SPSP 2024 Abstracts

Plenary Abstracts 1-3

Symposia Abstracts 4-27

Contributed Paper Abstracts 28-131

Poster Abstracts 29-148

Abstracts of Plenary Lectures

(in order of appearance)

The Art of Doing

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After enjoying a large success in the 1980s, 1990s and early 2000s, visual studies of science are now undergoing a period of critical revision. Pragmatist critiques of representationalism, as well as broader historical, philosophical and sociological critiques of the "ocularcentrism" that characterizes Western culture, have posed substantive challenges to what once was enthusiastically welcomed as a "visual turn" in Science and Technology Studies. In this keynote address I want to suggest that Philosophy of Science in Practice gives us the resources to tackle and reframe these critiques, and rethink the visual in science (and its relationships with society and culture) through the lens of a pragmatist and goal-oriented "art of doing". Drawing on case studies from my own research across science and art, I will identify some turning points where engaging with the SPSP community allowed me to develop and define this approach, posing challenges and offering insights that stretched it in novel and exciting directions. This will be an opportunity to take stock, as a community, of the crucial contributions we have collectively made, and are continuing to make, to the study of modelling and representational practices more broadly, but also – more personally – it will be an opportunity to acknowledge the influential and enduring role that SPSP continues to have on my own historical and philosophical work on representations across science and art.

Patient-Centered Measurement

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Contemporary medicine is Janus-faced. Evidence-based medicine is one face of it, emphasizing evidence, statistics, and method. Patient-centered care is the other, prioritizing patient experiences, judgement, and values. Government agencies, policy makers, major insurers and clinicians have sought ways to bring these faces together. Here I discuss one such approach, patient-centered measurement. Patient-centered measures can go by other names: the somewhat cumbersome 'patient-reported outcome measures' (PROMs) or the slightly whimsical 'quality of life measures'. Patient-centered measurement is the idea that patient perspectives on, for instance, physical functioning or quality of life, should play an evidentiary role in determining how effective a drug is taken to be, the degree to which a hospital provides good quality care or whether a particular intervention should be funded by an insurer. This idea may sound prosaic, but in fact it's nothing short of revolutionary. Patient-centered measurement treats patient perspectives *on par* with more traditional metrics such as mortality, morbidity, and safety. It says, patient views matter—not as an afterthought, and not only at the bedside, but in the nuts and bolts of creating our evidence base. What's more, these

measures are popular. They are part of FDA initiatives, the UK's development of the NHS, and Denmark's policy to improve patient care. Yet despite these policies, initiatives and recommendations, patient-centered measures present a puzzle. And this puzzle has its source in the Janus-faced nature of medicine. How can measurement, which relies on standardization, represent patient perspectives, which, if not idiosyncratic are at least various and changeable?

Being A Philosopher of Science In Practice, in an Inequitable World

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The notion of a distinct "philosophy of science in practice" has been around for nearly two decades. The philosophy of science in practice approach was partly a response to idealized notions of science that bracketed the messiness of the real world. But, much of that messiness—including messiness within the scientific community—is due to the world's massive social inequities. This raises the question: what are appropriate roles and responsibilities for philosophers of science in practice? What can and should we be doing, aside from getting the science 'right'? This presentation will give a semi-autobiographical look these questions, showing how different projects I have pursued illustrate my evolving attempts to decide what I can and should accomplish as a philosopher of science in practice, working in a world filled with intolerable inequities.

Abstracts of Symposia

(alphabetical by last name of session chair)

Demanding Evidence and Addressing Justice at the Intersection of Health and Environmental Sciences

Organizers: Federica Bocchi and Stefano Canali

Contributors: Federica Bocchi, Ariane Hanemaayer, Stefano Canali, and Helena Alves-Pinto

The humanities and social sciences have long investigated the challenges and opportunities in generating robust evidence for reliable, effective, equitable policy-making. These investigations have reinforced the work of science practitioners and policymakers, for instance in the fields of healthcare and environmental science. This session seeks to address evidence generation by focusing on the often-overlooked analogies and disanalogies between medicine and biodiversity conservation, particularly in the era of big data and evidence-based (EB) approaches.

Four presenters will critically examine the concept of evidence in these two domains, its nuances, limitations, and ethical implications, exploring how the proliferation of data-driven approaches both promises to improve interventions but may also perpetuate unequal power dynamics and marginalization. The symposium will bring together various disciplines—philosophy of science, STS, environmental science—working with diverse approaches and facing different challenges and will strive towards solutions drawing on interdisciplinary exchange. With a strong emphasis on timely issues and case studies, the symposium will foster interdisciplinary discussion at the intersection of evidential standards and ethical values.

The first speaker, philosopher of biology Federica Bocchi will introduce two types of operationalization of “evidence” in evidence-based biodiversity conservation. Using case studies from the Conservation Evidence Project and the GeoBon Biodiversity Network as paradigms, Bocchi will introduce a novel taxonomy of evidence and explore the advantages and disadvantages of operationalizing evidence in conservation, drawing contrasts with the field of medicine.

The second speaker, sociologist Ariane Hanemaayer, will problematize the turn to “precision medicine” in relation to the evidence-based paradigm and suggest that, far from being a novel development, precision methods are another reification of older approaches to knowledge production. While Hanmaayer’s presentation will rely on the Canadian healthcare system, the same issues can be raised in environmental contexts, where precision conservation is welcomed as a new development in nature management.

The third speaker, philosopher of medicine Stefano Canali will critically examine the promises and limitations of digital health technologies for Evidence-Based Medicine, with a focus on wearable devices. Complementing Bocchi’s talk on the notion of evidence with an example outside of conservation, Canali weighs on the potential of wearables to expand healthcare’s evidential ground while highlighting concerns related to representativity and overestimation.

The fourth speaker, applied ecologist and former policy consultant Helena Neri Alves Pinto will introduce and examine robust methods used in conservation assessment that are also widespread in healthcare policy. She will highlight the challenges of adapting well-established methods across disciplinary boundaries and the limitations of different techniques for decision-making.

With its focus on two important macro-areas that have been subject to great philosophical attention individually and the insistence on interdisciplinary collaboration, this session will advance the discussion around evidence-based, data-driven, and precision approaches, and it will appeal to a broad audience at SPSP.

Two senses of ‘evidence’ in Evidence-Based Conservation

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Over the past decades, the rise of ""evidence-based approaches"" has reshaped decision-making in science, emphasizing the importance of empirical data over subjective judgment. This paradigm extends to evidence-based conservation (EBC), advocating for environmental actions grounded in empirical data rather than untested ecological hypotheses. Despite the momentum behind evidence-based paradigms, philosopher Nancy Cartwright (2013) argued convincingly that this trend still lacks a philosophically sound and yet actionable account of evidence. In this talk, I explore the conceptual landscape philosophers should pay attention to when considering evidence in conservation, focusing on two case studies: the Conservation Evidence Project and the GeoBon biodiversity project. These projects highlight two distinct senses in which ""evidence"" is used in EBC, each raising philosophical challenges that demand attention.

The first sense of evidence philosophers should pay attention to revolves around assessing the effectiveness of conservation actions. Similar to evidence-based healthcare, EBC seeks to replace historically rooted practices with scientifically validated methods. A groundbreaking paper by Will Sutherland (2004) exposed the reliance on subjective opinions among conservation policymakers, hindering successful conservation outcomes. The Conservation Evidence Project, inspired by evidence-based medicine, systematically assesses conservation actions, employing a Delphi model to evaluate effectiveness. This allegedly Evidence-based project standardizes expert judgment to escape accusations of subjectivity.

The second sense of evidence philosophers should pay attention to involves the need to verify basic ecological knowledge underpinning conservation actions, such as the habitat fragmentation hypothesis. This second sense emerges from big data projects such as the GeoBon Biodiversity data network where what is meant by ""evidence"" is, actually, ""data."" Advocates for EBC propose testing these assumptions using diverse biodiversity data, attributing to data the foundational role of grounding reliable knowledge. However, equating data with evidence is a problematic move that overlooks how data are painstakingly ""turned into"" evidence and presupposes a commitment to the representational view of data.

I conclude by suggesting some crucial desiderata for a philosophically-sound-and-actionable account of ‘evidence’ for the evidence-based paradigm. These desiderata are extracted from the analysis of the two case studies and should

- consider the idiosyncrasies of conservation compared to biomedicine

- pay attention to the plurality of conservation practices (e.g. restoration vs protection).
- be committed to a reasonable view of the nature of data,
- account for how data are turned into evidence,
- reveal the impact of basic ecological knowledge on conservation strategies.

The notion of evidence is central to philosophers interested in epistemology and the methodological dimension of science. In this presentation, I investigate the notion of evidence at play in producing ecological knowledge to guide biodiversity conservation, thus contributing to the debate on meta-analysis (Kovaka 2022, Stegenga 2011), data as evidence (Leonelli 2016), and the knowledge-implementation gap.

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Discursive entrenchment: genetic epidemiology in precision medicine

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Is medicine undergoing a therapeutic revolution? This question has been raised by the countries that are integrating the use of genetic medicine under public and private umbrellas. This presentation will offer a view of the ""revolution"" of genomic medicine by considering the discursive field within which precision evidence is being incorporated. I demonstrate that the increasing role of genetic epidemiology and its techniques of data analysis, signal a further congealment of genetic medicine within the paradigm of liberal evidence-based medicine (EBM). As a case study, I rely on the institutionalization of genomics in Canada.

Epistemically and normatively, genomics introduces a form of evidence for decision-making based on a depth of biological and multi-omic data gathered from individual patients, rather than decisions based on statistical assessment of large therapeutic effectiveness studies (e.g., randomized controlled trials). However, there are obstacles to including genomic data in therapeutic decisions, which are both epistemic and regulatory. Furthermore, the legacies of EBM impact how laboratory or scientific data can become evidence for decision-making.

Canada is currently in a consultation period to develop a nation-wide infrastructure across its many provincial health systems that will have to respond to these issues. In the Genome Canada-funded initiative, All for One, representatives from Canada's various health systems have been involved in designing the "data ecosystems" needed for clinical and research activities. Other bodies, such as the Canadian Agency of Drugs and Technology in Health (CADTH) have been developing frameworks for emerging forms of evidence in health decision-making with respect to patient safety and reimbursement. In what the health fields have referred to as a new approach to diagnosis and therapeutics, genomic medicine is publicized as a precise way to treat individuals according to their unique needs (Garrido et al. 2018:443). While some critical scholars doubt the ability of precision medicine to realize its revolutionary promises (Mulinari 2023; Gobo & Marcheselli 2023; Engelmann 2022; Rose 2013), its institutionalization is already underway in Canada and abroad.

Research by medical and social scientists has already shown that genomic-based precision medicine functions as a "disruption" to the EBM conceptualization of efficacy and effectiveness (Dheensa et al. 2018:398). EBM sought to rectify the conventional gap between research and clinical care (e.g., Hanemaayer 2019; Cambrosio et al. 2020) through the use of the results of large trials to shape individual treatment decisions. For PM, there is an emerging reconfiguration of the notions of evidence: genomic researchers utilize "big data" strategies to gather vast amounts of "deep information" about individual patients through personalized diagnostics. Then, researchers in data sciences, such as the emerging field of genetic epidemiology, identify patterns (often with algorithmic technologies) corresponding with disease biomarkers through the use of large repositories of genomic data. This process is said to render the patient knowledgeable in an epidemiological terrain of biological data, using "deep data" to inform many types of decisions, including therapeutics, safety, and reimbursement.

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Can Wearables Fix Validity in Evidence-Based Medicine?

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Discussions of Evidence-Based Medicine (EBM) have dominated the last two decades of philosophy of medicine, leading to crucial work and collaborations between philosophers and biomedical researchers. While EBM has remained a central mode of evidence classification and use in medicine (and beyond), several of its assumptions and epistemic principles have been significantly criticized by philosophers of science and medicine (Stegenga et al., 2017). At the same time, new approaches are on the horizon. For example, the health context is increasingly permeated with new technologies, coming from developments in academic bioengineering and biomedical research, as well as the commercial and consumer technology sector. In this context, approaches under the umbrella term ‘digital health’ present new ways of collecting data and producing evidence for various areas of medicine and are surrounded by significant promises about their potential (Friend et al., 2023).

