light on those dynamics by placing some aspects of the formation, alteration and elimination of theories under a logical lens. Taking the scientific realism debate as a blueprint, I identify some important lessons concerning theory change and offer a number of history of science cases in support. I then put forth two quasi-logical notions, content weakening and content strengthening, which can aid the explication of the dynamics of theory change, particularly in relation to cases of (dis-/)confirmation.

The four lessons that emerge in the context of the debate over scientific realism are chosen primarily because they form common ground between at least some realists and anti-realists. They are:

1. The most empirically successful theories in our possession may be very far away from the whole truth (Kuipers 2000).

2. At least some parts of the most empirically successful theories in our possession must be replaced.

3. Empirically successful successor theories must be such that they either straightforwardly reduce to or degenerate into the well-confirmed parts of their empirically successful predecessors (Radder 1991).

4. Theories ranging over different domains of phenomena sometimes share analogous structures.

Several cases from the history of science are adduced in support of these lessons. For example, with respect to the fourth lesson (Bartha 2010), one can bring up conservation laws as applied to the domains of energy, linear momentum, angular momentum and electric charge.

To explicate the dynamics of theory change, e.g. the replacement of theory parts, we need to conceive of theories as having content that is decomposable into parts. I propose to understand the content of a theory T in terms of a set of sentences that is closed under the natural consequence relation. A set of sentences K is closed under the natural consequence relation DN(K) iff it contains as members all and only sentences that are: (i) members of the deductive closure of K, (ii) non-logical truths, (iii) non-redundant in content and (iv) relevant consequences of one or more sentences in K (Schurz 2014). Two quasi-logical notions can then be constructed to help explain what happens in theory-change:

A theory T is content-weakened to a theory T- if and only if $DN(T-) \subset DN(T)$.

A theory T is content-strengthened to a theory T+ if and only if $DN(T) \subset DN(T+)$.

The application of the two notions fosters our understanding of the dynamics of theory change by painting a more fine-grained picture of the interplay between the content parts of theories in producing testable consequences.

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Scientific understanding in the image of science-as-practice: a pragmatist proposal

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Analytic philosophers cling to an image of epistemology centred around propositional knowledge — a thoroughly unpractical view of epistemology, unfit for the image of science-as-practice. As the practice-turn and experimental turn in philosophy of science have taught us, the epistemic and practical achievements of the sciences are much more diverse than what the propositional conception of knowledge can account for. Propositional knowledge is static, but scientific practice is dynamic; science is not merely verbal, but best described by verbs. In this paper, I argue that the recent turn towards understanding in philosophy of science and epistemology can provide fruitful grounds for integrating achievements from practice-oriented philosophy of science with those found in the literature on the epistemology of science. Since understanding is not necessarily propositional, as I will argue, properly understanding scientific understanding can help us in thinking about the epistemic and practical achievements of science. This talk aims at formulating a pragmatist account of scientific understanding that fits within the image of science-as-practice. In the first part of the talk, I will detail a picture of science as given to us by practice-oriented philosophers of science.

Following Hasok Chang, I take epistemic practices and epistemic activities as the units of analysis of scientific practice. I show how some notions of scientific understanding proposed by philosophers of science do not accommodate or fit into this picture of science; scientific activities are too varied and too dynamic to fit with conceptions of understanding that depend on explanation or propositional knowledge alone. In the second part of the talk, I formulate an account of scientific understanding that is able to accommodate this picture of science-as-practice, detailing a practiceoriented, pragmatist account of the epistemology of scientific understanding.

Building on Joseph Rouse's detailed and deep account of conceptual understanding and conceptual articulation in the scientific image, combining it with insights from Chang's underappeciated pragmatist account of understanding and intelligibility, I provide a reconstruction of two notions of understanding discussed by philosophers of science and epistemologists: pragmatic understanding and holistic understanding. I argue that the epistemic achievements of individual scientists' activities and operations are captured well by a notion of pragmatic understanding, while broader epistemic practices — being sense-making practices — is accounted for by holistic understanding. I suggest that pragmatic understanding is a matter of having the ability to perform intelligible activities that practically articulate methods and concepts, while holistic understanding is a matter of practically articulating locally coherent webs of conceptual dependence. Holistic understanding, on this picture, depends on pragmatic understanding, grounding the epistemology of scientific understanding in practical (or pragmatic) activities. With these notions of understanding in hand, I have formulated a pragmatist proposal for the epistemology of science, helping us understand how scientists gain understanding through their operations, activities, and practices, rather than by statically grasping propositions or explanations. This provides a promising way of bridging gaps between contemporary epistemology (of science) and practice-oriented studies of science.