In this presentation I investigate how these promises of digital health can be understood as possible technological fixes for EBM, while warning against their limitations and the issues they bring.

I start by focusing on one of the most popular digital health technologies, wearable devices, as a way of investigating the concrete consequences and applications of the promises of digital health. Building on interdisciplinary collaboration with biomedical engineers, my analysis shows that digital health can be very promising from the point of view of the philosophical concerns on EBM, in particular in relation to issues of internal validity and external validity. Wearable data can be seen as an opportunity to include populations that are traditionally underrepresented in medical evidence, thus potentially fixing issues of external validity. In addition, wearables have the potential of expanding functions tested with EBM methodology, thus fixing some of the limitations of EBM related to internal validity.

These results expand discussions of EBM at the interface with new and emerging movements and trends in medicine, but in the second part of the presentation I argue that critical results from the philosophy of EBM remain central and extend beyond new modes of data collection and evidence classification. In particular, issues related to overestimation (the challenge where the detection of disease is systematically undermined by false positives) and lack of representativity (the extent to which data can be representative of health states of specific groups) offset the promises of digital health. Acknowledging these limitations is crucial to avoid epistemic issues related to evidence production and use, but also ethical issues and harms from the point of view of data justice (Taylor, 2017).

Engaging with current discussions on ongoing shifts in evidential standards for biomedicine and public health (Leonelli, forthcoming) and notions of evidence and conceptualisations of efficacy and effectiveness therein, the presentation expands existing philosophical work with a focus on new technologies and related methodologies and a discussion of their ethical and epistemic consequences.

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Are Protected and Conserved areas effective for biodiversity conservation? A reflection on Methods and tools for decision-making

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Protected and Conserved Areas (PCA), which include Protected Areas and other types of governance such as Indigenous Lands and Traditional Communities, are among the most important tools for biodiversity conservation. Lands governed by Indigenous Peoples and local communities have long been shown to have a positive effect in delivering environmental conservation outcomes (Hayes and Ostrom, 2003; IPBES, 2019). Yet, several of these areas are not formally recognized for their contribution to biodiversity conservation, and assessing the effectiveness is imperative for their inclusion in international targets such as the United Nations Global Biodiversity Framework (CBD, 2022) and for guiding national decision-making. PCAs can be particularly important in places such as the Brazilian Amazon, one of the most biodiverse biomes in the world, but highly threatened by land use change (Soares-Filho, 2010). Yet, only a handful of studies evaluating their effectiveness in doing so exist, and most are focused on forest formations (Nogueira et al., 2018).

In this presentation, I will describe some of the methods used in this work, in which I evaluated the effectiveness of PCAs with different governance regimes for promoting reduced deforestation in the Brazilian Amazon. More specifically, I will explore quasi-experimental counterfactual tests, which are among the most robust analysis tools for impact evaluation and that have been widely used in (and inspired by) the biomedical sciences. I demonstrate for the first time that Indigenous Lands and Quilombola Territories had positive results in reducing land use change, in some cases comparable to Protected Areas.

Based on the methods used in this work and its results, I will also reflect on some of the current discussions related to the use of data and methods for guiding decision-making. I will explore i) how and what tools for evidence-based impact evaluation practices are used, ii) what are the limitations of such tools, and iii) how these limitations and trade-offs reflect on risks and opportunities for the decision-making process.

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Sowing a Philosophy of Agricultural Science

Organizer: Julia Bursten

Contributors: Julia Bursten, Catherine Kendig, Ryan McCoy, and Joshua Tonkel

Agriculture is born on experiment: intervening on the land to create a crop from a previously uncropped space. Since the formation of settled human communities, the need to feed growing populations has led civilizations around the world to create and pass on knowledge about growing food, from plant domestication to plowing techniques, from crop rotation to irrigation practices. One of the oldest experimental practices, humans have for millennia dedicated themselves to making two blades of grass grow where but one grew before.

“Agricultural science” names a wide array of experimental and scientific practices that are united under the banner of using scientific methods, theories, and insights to improve agriculture. It is wildly diverse in terms of scientific disciplines, drawing from — and contributing to — plant and animal biology, chemistry, physics, economics, materials and civil engineering, and more.

As a discipline, U.S. agricultural science arose from a 19th-century movement to apply scientific methods to improve the cultivation of food, fuel, and fiber. This movement found common cause in contemporaneous education reform movements, and together these movements led to the establishment of land-grant universities by the 1862 Morrill Act. America's nascent agricultural science community harnessed these new institutions to pursue their own professional goals, solidifying the agricultural research orientation of land-grant schools through the agricultural experiment stations created under the 1887 Hatch Act.

These developments intertwined the histories of American higher education, modern agriculture, and agricultural science into the 20th century and beyond. Agricultural scientific research will feature prominently in solutions to urgent, global 21st-century problems, including the energy crisis, as well as food shortages and ecological shifts anticipated to result from global climate change. Therefore, analyzing the historical and conceptual foundations of agricultural scientific knowledge should be a research priority for contemporary philosophy of science. Agricultural science is also a source of case studies that show that a workable notion of scientific knowledge must include space for local as well as usable/practical knowledge, in contrast to grasping universal, exceptionless laws of nature.

Work in agricultural ethics has long shone a light on the ineliminable role of values in shaping agricultural practice and agricultural scientific research — whether they be the values of farmers, land-owners, agroindustry, agropolicy makers, or the rightful Indigenous stewards of land. However, the integration of conversations about values in agriculture and agricultural science within history and philosophy of the agricultural sciences is still nascent. So a rich opportunity exists to explore the philosophy of agricultural science from a variety of overlapping yet distinct perspectives. The talks in this symposium offer a vision of an agenda for future philosophy of science research on agricultural science. Bringing together researchers from a diverse array of institutions, departments, and research areas, we highlight four distinct ways to carry out research in philosophy of agricultural science. A panel-style Q&A will follow talk, wherein symposiasts will discuss additional work in progress and share results from an NSF grant on the philosophy of agricultural science in which all symposiasts directly participated.

The Pursuit of Useful Knowledge in Agriculture, 1887–1920

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Scientists and academic leaders in the US have appealed to “useful knowledge” throughout American history, but a shift in the conception of useful knowledge occurred in the early 19th century. In the 18th century, “useful knowledge” was considered to be knowledge that prepared the American elite to properly navigate republican society. However, by the 19th century, changes in sociopolitical landscapes and new philosophies of education rewrote “useful knowledge” as knowledge that would enhance the productivity and social standing of industrial occupations.

The creation of land-grant institutions by the 1862 Morrill Act inculcated this ideal into institutions of higher education. Justin Morrill, arguing on behalf of his namesake legislation, maintained that the creation of agricultural colleges would recognize “the propriety of encouraging useful knowledge among farmers and mechanics.” When the 1887 Hatch Act created agricultural experiment stations to “aid in acquiring and diffusing…useful and practical information on subjects connected with agriculture,” the useful knowledge ideal took root within the growing field of agricultural science. While historians have helpfully described the history of this useful knowledge ideal, they have not analyzed the influence of this ideal on the practice of science.

In this talk, I will use case studies from the history of agricultural science to articulate how the 19th century's conception of useful knowledge shaped 19th and 20th century scientific practices. In particular, I will discuss how the ideal of usefulness shaped the choice of research questions and the verification of research results within agricultural science. Analysis of these phases of agricultural research reveal that the creation of useful knowledge was generally more concerned with local problems and conditions rather than providing universal solutions. Comparative examples from the work of agricultural experiment stations in Iowa and Mississippi between 1887 and 1920 will highlight this local dependency of useful knowledge and the impact this had upon the practice of useful scientific research.

Agricultural scientists addressed problems raised by the farmers of their state, and results from agricultural experiments were not fully trusted until they bore fruit in the fields of those farmers. Different climates, social conditions, and agricultural practices made the concerns of Mississippi farmers different than those of Iowa, and the differences in research performed at Iowa State as compared to Mississippi State demonstrates the influence of that regional variation. Similarly, recommendations based on experimental results from Iowa were not followed in Mississippi if those results did not improve Mississippi agriculture. Rather than pursuing knowledge for its own sake, agricultural scientists recognized that for the knowledge they produced to be useful, it must be grounded in these local conditions. This was a key feature of agricultural science in the late 19th and early 20th centuries, and this talk's description of these features will highlight aspects of scientific practice that are not accounted for in traditional accounts within the philosophy of science. This makes agricultural science an exemplar for understanding the knowledge-making processes of more "applied" scientific fields that aim to create useful knowledge.

Implementing Sustainable Agricultural Practices: A Partial Overlaps Approach

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Agricultural production in the United States is both a contributor to climate change and yet also stands to be severely impacted by it. As the most recent National Climate Assessment notes, climate change not only increases agricultural production risks through changes in growing zones and growing days, but presents a broader social risk by disrupting food systems—the impacts of which disproportionately impact low-income and vulnerable communities (NCA5). In light of these risks, implementing sustainable agricultural practices is both critical for climate mitigation and adaptation, as well as ensuring resilient and just food systems. However, farmers, researchers, and other stakeholders are faced with unique challenges in implementing these practices. Included among these challenges are economic constraints, political differences, differences in belief regarding climate change, as well as other barriers for communication and outreach.

In this talk I propose utilizing a partial overlaps framework (Ludwig 2016, Ludwig and El-Hani 2020) to better analyze these barriers and differences, as well as provide entry points for adopting sustainable agricultural practices. Such a framework assumes partial overlaps in ontological, epistemological, and value commitments among heterogeneous actors. In turn, identifying these partial overlaps provides common ground for collaboration. In the case of implementing sustainable agriculture practices, I argue that this framework importantly illustrates how collaboration is possible even in light of deep differences among actors, particularly differences in beliefs about the existence of climate change.

In order to illustrate this framework's application within sustainable agriculture, I draw on case studies from field observations and semi-structured interviews with farmers, researchers, and Cooperative Extension personnel in Michigan that were conducted as part of a National Science Foundation-funded project on the epistemology of agricultural science. In these studies I focus on climate change impacts on potato crop storage in Michigan, adaptation strategies taken in response, as well as climate mitigation efforts in potato production.

For example, Michigan's climate has historically been ideal for potato crop storage. However, rising temperatures in the region have increased crop spoilage in storage facilities and now present a challenge for adapting these facilities to climate change. I highlight some of the barriers for implementing these adaptation strategies, including economic constraints for farmers purchasing new equipment, as well as barriers for communication given differences in beliefs regarding climate change. Utilizing a partial overlaps framework, I show how adaptation strategies have been implemented despite these differences. For example, by attending to epistemological overlaps in observations of crop spoilage, as well as value overlaps regarding economic concerns, farmers, researchers, and Extension personnel have found common ground despite ontological differences in their beliefs regarding climate change.

Digging deep in the sociality of interaction: lessons from knowledge-making in potato science

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In her recent "What's Social About Social Epistemology?" (Longino 2022, JFP 109(4)) Helen Longino turns her attention to a particular aspect of the sociality of science, which she terms "the sociality of interaction." (171) In her account, social interaction among scientific groups is essential to the production of scientific knowledge. She discusses many activities that exemplify the sociality of interaction in the sciences — from the uses of testimony and shared categories for representing phenomena to collecting, sharing, and disagreeing about data — and argues that these activities produce scientific knowledge through social interaction.

We are broadly sympathetic to this view. However, in her analysis, Longino argues that "concern with practices that are productive of knowledge, rather than with the content and subject of knowledge" should be the focus. (173, emphasis added) This suggests that it is both possible and desirable to analyze practices without analyzing the content and subject of knowledge, and further that analysis of content-knowledge in a given scientific domain should not be the focus of attention if the goal of a philosophical investigation is understanding scientific knowledge production. We disagree.

Consider the agricultural-scientific practice of extension, a type of knowledge-producing work in agricultural science. Extension is a legally and institutionally defined social system that produces interactions between scientists and the public in contemporary U.S. agricultural science. The success conditions of producing extension knowledge are inherently, deeply, and interactively social. In this talk, we show that the sociality of successful knowledge production in extension is inextricable from the “content and subject” of new scientific knowledge produced through extension.

We illustrate our point through a discussion of agricultural-scientific research on potatoes. Such research is often carried out via extension. We argue that what potatoes are is intertwined with human interaction. The significance of potatoes as a food crop impacts how research on potatoes occurs, from what questions are asked to what scientific practices are able to be carried out to answer those questions. Most field trials on potatoes occur on privately-held farms that partner with researchers, and commercial farm workers rather than principal investigators grow the crops that produce the objects of study. Extension often mediates these partnerships, and from historical and present-day farmers and extension workers to commercial and academic agricultural scientists, networks of interactively social partnerships remain intertwined with the production of potatoes — and knowledge about them. We show how extension generates content-knowledge that is inseparable from the practices productive of that knowledge.

Our analysis is ultimately a friendly amendment to Longino’s view. We take seriously the centrality of the sociality of interaction in producing scientific knowledge. We contend that knowledge co-production practices in agricultural science illustrate (1) how knowledge about agricultural experiment is made through social interaction and (2) why such interaction is essential for epistemic content. What we propose is an admittedly strong form of sociality in which sociality is constitutive of knowledge in a way that without it, that which is being discussed ceases to be knowledge if it is not social.

Measurement Across the Life Sciences: Conceptual Issues

Organizers and Contributors: Dana Matthiessen, Alan Love, Marina DiMarco, Aja Watkins, Ruth Shaw

Recent decades have witnessed a renaissance in the philosophy of scientific measurement. This work has uncovered a variety of epistemological and methodological issues in measurement practices across a range of disciplines, including: metrology, physics, psychology, and the social and behavioral sciences. However, there are few analyses of measurement in biology. This is not because it lacks the conceptual richness encountered elsewhere, nor is it because biologists are unconcerned with the challenges of measurement. Biologists working on quantitative analyses of form (“morphometrics”) have lamented the absence of sustained reflection on measurement methodology (Bookstein and Schaefer 2009). Quantitative geneticists have explicitly utilized the representational theory of measurement to diagnose pervasive errors in their field that threaten to render the findings of many of their studies meaningless (Houle et al. 2011).

The time is ripe, then, for philosophical analyses of measurement in different contexts of biological inquiry. Such work stands to contribute to our understanding of foundational issues in the life sciences and in the philosophy of measurement. With regard to biology, attending to measurement opens up diverse and underexplored domains of scientific practice. In some cases there is no explicit discussion of the role of theoretical presuppositions in the design and execution of measurements. Debates over the proper meaning of biological terms like function, fitness, and sex have largely played out at a conceptual or definitional level. Closer attention to measurement practices in which competing conceptions gain traction or encounter obstacles can and should inform stances on the biological significance and plausibility of these philosophical accounts. With respect to philosophy of measurement, paying attention to biology will help to expose new topics distinctive of the life sciences. Measurements play different roles in different fields. Rather than a march toward precision, measurement in biology has been described "as a steady accumulation of the understanding of variation, its models and significance" (Bookstein and Schaefer 2009, 2).

The contributions to this symposium are deliberately drawn from a range of theoretical and practical traditions within biology; the aim is to capture the diversity of measurement issues that arise in the practices of its heterogeneous disciplines. Dana Matthiessen argues that recent controversies over the reliability of high-throughput techniques call for a conceptual framing that goes beyond traditional justifications for computational methods in biology as a means to circumvent researcher bias and theoretical presuppositions. Marina DiMarco and Aja Watkins argue that the heterogeneous measurements of "sex" in biological practice extend and fragment the concept in ways that resist unification . Alan Love draws on measurement practices in biomechanics to argue that philosophical analyses of function must take into account the unutilized capacities of biological parts, the incompleteness of empirical analyses, and how the functions of parts relate to an environment. Finally, Ruth Shaw discusses her scientific experience developing analyses of fitness measurements as an evolutionary biologist, including the need to account for unique statistical features of fitness data in order to reliably infer population properties in practice.

Measurement in High-Throughput Biology: Moving Beyond the "Objective" and "Data-Driven" Ideals

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The latest methods in high throughput biology, based on single-cell resolution measurements, have been simultaneously celebrated as a path to novel biological insights and labeled a "specious art" (Chari and Pachter 2023). The praise and criticism echo prior controversies over the use of computational methods in molecular biology, and occur alongside worries over the reproducibility of research in the field (Fanelli 2010; Tiwari et al. 2021; Ziemann et al. 2023). Such methodological debates have historically been framed in terms of two dichotomies: objectivity versus subjectivity and hypothesis-driven versus data-driven research. I argue that neither framing is adequate for addressing current issues. Rather than downplay or simply criticize the role of researcher choices and theoretical presuppositions in high throughput measurement, these should be explicitly acknowledged as integral elements of the data generation and handling process. Thinking about how to properly constrain or guide researcher decisions (e.g., by examining their theoretical presuppositions) is crucial to securing their reliability.

First, I provide historical context. Arguments for the adoption of numerical and big data methods in the sciences and public policy have historically associated increased quantification with increased objectivity (Porter 1995; boyd and Crawford 2012). These arguments played a significant role in the rise of computational biology. In the 1960s, researchers claimed that automation and computation would free the production of biological knowledge from an undesirable dependence on expert judgment and intuition (Hagen 2001). In the 1990s, the Human Genome Project spurred development and promotion of high-throughput technologies, capable of generating far greater volumes of genomic data (Garcia-Sancho 2012). Since the value of ever-more data was not obvious, advocates justified this approach by appeal to a "data-driven" model of research in which the algorithmic detection of patterns would lead to the discovery of novel genes and functions, in contrast to the predominant hypothesis-testing model (Strasser 2021). As one textbook put it: "The fundamental idea behind these approaches is to learn the theory automatically from the data" (Baldi and Brunak 1999). Despite their successes, scholars have described a persistent anxiety among biologists over the objectivity of computational methods (Suárez-Díaz and Anaya-Muñoz 2008). Some have castigated such work as subjective, purpose dependent, and more art than science (e.g., Bowman 2009).

This conflict is unresolved, as seen in controversies over single-cell techniques. The second part of the talk analyzes recent controversies strategically. Drawing on tutorials (e.g., Leucken and Theis 2019; Heumos et al. 2023) and training seminars, I identify the many choice points that arise in single-cell data processing pipelines. I describe problems these choices present for the reliability of single-cell measurement outcomes, per recent criticisms (Chari and Pachter 2023; Sparta et al. 2023). Finally, I critique the traditional framing of these issues and demonstrate that they are better addressed by resituating the role of theory with respect to measurement, acknowledging the role of purposive agents in the measurement process, and resisting decontextualized views of biological data (e.g., de Chadarevian 2018).

Measuring and Extending Biological Sex

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Biological sexes figure in a broad array of projects in the health sciences and evolutionary biology. In these contexts, sexes are operationalized with incredible variety or "dimensionality," ranging from morphology to hormones, karyotypes, and gamete sizes (Bauer 2023). Philosophers of science have likewise approached the concept of sex from a variety of perspectives. Sarah Richardson's (2022) sex contextualism focuses on sex as it is measured in biomedical research practice, yielding many legitimate operational definitions. By contrast, other philosophers have approached the concept from the perspective of classification and its role in scientific theorizing, asking whether sexes are natural kinds (Franklin Hall 2021; Khalidi 2021) and investigating biological sex in light of its role in theories such as sexual selection (Griffiths 2021; Evron 2023). Further, whereas Richardson argues that operationalizations of sex are subject to ethical and pragmatic considerations, in line with recent work on critical metrology (Boulicault 2021), Griffiths argues that the uses of "sex" in biomedicine and policy contexts are mere operational definitions that "all rely on the more fundamental definition that comes from evolutionary biology." In light of this, Griffiths (2021) distinguishes the adequacy of these operational definitions for social and scientific purposes from their adequacy qua operationalizations of biological sex.

Approaching biological sex from the perspective of the philosophy of measurement helps put these competing concepts of sex and debates about their ethical implications in more direct and fruitful conversation with each other. We reconstruct Griffiths' gametic reproductive strategies account and Richardson's sex contextualism explicitly in terms of possible views one could have about what researchers are doing when they claim to measure "sex." Drawing from work on operationalism (Chang 2019), we use this to contest Griffiths' assertion that operational definitions of sex in biomedicine are operationalizations of the gametic concept and to claim that different operational definitions are being used to extend the concept of biological sex in distinct ways.

First, we diagnose the heterogeneity of practices for measuring sex in the health sciences as instances of using different measurement methodologies to extend the concept of biological sex to new scientific purposes. When health scientists measure "sex" in humans, they are concerned with a variety of local and pragmatic research aims, largely orthogonal to evolutionary theorizing. This is concordant with established challenges of adapting measurements to specific, local purposes in social science (Chang and Cartwright 2013). We contrast this with the case of fungal "mating types" in evolutionary biology, where calling mating types "sexes" would seem to extend the concept to accommodate new evidence. Here, operationalizing sexes as mating types seems analogous to episodes of extending concepts in the physical sciences, such as temperature (Chang 2004; 2009). Extending operational definitions of biological sex to local and pragmatic inquiries in biomedicine does not diminish their status as sex concepts but rather fragments the concept across research areas. Instead of insulating concepts of biological sex from ethical considerations, this merely makes them more specific.

Measuring Biological Function in Biomechanics

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Functional reasoning is ubiquitous in biology. Traditional philosophical debates have concentrated on formulating theories or concepts of function, especially with the aim of evaluating whether they are mutually exclusive or suggest some form of pluralism (Garson 2016; Wouters 2003). However, there has been almost no attention to how function is measured empirically. This is poignant given that practices of measurement related to function are central to the life sciences across spatial and temporal scales, from cellular signaling to the functional morphology of fossils.

Another reason to scrutinize the measurement of biological function is the renaissance in the epistemology of measurement (e.g., Tal 2017). Under what conditions is measurement sufficiently accurate or precise and how does it contribute to knowledge in different domains of inquiry? How is measurement related to other concepts like observation, experimentation, or modeling? How are indirect measures or proxies utilized, especially in light of rapid technological change? Answers to these questions may harbor unexpected insights. An analysis of proxies in functional genomics indicates that researchers utilize diverse indirect measures of functionality, rather than trying to improve or replace proxies, because researchers combine them in creative ways to generate new insights and research questions (Guttinger and Love forthcoming).

Biomechanics is the study of organismal behavior involving mechanical principles usually applied to non-living objects. Studies in this area conceptualize traits as complex configurations of functionality (e.g., the cranium complex of bone, muscle, and neuroarchitecture for mastication). One key area of biomechanics is animal locomotion (Biewener and Patek 2018), which involves the study of distinctive activities or movements (e.g., flying or swimming) related to different environments (e.g., air or water), the components and organization that generate locomotory capacities (e.g., muscles and tendons), and their underlying metabolic requirements (i.e., energetics). The quantitative measurement of variables relevant to both the organism and its environment are crucial for these investigations. However, these measurements are always indirect (i.e., proxies), typically physical magnitudes like component forces or areas and lengths from anatomical constituents, and often involve relational properties between organism and environment. Variables for the values of these measurements are juxtaposed in equations via abstract concepts (e.g., lift or strain) that quantify the functional capacities of organismal parts in relation to specific contexts.

Analyzing the measurement of function yields philosophical insights in two different dimensions. First, our understanding of scientific practice is expanded to appreciate the ubiquity of proxies in the epistemology of measurement and recognize how measurement often demands interdisciplinary coordination (e.g., between biology and physics). Second, traditional debates in philosophy of biology surrounding function are reconfigured: (i) analyses of adaptation and optimality must reckon with measurements of skeletal load functionality that reveal ‘unutilized’ capacity three to five times greater than is experienced by an organism, and (ii) our accounts of functional analysis must grapple with incompleteness (investigations rarely identify all working parts, characterize each operating behavior, or validate every contributing role) and relationality (how the environment is relevant to system functioning).

Measurement of Fitness and Estimation of its Properties

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Fitness is a central concept in evolutionary biology. A widely accepted measure of fitness is the number of offspring an individual produces. It also encompasses survival to reproduction, given that only survivors contribute descendants. These components of fitness can be considered for entities at different biological ‘levels’ (e.g., individual, group, gene). For simplicity, I here focus on individual fitness, taking as the time horizon the immediate offspring. Context-dependence is also a critical aspect of fitness. Environmental conditions influence expression of virtually all organismal attributes. Moreover, the term ‘fitness’ refers to how well suited an organism is to its own habitat. Accordingly, it is of particular relevance for evolutionary studies to measure fitness in ‘realistic’ environments (i.e., those the organisms themselves inhabit).

Measurement of fitness of an individual is, in practice, quite labor-intensive, but it is straightforward, in principle, if only for short-lived, macroscopic organisms. Given this, it is surprising that there was no documented effort to measure fitness for nearly a century after

Darwin (1859) implied, though did not name, the attribute. Fitness was first treated formally in theory that JBS Haldane (1924) developed 65 years after Darwin's Origin. Here, and in much

subsequent population genetic theory, fitness is treated, again, for simplicity, as survival alone. Once the empirical study of fitness began (Kettlewell 1955), initial efforts recorded survival versus mortality, neglecting reproductive output. The sporadic efforts that followed continued this practice until plant population biologists, appreciating that plants "stand still and wait to be counted" (Harper 1978), included fecundity in fitness records (e.g., Antonovics 1984).

Beyond measuring fitness for a set of individuals, though, there is generally interest in using such a dataset to assess one or more properties of the population that individuals are considered to represent (e.g., its growth rate, the selection impinging on traits, or its adaptive capacity). Some authors use the language of 'measuring' these properties, but I urge the distinction that we estimate them, recognizing that we have incomplete samples of the reference populations and also that we draw on statistics, a body of practice grounded in its own rigorous theory, to estimate them, along with their uncertainty. Realizing these goals entails attention to design of the study, whether observational or experimental, to minimize or eliminate confounding and also to enable genetic inference.

In confronting these datasets, investigators noted that the population distribution of a comprehensive measure of fitness as an individual's lifetime contribution of offspring does not meet standard statistical assumptions (e.g., of a normal distribution). As a plant evolutionary biologist working to address empirical questions about fitness, I see many early efforts to analyze these unruly data, including my own, as stumbling toward the inferences of primary interest. More recently, we have secured sound statistical grounding to support inferences about fitness. I will highlight my collaboration with statistician Charles Geyer to develop aster analysis (Shaw et al. 2008) to address the challenges of analyzing fitness data for inference of properties like those listed above.

Individuality and Genealogy Across Living Systems

Organizers and Contributors: Celso Neto, Haber Matt, Maureen Kerney

This symposium is about individuating practices in biology. In particular, we examine how living systems form lineages, how (if) these lineages can be reconstructed, and what this means for the individuality and history of those systems. Thus, we explore the intersection of two topics of major concern for philosophers and scientists, namely: the problem of biological individuality and the problem of genealogical/phylogenetic reconstruction.

Traditional views on individuality have been challenged by a wide range of phenomena, such as plant reproduction, lateral gene transfer, bacterial assemblages, and holobionts (Clarke 2010; Bueno, Chen, and Bonnie Fagan, 2018). Some of these phenomena have been challenging phylogenetic and other classificatory approaches to genealogical reconstruction (Doolittle 1999; Degnan and Rosenberg 2006; Velasco 2010). These challenges led to a rejection of the traditional view of evolutionary history as an ever-bifurcating tree, prompting the continuous development of new integrative methodologies and concepts (Huson and Bryant 2006). These developments have the potential to reshape our thinking about individuality, but this potential has been mostly neglected (but see Haber 2016). Moreover, advancements in phylogenetics suggest that living systems form an array of different but integrated types of lineages at different levels (Haber 2012; Neto 2019). The epistemic and ontological dimensions of this pluralism have been gradually explored by philosophers, but more engagement with scientists can improve this exploration.

To explore these gaps, the symposium brings scientists and philosophers together. We consider new methods in phylogenetics and a wide range of lineage-forming living systems, from bacterial colonies to humans.

Phylogenetic Complexity and its Consequences

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Phylogenetic biology is undergoing a significant conceptual shift given major new discoveries that challenge traditional views of evolutionary and phylogenetic dynamics. Earlier views are now challenged because, for example, genomes can be transferred across diverse clades, introgression between lineages is more common than imagined, and symbiotic associations blur the lines between individuals. Biological entities have variably fuzzy boundaries that enables these dynamics. Overall, the degree of complexity, entanglement, and dynamics of hierarchically connected entities has been found to be prodigious. As a result, evolutionary patterns and processes are deeply interconnected at various temporal and spatial scales, among various hierarchical entities, and across genealogical domains. A contemporary view of the phylogeny of life is of a multidimensional complex system with multimodal diversification of lineages (both vertical and horizontal modes), multilevel historical lineages, and emergent properties that manifest at various levels of integration.

This sets the stage for an exciting new era of research in evolutionary biology and phylogenetic biology. For example, empirical research into phylodynamics – the exploration of where, how and why vertical and non-vertical evolution occurs across phylogeny – is an enormous and relatively unexplored area. While phylogeny does not conform to a fully neo-Darwinian divergent tree, it is also true that phylospace is not completely reticulate – everything does not happen everywhere all at once. Conceptual and empirical bridges between micro- and macroevolution must also be reconsidered. Based on a contemporary understanding of evolutionary biology, it is challenging at best to compartmentalize processes and patterns into discrete areas of the genealogical hierarchy. Instead, patterns – and the processes underlying those patterns – may be “macroevolutionary “or “microevolutionary” (using the traditional parlance) in scope.

On the methodological side, development of new, integrative models for phylogenetic comparative methods that accommodate the complexity of evolution and consider multimodal phylogenetics will be a significant advance for macroevolutionary and other comparative biology research. Also, new methods of phylogenetic reconciliation are necessary – not just for reconciling gene and species trees, but instead extending reconciliation approaches to enable modeling a more complete phylogenetic system without privileging any one genealogical level. Finally, wide-ranging phylogenetic applications used in other areas of science and science policy are significantly impacted by, but do not yet reflect, these major shifts.

Applying the Recursive Account of Individuality

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The recursive account of individuality (RAI) is that "biological individuals are lineage generating entities that are both constituted by and constitutive of other biological individuals." This work-in-progress talk explores how that account may be applied in different biological contexts. This informs us about important elements of biology and, more broadly, about pattern-based sciences.

The RAI is intended as a heuristic account, as opposed to an analytic one. Rather than identifying necessary and sufficient conditions or other definitional criteria, the account prompts research strategies and questions that advance our understanding of biological individuality. These questions, in part, are derived from biological practice, tracking research strategies that have proven effective at interrogating the systems we seek to understand—or that have challenged entrenched views on things like lineages or biological individuality.

In practice, an important element of the RAI is tracking the recurrence of lineages along different dimensions and temporal scales. The nature (dimension) of that recurrence is left unspecified, i.e., it is atheoretical, though requires an empirical grounding. Recurrence may be expressed in the ways lineages are bound by shared fitness, metabolism, immunology, physical proximity, or other relations. These sorts of recurrences may overlap or identify distinct individuals.

Recurrence of lineages extends over time. That may be persistent or intermittent and over very different temporal scales. Recurrence may be ephemeral, extend over developmental or generational time, or persist over evolutionary scales. The different ways that lineages may recur along various temporal scales is a feature of the RAI that may be investigated empirically. These patterns of lineage recurrence reflect important patterns of divergence and diversification, reflective of biological mechanisms, processes, theories, and practice that help advance our understanding of biology.

Here, I will look at what this looks like in practice by examining candidate cases of the application of the RAI. Those include (a) studies on bacterial swarming behavior and the evolution of ephemeral bacterial colonies; (b) taxonomic accounts of benthic meiofauna displaying transitional individuality; (c) appeal to patterns of lineage recurrence as a difference maker in determining the individuality of candidate holobionts and perspectival individuality; and (d) macroevolutionary patterns of lineage persistence strategies.

Paying attention to biological practice identifies exemplar cases of the application of the recursive account of individuality. This highlights important features of the RAI, e.g., that it is a heuristic account; that it is empirically driven; that is is reflectively bound in theory and practice, though not any specific theory or practice, etc. More broadly, this approach also highlights how pattern focused sciences may be distinctive from those focused more on processes or mechanisms.

Individuating Human Lineages

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“Lineage” is a common currency in scientific parlance, appearing in theories, models, and explanations across biology. Nevertheless, the emphasis of philosophers on organismic evolutionary theory has led to an overly narrow account of lineages, one focused on easily individuated genealogical sequences of evolving populations (Godfrey-Smith 2014). Previously, I have argued against this narrow account and proposed lineage pluralism, i.e., the view that lineages come in different kinds and play distinct epistemic roles in scientific practice (Neto 2019; 2020). In this talk, I consider the different kinds of lineages present in the high-impact field of human ancestry research.

In this area, scientists reconstruct the ancestry relationships of human individuals and populations to understand evolutionary history and improve biomedicine. Scientists distinguish two notions of ancestry: genealogical ancestry refers to the ancestors in someone’s family tree, while genetic ancestry refers to only those ancestors whose genes were inherited by that person (Mathieson et al. 2020). Someone’s genetic lineages will be only a sub-group of their genealogical lineages. This distinction gives us two criteria for identifying ancestors and, thus, individuating human lineages. One criterion is based on human reproductive events, while another is based on genetic inheritance.

Reproduction and inheritance are usually coupled, but they can lead to different individuation practices depending on the timeframe considered in the research. This point determines the scope and legitimacy of human ancestry research. Modern DNA studies of ancestry are focused on tracing genetic ancestry and cannot fully reveal someone’s genealogical ancestry. Knowing this fact is central to avoiding flawed inferences about human history.

In the talk, I tackle philosophical questions about genetic and genealogical lineages in human ancestry research. Epistemologically, one important question concerns the evidential relationship between those lineages. Genetic lineages serve as evidence for genealogical lineages, but can the opposite be also true? Another question concerns whether individuating genetic lineages requires assumptions about genealogy. For instance, to choose what types of genetic lineages are relevant for human ancestry research, do scientists always need to gather genealogical information or make assumptions about past ancestors? Furthermore, I consider whether genetic and genealogical lineages ground a form of ontological or epistemological pluralism. Additionally, one might wonder to what extent that distinction is parasitic on a pluralism about levels of lineages, namely between gene and organismal lineages.

By answering these questions, my goal is to systematically identify the epistemic roles that different notions of lineage play in human ancestry research and to consider some of their ontological consequences. Understanding such epistemic roles can help philosophers and scientists make sense of individuation practices and their limitations. Moreover, as scientists unveil the complexity of individuals and lineages, lineage pluralism might offer a valuable way to start conceptualizing this complexity.

Philosophy on Fire: Evidence, Ethics, and Explanations in Fire Science and Fire Policy

Organizers and Contributors: Aja Watkins, Derek Halm, Jay Odenbaugh, Katie Deaven

Forests burn. Forest fires can be dangerous, destructive, rejuvenating, anthropogenic, naturally-occurring, unmanageable, ecologically necessary, or have various combinations of these characteristics. Contemporary climate change, as well as histories of forest mismanagement by settler colonists in some regions and ongoing human desire to interact with forests in particular ways, have made questions of fire policy both weighty and urgent.

This proposed SPSP session will be (as far as we know) the first time that philosophers of science will take a close look at the science behind these policies. The four included talks will do what philosophers of science do best, applied to fire science: analyze concepts (Halm), evaluate explanations and evidence (Deaven, Watkins), and assess causal claims (Odenbaugh). First, Derek Halm will present on the concept of "fire regime," which is used ubiquitously in fire science and policy but suffers from several issues, including how to apply it uniformly and sensibly to natural, human-constructed, and novel ecosystems. Second, Aja Watkins will discuss problems of conflicting evidence in fire science, specifically concerning whether, when, and how forest managers should use "prescribed burning" as a fuel-reduction strategy. Third, Jay Odenbaugh will address what it means to say that contemporary climate change is a cause of (more frequent, more severe) wildfires, connecting this question to existing work in philosophy of causation. Finally, Katie Deaven will talk about the different types of explanation used in fire science, applying existing philosophical insights about narrative explanations, contrastive explanations, and equilibrium explanations to recommend improvements to the kinds of explanations fire scientists have so far provided.

Each talk is significantly informed by scientific practice. All of us start by meeting fire scientists on their own terms: using their concepts, explanations, and sources of evidence. We focus on questions fire scientists and fire policymakers themselves care about, such as how to integrate diverse sources of evidence or how to apply old concepts to new ecosystems. And we each pursue these projects with an eye towards giving scientists and policymakers tangible recommendations about how they can improve their practices. In addition, several, more traditional philosophical themes are woven through the talks, including: issues with setting ecological baselines for the purposes of conservation, distinguishing between "natural" and "artificial" environments and deciding which such environments are desirable and should be conserved/produced, and integration of different ways of knowing how to manage ecosystems and relations (e.g., Indigenous and Western perspectives).

Overall, we expect that applying classic methods in practice-oriented philosophy of science to a relatively unexplored scientific area — not to mention an area with grave consequences for policy and society — is sure to appeal to SPSP attendees, and we look forward to the opportunity to utilize an SPSP session as a springboard for this promising, new area of research.

Regime change, new tinder, and fire ecology in a novel pyrocene

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A fire regime is fire's intensity, duration between, and commonality in a particular ecological context or community (e.g., California coastal chaparral or Mojave creosote-bursage). "Fire regime" is commonly used in fire ecology to describe a specific ecological community's dynamics. For example, some particular communities may have a fire regime spanning a few years compared to another with a regime over decades or even hundreds of years. Often, the concept is primarily past-facing, which means it picks out historical relationships between fire and vegetation and uses that to make claims about future biotic and abiotic relationships. Claiming a particular fire regime often requires utilizing multiple lines of evidence: pollen in lake bed sediment, charcoal deposits, historical written records, and Indigenous testimony are all used in various contexts.

Fire regimes have become prominent in light of historical fire suppression, such as in the United States, Canada, or Australia, because the purported fire regime of an ecological community may be used to guide or even direct conservation or restoration actions. That is, restoring an ecological community may require the usage of fire, particularly in fire-adapted environments: longleaf pine ecosystems need a frequent fire regime to persist, as other vegetation will likely grow and replace the existing biota with a different regime. Therefore, conserving or restoring a longleaf pine ecosystem requires allowing fires to persist or actively prescribing them to mimic particular fire regimes.

In this talk, I explore tensions with the concept and its use in conservation, restoration, and ecological communities. First, fire regimes are mainly applied to specific ecological contexts, omitting others, such as urban or suburban environments. Second, there is significant disagreement about whether humans should be excluded from the discussion of fire regimes (that is, a “fire regime” is about ecological relationships sans anthropic influences). Third, the concept may not help disentangle debates about the political or social use of fire. Fourth, there is the question of whether the concept is necessary for land management or whether it complicates debates. Finally, since each ecosystem has a different regime, novel ecological communities will have novel—and thus uncertain—fire regimes, making the concept a coarse-grained tool for descriptive or prescriptive management in the face of unknowns.

These critiques interrogate the fire regime concept and its various roles in fire ecology, conservation, restoration, and land management, Indigenous or otherwise. I recommend scrutinizing the concept, particularly its ethical import in the science-policy interface. This is mainly to cast doubt on its prescriptive use, whether in terms of anthropogenic or non-anthropogenic fires. Overall, fire professionals should rekindle the concept for more inclusive environmental debates about the usage of land and what successful conservation or land management is. This should include more discussion amongst fire professionals, the public, and community members, mainly including limitations and unknowns with fire regimes.

Evidence in Fire Science

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Environmental scientists must combine or weigh multiple sources of evidence. For example, environmental scientists often incorporate results from fieldwork, laboratory experiments, computer simulations, and more into their research, and, in doing so, make decisions about the relative importance of each of these sources of evidence. When multiple lines of evidence agree or converge on a particular hypothesis, environmental scientists can argue based on consilience in favor of that hypothesis.

However, sometimes the lines of evidence do not agree. This paper focuses specifically on a case in fire science, concerning whether (and when) to accept or reject the hypothesis "prescribed burning is an effective way to prevent unmanageable, severe forest fires." Prescribed burning is the removal of fuel (e.g., dead plant matter and other debris) from a forest ecosystem via intentionally burning it, usually during conditions more favorable to fire management (e.g., low temperatures, low wind speeds). Preventing unmanageable, severe forest fires is of the utmost importance, especially as climate change and decades of fire suppression policies have made these fires more frequent and more severe. However, lighting prescribed burns is risky, especially because sometimes these fires turn into the very unmanageable, severe fires they were intended to prevent. Consequently, prescribed burning is politically as well as scientifically controversial.

There are conflicting lines of evidence concerning the effectiveness of prescribed burning. On the one hand, there is substantial evidence that "good fires," including those lit by humans, have historically been an integral part of forest ecosystem health, in many locations around the world (the American/Canadian West, Australia, Amazonia). Some of the evidence documenting the history of prescribed burning is physical (e.g., fire frequency and severity can be gleaned from tree rings), but one of the main sources of evidence is the testimony of Indigenous persons, both contemporary and as recorded by anthropologists over time.

On the other hand, recent modeling results suggest that the risks of prescribed burning may outweigh the benefits. Modelers argue that climate change is altering the expected efficacy of prescribed burning, because increased temperatures (and, in some locations, drought) are increasing the likelihood that prescribed fires will turn into or accidentally ignite severe, unmanageable fires. These researchers argue that mechanical fuel removal techniques or ignition prevention are preferable to prescribed burning.

In analyzing this case, I will identify the relevant considerations for weighing these sources of evidence against one another. First, there is a question of whether we expect the past history of successful prescribed burning to be a useful guide to future decision-making, or whether we think that climate change has ushered in an entirely new regime. Second, there is a question of how to weigh two different types of evidence: in this case, testimony about cultural practices and simulation results. I will argue that sociopolitical, rather than epistemic, features of these sources of evidence determine how they should be weighed, thereby identifying a novel role for values in science.

Climate Change, Wildfires, and Causation

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Many climate scientists and ecologists think that anthropogenic climate change causes changes in the intensity and frequency of wildfires (e.g., Abatzoglou & Williams 2016). However, it is very difficult to explain what this causal relationship is. For example, it does not appear to be a singular causal claim such as striking a match causes it to light. That is, it does not appear to be one event causing another event.

In this talk, I provide an account of what this causal relationship might be. First, I deploy the distinction between structuring and triggering causes from Fred Dretske (1988). Suppose we have two causal factors X and Y where changes in X causes Y (or more exactly, changes the relationship between X and Y). A triggering cause is a cause of X. A structuring cause is a cause of the causal relationship between X and Y.

Second, I show how we can understand the causal relationship between climate change and wildfire to be a structuring cause (independently of being a triggering cause). Many scientists view climate change and wildfire relationship through the notion of a vapor pressure deficit (i.e., air's "drying power"; e.g., Zhuang et al. 2021). The vapor pressure deficit is the difference between the saturation vapor pressure and the actual vapor pressure.

As temperature increases, air can hold more water vapor and more is evaporated by plants drying them out. Thus, increase in temperature increases both the amount of water vapor air can hold and evaporation in plants. Rising temperatures thus change both the saturation and actual vapor pressure and thus the relationship between them. This increase in the vapor pressure deficit increases the likelihood of wildfires especially in conjunction with past fire suppression.

Third, we can more carefully distinguish the anthropogenic influences altering our planet. On the one hand, humans can alter the values of causal factors like temperature, which can be triggering causes. On the other hand, they can alter the relationship between causal factors as we see with saturation and actual water pressure.

I finally explore the implications of my analysis for "natural fire regimes." Many people view wildfires as unnatural when humans cause them. For example, a campfire that is not extinguished can create a wildfire. However, here it is the triggering cause that is anthropogenic. Fire regimes can be unnatural in a different way when we manipulate structuring causes.

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Do Explanations in Fire Science Need a Rethink?

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In response to a recent call urging fire scientists to "embrace complexity," there has been a surge in new research projects aimed at enhancing our understanding of fire behavior. These projects include the development of new predictive models, investigations into the resilience of fire behavior under changing climatic conditions, and proposals for revising fire classifications in a way that is sensitive to the different behaviors it may exhibit. All of these seem to be promising steps towards being able to explain why wildland fires burn in the way that they do.

However, rethinking explanations in fire science also involves identifying the kind of explanation that is being sought, prompting philosophical intervention. Fire science, in particular, provides a rich context for thinking about the upshots and weaknesses of different explanatory frameworks. In addition to the scientific and epistemic significance of these explanations, the degree to which we understand fire behavior has serious implications for policy decisions concerning fire management. Moreover, unraveling the relations that result in causally complex events like wildfires requires significant interdisciplinary collaboration and support.

In this presentation, I take a step back to examine the prevalent types of explanations in wildland fire science literature to then make recommendations on steps toward better explanations of fire behavior. I suggest that three types of explanations are commonly found in this literature: contrastive explanations, narrative explanations, and equilibrium explanations. Each of these frameworks offers unique advantages. Contrastive explanations serve as a way of identifying the mechanisms that cause fire regime changes. Narrative explanations provide a broader context for understanding the contingent events that shape a particular fire's behavior, accommodating Indigenous perspectives on fire and land stewardship, which are place-based and sensitive to the relationships between humans, plants, animals, and land within a particular ecological context. Equilibrium explanations, derived from steady-state models, offer insights into the ecological patterns of fire activity and the interactions between vegetation and climate that they depend on.

Along with these unique strengths, each framework has its limitations. By outlining these weaknesses, I aim to propose two types of recommendations where fire science explanations may be improved. First, I outline several considerations focused on the structural features that explanations of fire behavior may possess. Secondly, I advocate for a careful examination of the conceptual commitments associated with each explanatory framework. Through these recommendations, I contribute to the ongoing discourse on fire behavior explanations and hunt for a more robust and comprehensive understanding of fire behavior.

Abstracts of Contributed Papers

(alphabetical by last name of lead author)

Individualized niche construction in population-environment systems

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In recent years, a number of biologists and philosophers of biology have argued that there are various respects in which evolutionary biology ought to focus more on individual differences between organisms and their interactions with environmental conditions. Such approaches, sometimes described as focusing on organism personality or on organisms as agents, are often contrasted with more traditional empirical and theoretical methods that treat individuals as bearers of alternative genotypes or phenotypes.

In his book Evolution and the Machinery of Chance, Abrams argued that we can best make sense of uses of modeling and statistical inference within empirical research in the latter tradition, by viewing evolution as taking place in ""population-environment systems"". This treats a population and its environment as realizing up a chance setup, a complex analog of (for example) a dice tossing setup. Abrams also used arguments from practice and theoretical concerns that conceptions of fitness that treat it as a property of individual organism plays no significant role in evolutionary biology. Abrams view conflicts, prima facie, with endeavors to reform the study of evolution by focusing on differences between individual organisms.

I develop arguments that Abrams' population-environment understanding of evolutionary processes is rich enough to illuminate causal, probabilistic, and conceptual roles of individualized-organism approaches to studying evolution. I explain why within the population-environment conception of evolutionary processes, individual organisms with idiosyncratic combinations of properties can be viewed as elements of realizations of complex outcomes within a population-environment system.

It's particularly illuminating to focus on recent papers by Kaiser and a number of colleagues in biology and philosophy that explores the fruitfulness of various concepts related to individualized niche construction, because this research emphasizes individual differences involving interactions with environmental conditions. I take this research as my starting point here, but suggest that implications of my approach go much further.

Although my arguments don't directly address arguments that traditional approaches to studying evolution are inadequate, I argue that my approach provides a conceptual framework that unifies and clarifies relationships between traditional genotype/phenotype oriented empirical research, and methods focusing on individual-level variation. I suggest that this approach makes it easier to see not only how individualized and traditional empirical research can be viewed as complementary, but more specifically how their relationships can be conceptualized.

Error Repertoires

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To transform haphazard trial and error into enduring learning, memory is essential. Yet negative results, dead ends and missteps often go undocumented and are rarely shared formally in the journal archive. Still, such "negative knowledge" is often preserved in local contexts and informal communication networks, where it guides subsequent research through ad hoc catalogs of domain-specific missteps, or error repertoires (Mayo, 1996). Here, I explore several historical cases and discuss how they function epistemically. Various forms of incongruences — discordant results, theoretical anomalies, and interpretive disagreement — yield new uncertainty and provide researchers a focus for further fruitful research through "troubleshooting," isolating errors, and resolving the apparent discrepancies. However, when investigators find that a former justification was faulty, knowledge grows: a definitive error has occurred. Such errors are not useless, residual byproducts. Rather, knowledge of past error can help in interpreting unexpected new experimental results. Identifying possible sources of error also tends to raise evidential standards for subsequent research (at both observational and conceptual levels). Sometimes, they spur new methodologies to forestall or counterbalance errors. Error — and memory of specific sources of error and of general error types — ironically contributes to progress in science.

Epistemic Actions: A Scientonomic Framework

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In a series of publications, Hasok Chang makes the case for analyzing scientific change through the lens of activity, with activities carried out by agents forming the basis of the scientific enterprise (Chang, 2011; Chang, 2022). The discipline of scientonomy, which works towards describing the general mechanism underlying scientific change, however, focuses largely on the status of theories. Despite scientonomy's inclusion of a few actions such as accepting, rejecting, and assessing, Chang's perspective does not currently fit neatly or comprehensively into the accepted scientonomic ontology. I attempt to rectify this by proposing an action-based scientonomic perspective of scientific practice.

In determining what types of actions ought to be accounted for in such a framework, I begin by recording a list of action verbs from various relevant sources in the philosophy of science, before grouping these action verbs into their four basic categories: 1) relations between epistemic elements, 2) relations of epistemic elements with the world, 3) actions of agents involving epistemic elements, and 4) actions of one agent towards another agent. It becomes evident that often verbs are used in conjunction with epistemic elements, without themselves denoting actions per se. Verbs in category 1 (like, "implying" or "presupposing") and category 2 (like, "defining" or "describing") should not be referred to as "actions", for only agents can

perform actions. Furthermore, it can be shown that the epistemic content of actions in category 4 is fully expressible in terms of actions on epistemic elements, i.e., actions in category 3. As such, I define ""epistemic action"" as an action of an epistemic agent that involves an epistemic element, while discussing the merits and pitfalls of both narrower and broader epistemic action definitions for the purposes of analyzing historical episodes.

It becomes apparent that only a few relevant epistemic actions can be considered to be global actions, that is, available to all epistemic agents trans-historically and universally. For instance, taking a stance of acceptance (i.e., accepting) seems to be a global action, as without this epistemic action no process of scientific change seems possible. In contrast, such epistemic actions as simulating, experimenting, or modeling seem to be local actions since they need not necessarily be part of the repertoire of epistemic actions of all conceivable epistemic agents; such local actions emerge at a certain time and become available to some but not all epistemic agents. I therefore define those epistemic actions that are not available trans-historically to all epistemic agents, but are specific to some time periods or some agents, to be local actions.

The availability of a local action to an epistemic agent amounts to the agent employing the norm that the local action is permissible/desirable. To unearth the mechanism by which local actions become available to epistemic agents, I derive the local action availability theorem, according to which, a local epistemic action becomes available to an agent only when its permissibility is derivable from a non-empty subset of other elements of the agent's mosaic, i.e., from that agent's employed norms and accepted theories.

The emergence of the local action of determining the composition of chemical substances by weighing, as practiced by Lavoisier and his followers, serves to provide an illustration of the theorem. The availability of this epistemic action involves the employment of the norm "It is desirable to determine the composition of substances by weighing". As the local action availability theorem would suggest, I show that this norm is derivable from other employed norms and accepted theories of the relevant agents.

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Revaluations of 'Paiute Forestry': Prescribed Burns as Traditional and Scientific Ecological Knowledge

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Part of what makes the idea of traditional ecological knowledge (TEK) potent for recognizing and repairing injustices against indigenous knowers is that the relationship between traditional and scientific ecological knowledge is dynamic. Consider the use of fire in resource ecology land management. In the 1910s, Aldo Leopold and other foresters dismissively campaigned against burning as Paiute forestry, denigrating and driving out indigenous land management as though it had never existed, as though there was no credible ecological knowledge before settlement. Fire suppression as longstanding federal policy not only failed in reading historical tribal burning practices and in applying that knowledge to settler resource management, but also systematically undercut contemporary tribal ecological knowledge in significant ways. A century later, scientists and policymakers are coming to better understand the dangers of fire suppression, benefits of burns, and the fact that these are not actually new insights but traditional land management practices of many tribes across the continent.

Exciting as they are, however, scientific revaluations of marginalized indigenous knowledge have not necessarily extended to include reparative epistemic justice for marginalized indigenous knowers. Some see TEK as a body of knowledge created, stored, ready to be applied; for others, it is not an archive so much as an ecologically situated way of knowing, a participatory activity in which knowledge and knowers are interrelated. On the former conception, such TEK can be extracted and plugged into settler practices; on the latter, this knowledge cannot be integrated into novel applications unless those who know it are welcomed to the table as epistemic equals. Reparations as relational epistemic repair in the aftermath of historical and persisting willful hermeneutical ignorance of indigenous burning means taking this latter conception seriously. Among other things, it means organizing prescribed burns not only for their overall ecological and economic utility but also and more specifically as epistemic amends, which is to say, as ameliorative expressive acts reaffirming and foregrounding their governance value for contemporary indigenous people. If burning is a complex assemblage of epistemic practices and settler-colonial reactions have perpetrated epistemic injustices against indigenous peoples, how can modern (tribal, settler, and collaborative) burning practices be better?

This project draws on Kyle Whyte and Deborah McCregor on the value of TEK, Robin Kimmerer and Frank Lake on indigenous burning, Jennifer Lackey on epistemic reparations, and my own work on reparative environmental justice to assess the reparative value of fire today. I offer a close reading of the early 20th Century light-burning debate, with particular focus on Leopold's characterization of indigenous ecological knowledge. I then turn to critically evaluate several 21st Century prescribed burn projects (with particular focus on Pacific Northwest and Great Lakes regions) for their reparative potential in both social-ecological and social-epistemic terms.

The Future of the Model Organism Repertoire

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This talk considers two recent and novel uses of simpler model organisms (such as zebrafish, water fleas, and nematodes) in contemporary life science research: as an indicator species in predictive toxicology and as a substitute for rodents and other mammals in translational biomedical research associated with new efforts to foster new approach methodologies (NAMs). We explore the ways in which the model organism repertoire is evolving in association with these domains, including the financial, sociocultural, political, technological, and experimental factors involved in their use. We conclude by showing how these new deployments and the associated model organism repertoire are impacting the epistemic functions of such entities within biology, including what they are taken to represent and how they continue to simultaneously serve as material objects found in nature and constructed for laboratory use.

Using the interventionist theory to unify exploratory and hypothesis-driven activities in biomedical research

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This study focuses on biomedical research, addressing two problems regarding the relation between knowledge formation and plural roles of intervention in research activities. The two problems respectively concern the contribution of exploratory activities to the formation of biomedical knowledge and the relationship between exploration and causal explanation in knowledge formation. This study discusses the usefulness of interventionism for unifying exploration and causal explanation in biomedical research.

My case study is the microbiota-gut-brain (MGB) axis research field. It has been rapidly growing for two decades and is still considered by its practitioners as frontier. The practical context is that mechanism-oriented research has long been considered by biomedical practitioners as the mainstream practice. By conducting mechanism-oriented research, biomedical researchers normally pursue causal knowledge of the phenomena of interest. Mechanism research relies greatly on hypothesis-driven experiments to identify the components such as entities and activities of possible mechanisms. In other words, mechanism research requires experimental interventions on candidate components to test hypotheses. The interventionist theory nicely captures the relation between intervention and the inference of causation from the result of intervention.

Based on these practitioners' views and theoretical backgrounds, a significant part of the practice in the MGB field plausibly aims to search for mechanisms responsible for the phenomena of interest. Meanwhile, I define mechanisms as mechanistic explanations. Therefore, a significant part of the practice in the MGB field arguably aims to develop explanatory knowledge that (1) normally includes causation and (2) can be inferred from the result of intervention.

Several gaps remain in such an assumption. First, the complexity of microbiota-gut-brain interactions generates considerable difficulties in hypothesis-driven intervention and causal inference. Some sort of open-ended, non-causation-searching experiments must be necessary for the knowledge formation in fields dealing with biological complexities such as the MGB field. Second, while the philosophical literature has articulated the existence of open-ended explorations in biomedical sciences, the studies on exploration and hypothesis-driven experimentation tend to be separate. The relationship between these two approaches needs to be revealed. Third, previous studies have not discussed the roles of knowledge obtained from exploration in mechanism-minded biomedical practices.

To fill in these gaps, I approach the problems addressed in the first paragraph by examining some existing versions of interventionism and some causation-related theories. Drawing on a historical survey and a field study, I classify the research activities in terms of the plural involvements of intervention. The empirical results simultaneously reveal an interplay between exploration and causal explanation in the MGB field and disambiguate the term ‘intervention’ in biomedical research. In an interdisciplinary context engaging philosophers and biomedical practitioners, I point out the problem of arbitrarily using the term ‘intervention’. Then, I suggest that while biological and medical studies are distinct in their scales, objects and definitions of intervention, they need not be demarcated because of the interplay between and intertwinement of their contributions to knowledge formation. In conclusion, I propose a generalised framework that accommodates some theories of intervention, causation and exploration useful for an understanding of multifaceted biomedical research.

Stopping Cancer: Understanding Intervention in the Glycolytic Pathway

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This paper examines the case study of inhibiting the glycolytic pathway to prevent the growth of cancer cells. It addresses pathways as causal structures providing explanations through their representations. The paper emphasizes two prominent characterizations of pathways: Lauren Ross’ (2021) and Paul Thagard’s (2003). The paper argues that a specific type of characterization of pathways, along with James Woodward’s (2003) interventionist account of causal explanation, serves as the conceptual framework underlying the case study in question.

Firstly, the paper examines the relevance of the glycolytic pathway for cancer treatment. Glycolysis, a central metabolic pathway, consists of ten sequential steps. During this process, a glucose molecule is broken down into two molecules of pyruvate, releasing ATP molecules in the course. Since the 1930s, following Otto Warburg's discovery, it has been established that cancer cells extensively rely on glycolysis as their primary energy source for growth. Consequently, specific segments of the glycolytic pathway are considered as potential targets for medical intervention, aiming to inhibit the pathway and hinder the growth of cancer cells. The paper highlights glycolysis as a promising pathway for cancer treatment purposes and aims to provide a conceptual framework for explaining and understanding interventions in glycolysis.

Secondly, the paper presents two abovementioned characterizations of pathways and aims to differentiate between three aspects of pathways, namely the ontic, epistemic, and strategic aspects. The ontic aspect pertains to the features of pathways as causal structures found in nature, consisting of a sequence of causal steps. For instance, glycolysis follows a fixed order of steps, initiating with the molecule of glucose catalyzed by a specific enzyme, such as Hexokinase, followed by Glucose 6-phosphate, which is further catalyzed, leading to the subsequent step. The epistemic aspect considers pathway’s representations as vehicles for explanation and understanding. The epistemic aspect of a pathway highlights the abstract nature of the pathway and the connection between a fixed order of steps. The strategic aspect relates to the pathway's investigative strategies, i.e., the expanding/mapping out strategy used to investigate pathways. Usually, scientists aim to create a map, i.e., a network or a landscape, depicting available causal routes. These maps serve as representations of potential and accessible routes for investigating new pathways, as seen in pathway databases such as WikiPathways. The paper aims to advocate for the strategic and epistemic aspects as relevant aspects considering the case study in question, focusing on the representation of the glycolytic pathway. Additionally, the paper outlines Woodward's (2003) interventionist account of causal explanation, serving as a conceptual framework for explaining and understanding interventions in the pathway.

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Uncertainty, Control, and Affective Affordances: Towards an Integrated Enactivist Account of OCD

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In the philosophy of psychiatry, several philosophers have recently developed frameworks to interpret the etiology of psychiatric disorders that depart significantly from those used in the medical field. One such framework, the "enactive approach," has been outlined by Sanneke de Haan in several recent publications. de Haan aims to address the "integration problem" in psychiatry, which is "the explanatory problem of integrating such heterogeneous factors that may cause or contribute to the problems at hand, ranging from traumatic experiences, dysfunctional neurotransmitters, existential worries, economic deprivation, social exclusion, and genetics" (de Haan 2020, 3). Enactivism is one of many theories of 4E cognition that sees the mindedness of organisms as something they enact through engagement with their environment rather than simply from neural processes located in the head. Accordingly, de Haan claims that psychiatric disorders are disorders of the organism-environment system rather than disorders of the brain or body alone.

De Haan has published several papers in which she uses the enactivist toolkit to investigate the symptomology of Obsessive-Compulsive Disorder (OCD). For example, she uses the conceptual resources of ecological psychology to describe OCD as resulting from a nontypical 'field of affordances,' where an 'affordance' is a possibility for action provided by an environment to an organism based on both its characteristics and the abilities and capacities of the organism. While I find de Haan's work to be compelling, I contend that her favoured ecological and phenomenological concepts do not account for the critical role of emotion in the symptomology of OCD. I argue that this shortcoming can be remedied by further fleshing out her account with the notion of the 'scaffolded mind,' which was developed by Kim Sterelny as a contribution to the field of 4E cognition. Sterelny believes that by producing specialized cognitive tools and modifying their environment, human beings create 'epistemic niches' that facilitate otherwise complex cognitive tasks and effective behavioural regulation. Two enactivists, Giovanna Colombetti and Joel Krueger, have built on this account to show how we also produce 'affective niches' that can be characterized in terms of 'affective affordances,' or opportunities for actions that promote emotional regulation, rather than possibilities for action simpliciter. More recently, Somogy Varga has put forward similar ideas in his book Scaffolded Minds: Integration and Disintegration. I will show that once these concepts are integrated into de Haan's framework, it can account for the role of affect in psychiatric disorders in a rigorous manner. I will then show how my modified account of enactive psychiatry can shed light on and further integrate the various neurobiological and cognitive accounts that have been put forth to explain OCD in the medical literature. By doing so, I will show that enactivism can solve the integration problem of psychiatry not only at a general, theoretical level, but also in specific cases of psychiatric disorder.

A Virtue Epistemological Rejection of the Value-Free Ideal

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In laying out standard arguments against the so-called value -free ideal in science , Heather Douglas (2016) identifies what she calls the "descriptive challenge" – the idea that science has always been value-laden and that we should scrutinize those values and replace them with better values if need be – and the "normative challenge" – the idea that the value-free ideal is the wrong ideal to have and that in fact, science benefits from the right values. Here I bolster these challenges with two distinct arguments based on virtue epistemology. First, regarding the descriptive challenge, the point is simply that rejecting the inclusion of values in science, while ignoring, for instance, the reasoning that diversity (in say research subjects) would lead to better science, amounts to epistemic viciousness. Failing to recognize the presence of bad (e.g. racist/ sexist) values in science and/or being closed to corrective (e.g. feminist) values – point to various epistemic vices ranging from promoting epistemic injustice, closed-mindedness, and/or willful ignorance. Second, the predominant rationale behind the normative challenge, for the inclusion of values in science is that science is inextricably embedded in society and hence should be concerned with social/ethical consequences. For instance, the argument from inductive risk goes that the amount of evidence needed for accepting/ rejecting a hypothesis depends on the social costs of false positives/false negatives. I argue that the stakes are not just ethical, but also epistemological. A vast portion of today's science involves knowledge production that is expressly meant for application in some social domain, and the goal to produce such domain-specific knowledge is epistemic. For instance, accepting a suitable hypothesis about the toxicity of a drug is an epistemic outcome although the suitability may be a function of social factors. Given all this, it would be epistemically irresponsible for a scientist to either – as Richard Jeffrey (1956) suggested – not accept or reject hypotheses, or – as Isaac Levi (1960) suggested – to stick to the "canons of inference" and exclude the very relevant social factors. Douglas worries about values acting as a direct reason for choice of hypothesis/ model leading to wishful thinking. She argues that they should only play the indirect role of helping decide sufficiency of evidence. However, I think the epistemic virtues scientists cultivate along their training would ideally allow scientists to include values in a direct role (such as when they "promote the goals of assessment" (Elliot, 2013)) while preventing them from engaging in wishful thinking, an epistemic vice. Finally, scientists should be transparent about the values they include (Elliott and Resnik, 2014) because not only should they care about "self-regarding" virtues such as open-mindedness and avoidance of wishful thinking, but also because they should adhere to "other-regarding" (Jason Kawall, 2002) virtues such as intellectual integrity which promote other people's knowledge acquisition.

How Can We Know if You're Serious? Ethics Washing, Trustworthy AI, and Science Governance in Practice

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This presentation has two primary aims: (1) to articulate and defend criteria for the epistemic assessment of "ethics washing" in AI and (2) to argue that the identification of such criteria can be aided by extending approaches in philosophy of science in practice to the philosophy of science governance in practice.

In the midst of the current excitement about AI, we are witnessing a proliferation of organizational communication about "responsible AI," "ethical AI," and "trustworthy AI." Organizations that research and develop AI systems are publicly touting their commitments to social responsibility and claiming to design and develop their AI systems in accordance with standards of ethics and trustworthiness (e.g., Schiff et al. 2021). These communications – in conjunction with some high-profile ethics scandals – have led many commentators to raise concerns about ethics washing (e.g., Munn 2022). These concerns about ethics washing, in turn, give rise to important questions at the intersection of philosophy of science, AI ethics, and science communication. In particular, how can individuals who are not experts in AI – and who are also not "insiders" of an AI development organization – make informed epistemic assessments of the seriousness with which AI companies take matters of ethics and responsibility in their R&D practices?

In this presentation, again, I articulate and defend criteria for the epistemic assessment of concerns about ethics washing in AI, and I do so on the basis of examinations of emerging governance frameworks for trustworthy AI. Governance frameworks for AI – such as the US-based National Institute of Standards and Technology (NIST) AI Risk Management Framework and the European Union Artificial Intelligence Act – include recommendations for how AI R&D should be organized, including structural recommendations and recommendations for inter- and intra-organizational processes that will purportedly facilitate trustworthy AI. On the basis of examinations of emerging governance frameworks such as these, I argue that informed (yet fallible) epistemic assessments about ethics washing can be made on the basis of organizational features. More specifically, this presentation will identify structural characteristics of AI development organizations that constitute grounds for reasonable assessment that the organization in question is, indeed, serious about matters of ethics and responsibility.

The strategy adopted in this presentation of drawing upon examinations of emerging governance frameworks to articulate and defend criteria for the epistemic assessment of concerns about ethics washing can, I hope, illustrate a meta-philosophical point that merits further attention – namely that the governance of science and technology is a fruitful topic for the philosophy of science in practice.

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The President and the Hurricane

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In September of 2019, a curious (and, as I’ll argue, philosophically rich) series of events occurred at the intersection of hurricane forecasting, politics, and Twitter. Here’s a timeline:

Sept. 1, 2019, 10:51am ET

Then-President Trump tweeted: “In addition to Florida - South Carolina, North Carolina, Georgia, and Alabama, will most likely be hit (much) harder than anticipated.” The official National Hurricane Center (NHC) forecast at the time did not show the track of the hurricane eye impacting Alabama.

Sept. 1, 2019, 11:11am ET

Twenty minutes later, in an attempt to prevent unnecessary panic, the Birmingham National Weather Service (NWS) office tweeted: “Alabama will NOT see any impacts from #Dorian.”

September 4, 2019

Trump held a press conference in which he displayed an outdated map of the forecasted track of Dorian. A small semicircle had been conspicuously added to the forecast track with a Sharpie to show the cone of uncertainty reaching the far southeastern corner of Alabama.

September 6, 2019

The National Oceanic and Atmospheric Administration (NOAA), which oversees both the NHC and the NWS issued a statement which cited a swath of previously-issued advisories from the NHC and said that the “absolute terms” used in the NWS Birmingham’s tweet were “inconsistent with probabilities from the best forecast products available at the time” (NOAA, 2019). Technically, this was true–the NWS tweet suggested that there would be no impacts whereas the NHC's advisory issued that morning indicated a 5-10% chance of tropical-storm-force winds reaching the eastern edge of Alabama.

March 2020

Several months later, an investigative report from the National Academy of Public Administration issued a report that the statement had violated NOAA's scientific integrity policy.

In this paper, I will offer a detailed analysis of this case study in which meteorological predictions, scientific uncertainty, science communication, and politics intertwine. I will do my best to bracket off the questions proper to political science to focus on an interesting philosophical question which this case brings forth: what are the epistemic standards by which a meteorological prediction is judged good or bad? Trump's tweet was technically right–there was a small possibility of some effects from the hurricane. But it was a bad prediction, as the later reports showed. And while the NWS tweet may have been technically false (as per the NOAA statement), it was arguably a good prediction–a good reading of the data trends and, more importantly, a better message for the public.

Following previous work (Boesch 2021), I will argue that the value of predictions includes an important sociological element, since their goal is not "getting things right", but rather yielding life-saving actions, protecting property, etc. Put differently, the features of a good prediction are not determined solely by an isomorphism between the elements within the prediction and various measurements taken at a future point. Good predictions are true, yes–but they are also, importantly, helpful. I close by showing how this insight generalizes to predictions made in other contexts: epidemiology, climate change, medical prognosis, and economics.

Models of Information in Structural Biology

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How are models compared to their target systems, given that these systems are often not directly accessible? A common view links theoretical models to their targets via models of data. Models of data enable researchers to compare relationships among data generated by experiment with relationships among variables posited by theoretical models (Suppes 1962; Mayo 1996). In this paper, I extend recent work criticizing this view (Leonelli 2019; Bokulich and Parker 2021; Karaca 2018), arguing that this view does not adequately characterize many instances of scientific practice. We often cannot construct a model from theoretical principles alone. Further, we want to understand how our model fits not only with a single dataset, but with data from multiple sources and with various theoretical considerations. I thus propose that some cases of modeling are better understood as generating models of information, where 'information' is understood as anything that can serve as evidence for a particular model—including both data and theory. I illustrate how this occurs in structural biologists' use of integrative modeling, a method for determining the structures of large biomolecular complexes by taking into account all available theoretical and empirical information about them. Integrative modeling proceeds via four steps: defining the model representation, which

specifies the variables whose values will be determined by modeling; scoring alternative models according to how well they accommodate the information; searching for models that accommodate information sufficiently well; and analyzing those models. Different pieces of information can be used to design each step (Rout and Sali 2019). Whether information comes from theory or data is not taken into consideration. Instead, two other factors determine how it is used: the strength of the evidence information provides and the security of evidence claims—how susceptible they are to defeat by the failure of auxiliary assumptions on which they are based (Staley 2004)—that can be made on its basis. This paper thus sheds light on the evidential role that data and theory can play in modeling practice.

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What Makes Data Empirical?

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Despite growing scholarship in philosophy of science focused on the nature and uses of data, the source of their epistemic value remains mysterious. Empirical data are useful for learning about the natural world—for constraining theorizing about nature. They are useful in this manner in virtue of being empirical (as opposed to virtual, synthetic, simulated, fabricated, etc.). But what makes data empirical in the first place? I argue that in order to understand what makes data empirical, and therefore to understand what gives them their special epistemic power, we must understand how empirical data are produced by the right sort of causal process, which appropriately connects them to the worldly targets of research. This argument adds a crucial missing piece to existing scholarship on data in scientific practice. The relational

view of data that Sabina Leonelli defends, for instance, does not by itself account for the special status of empirical data. Her view rather helps us identify data as that which is put to use in a certain way. But this approach leaves us with the important question of why those data are suitable for being put to such a use. Similarly, any view that identifies the informational content of data as the key to its epistemic usefulness both opens up the mistaken possibility of using synthetic data with certain informational content to play the role of empirical data in theory testing (which it ought not), and again misses the explanation for the aptness of empirical data for use in constraining theorizing.

I will suggest that the most promising route to providing an account of what makes data empirical, and thus suitable for use in constraining our scientific theorizing, draws on conceptual resources from the process account of causation advanced by Wesley Salmon and Phil Dowe. Rather than claiming that a process account of causation is the way we ought to understand causation generally, I claim that a such an account is helpful for understanding what the 'right sort of connection' is between data and the worldly target from which they originate, in order for those data to count as properly empirical and thus useful for constraining scientific theorizing—for playing their epistemic role in science in practice. The argument thus highlights the perhaps surprising necessity of applying an analysis of causation to the understanding of something in which philosophers of science in practice do take a deep interest: the particular usefulness of empirical data. Moreover, by limiting the application of the approach inspired by the process theory to the elucidation of what makes data empirical, rather than interpreting the theory as a universal account of causation, this argument remains consistent with a broad empiricist skepticism about the metaphysics of causation. I will also clarify the relationship between this account of data and empirical constraints and extant accounts of 'observation' (e.g. Dudley Shapere's) and 'detection' (e.g. Jamee Elder's) using scientific instrumentation, using illustrative cases from astrophysics and cosmology.

Evaluative Categories and their Blinding Effect

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Evaluative categories, such as validity and reliability, are central to the methodological debates about scientific knowledge. If a method is said to be valid, reliable, or accurate, it is assumed to be trustworthy, i.e. to be a useful and efficient tool. An invalid or unreliable method, on the contrary, should not be trusted and its results treated with caution, if at all.

In the present paper, we argue that such evaluative categories can and have been misused. They can have what we call a blinding effect. Though they do help in producing knowledge, they sometimes prevent scientists from using tools (that is, scientific methods) in the most generative way. We propose to study how the use of evaluative categories has (mis)led scientists into over-hastily accepting or rejecting a scientific method and the knowledge produced by it. We compare two historical case studies of concrete uses of these categories, in the biomedical and agricultural scientific practices, with a philosophy of science-in-practice approach.

The first case study looks at quality assessment tools for clinical trials. They emerged in the 1960s and have been widely used by meta-analysts and regulators since the 1980s. For these tools, inter-rater reliability has established itself as a category by which they must all be evaluated. But historical research shows that most and especially early tool-makers did not consider reliability as an evaluative category that their tools should meet. Instead, it was only because of how these tools were misused in meta-analytic and regulatory practices that concerns about reliability were forced upon the standardization of quality assessment. But, we claim, thinking of quality assessment tools in terms of reliability is misleading. It is not only impossible to achieve (a tension between theory and practice), but also obstructs the view of how they could be used differently.

The second case study is the notion of “welfare”, as used by the early animal welfare sciences (1920’s-1950’s, UK). This notion first came into being in the political debate about anti-vivisection, i.e. whether animals should be used for scientific experimentation. But archives at the crossroads between scientific and activist practice show that the very use of this term was intended as a sociological weapon: it was elaborated as a means to protect science against anti-science, anti-speciesist claims. By elaborating the statistical tools that led to the 3Rs principles for humane experimentation (that is, in their very practices of validation), British scientists silenced calls for the abolition of animal experimentation, and translated “animal welfare” into a tool for optimizing animals’ production and use.

Both cases taken together show how evaluative categories shape scientific practices – and how this can be obstructive. In the biomedical as well as in the agricultural case, evaluative categories have not gotten us closer to what the respective practice is good for, but rather contributed to losing sight of the actual goal. This is what we call the blinding effect of evaluative categories in scientific practice.

Normative Work for Concept Functions: From Scientific Concepts to Conceptual Engineering

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Philosophy of science has for some while investigated scientific concepts, often investigating concepts as a further epistemic entity (apart from theories) to understand the workings of scientific theorizing and practice in a certain domain. Yet during the last decade, conceptual engineering has become a trending approach employed in various fields of philosophy (from analytic metaphysics to feminist philosophy). Unlike traditional conceptual analysis, conceptual engineering is about putting forward the best concept for a philosophical task, which can involve the modification or even discarding of existing concepts and the introduction of novel concepts. Conceptual engineering has particular significance in cases where environmental, social justice, and other political aims are at stake, for instance, the concepts of race, gender, disability, torture, climate change, and even food.

This talk will compare the philosophical study of scientific concepts and the practice of conceptual engineering regarding their appeal to the (epistemic as well as non-epistemic) roles and functions of concepts. My agenda is to reveal how insight from conceptual engineering can inform the philosophical study of scientific concepts, and vice versa, especially for normative philosophical purposes.

During the last few years, conceptual engineering has come to appeal to concept functions. The main reason is to argue that revising a concept is not an illicit change of topic, insofar as the concept function remains the same. Based on examples of scientific concepts, I document that philosophers of science have invoked concept functions for several other reasons, some of which should also become relevant to conceptual engineering practice. Apart from understanding the dynamic nature of scientific concepts, this in particular includes using concept roles and functions to resist concept eliminativism and instead normatively uphold concept complexity or concept pluralism. In this context, concept roles are deployed to counter the worry of miscommunication by showing relations among different conceptual variants and means of navigating between them. I indicate how these insights also matter to conceptual engineering for social-political purposes in the case of different concepts of race and gender.

On the other hand, currently conceptual engineers have been more thorough (than philosophers of science during the last decades) in discussing what a concept function or role is. I critically compare the suggestions that concept functions are to be articulated as designed functions, as etiological functions, or alternatively as causal role functions—with an eye toward the normative work that concept functions are to perform in philosophy of science as well as in conceptual engineering. Given that the notion of a concept function or role can be invoked for several legitimate reasons, I conclude that no single construal of ‘concept function’ is sufficient, but instead can depend on the normative task at hand.

Three notions of ‘context’ for neuroscientific investigation

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There is a slowly emerging consensus that neural function is affected by context. Due to the brain’s plasticity, the dense interconnection of neural systems, and the multifunctionality of those systems, which function a part or parts of the brain are performing appears to depend on the circumstances in which those parts function. Thus, it is looking increasingly unlikely that functional explanation in neuroscience can rely on discrete mappings of psychological and behavioral functions to parts of the brain. One of the major questions in philosophy of neuroscience involves what recognition of these facts entails for neuroscientific explanation. Does widespread context-sensitivity, for instance, undermine functional decomposition and mechanistic explanation, or are they compatible?

The notion of ‘context’, however, is still more complicated, due to the fact that functional explanations are explanations. That is, they are theoretical posits made by scientists, and hence come with a range of often implicit pragmatic choices. In this talk, I will discuss three notions of ‘context’, argue that they are dissociable, and claim that proponents of abandoning mechanistic explanation mistakenly lump them together. Properly understood, context is a multi-faceted notion, and functional explanation is (and should be viewed as) a product of a series of contextual descriptions.

The three notions of context I will discuss are task context, scale context, and explanatory context, all of which involve pragmatic choices on the part of neuroscientists. Task context involves the parameters of the behavioral settings in which neural function is studied — these include stimuli, response, and reward parameters, as well as temporal parameters such as learning and memory delay. These are pragmatic because scientists choose behavioral distinctions that they think reveal functional distinctions in the brain. Scale context is the spatial and temporal scale of the brain system one is analyzing. These contexts involve intentional idealizations about where one draws boundaries between the system one is analyzing and its outside influences. Lastly, there is explanatory context, which involves pragmatics in how one describes the explanandum phenomenon.

I argue (i) these notions of context can vary independently of each other, and (ii) that isolating individual situations where mechanistic information is neither required nor sought does nothing to undermine the overall importance of mechanistic explanation in neuroscience. For instance, one can study a task switching paradigm (task context) at the level of functional connectivity in neural networks, or at the level of cross-frequency coupling in individual populations of neurons (scale context). One can study network dynamics (scale context) in the abstract, with no analysis of task setting (task context) to isolate system properties (explanatory context), but one can also study that scale in particular task settings while attempting to discern causal/functional relationships between parts of the network.

Hence, whether one is attempting functional decomposition depends on a host of pragmatic factors, and not on the simple fact of widespread context-sensitivity in function. Philosophical discussion of neuroscientific explanation needs to recognize the different elements of context to avoid oversimplifying its own explanandum.

"Yes | No | Other": Error, Uncertainty, and Ontic Risk in Cross-Cultural Databases

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Cross-cultural work is opportunistic about the underlying ontology of what it studies: cultural groups. This opportunism is pragmatic, supporting vibrant research communities in disciplines across the humanities (e.g., cultural anthropology, philosophy), social sciences (e.g., sociology, economics), and natural sciences (e.g., experimental psychology, archaeology). Alongside the boom in research, however, is an explosion of operationalizations, cross-cutting terminologies, and inferential tools.

Philosophers have long focused on how researchers manage issues of incompatible terminology, operationalizations, and local norms—with Kuhn's (1970) remarks on ""incommensurability"" during theory choice being a notably touchstone. Much work since Structure has focused on how material artefacts, collaborative work environments, and deliberate epistemic processes can facilitate local integration (Star and Griesemer 1989; Potochnik 2011; Nersessian 2022). These are local insofar as they target specific phenomena at the intersection of research teams' disparate interests. But it is the restricted focus that permits processes of mutual coordination—however piecemeal—to facilitate scientific work despite different disciplinary conventions and norms.

Can such coordination be achieved at scale? The rise of large database platforms and infrastructures promises to aggregate disparate and eclectic data into a single source. Cross-cultural work has seen several such databases emerge over the last ten to fifteen years; databases like SESHAT, the Database of Religious History, and the Global Cultural Evolution Database. The goal of such databases is to serve as a clearinghouse for data on cultural groups which can be used and re-purposed for inference and experiment across different disciplines.

While we are familiar with the idea that data have journeys—changing their evidential character through shifts in materiality, place, and empirical situatedness—it is worth scrutinizing large databases given their potential for outsized influence across a variety of domains (perhaps something akin to ""foundational models"" in AI, see: Bommasani et al. 2022). In particular, I want to highlight several ways in which the establishment of database ontologies—through coding manuals, ontologies of data, and coding protocols—functions to reduce error and uncertainty.

My effort here is to taxonomize three kinds of epistemic practices—linked to database ontology and design—that function to reduce error and uncertainty. First, through exposure to data entry (and, perhaps, through oversight, coaching, meeting, reviews, and the like), coders become more familiar with protocols and as a result of this become more certain int he use of a database ontology. Second, that the structure of data entry itself, which only allows for certain kinds of answers, can prevent more structural critiques of database ontology from coming to light. Third, that database ontology can influence how downstream researchers themselves actually collect data, leading to an overinflated sense of certainty about that ontology.

These epistemic practices feed into a framework I have been developing around the concept of ontic risk. As I articulate it, ontic risk involves exposure to harm arising from establishing a particular ontology to be authoritative over a domain of entities. The mechanism both of establishing the authoritativeness of an ontology, and of causing harm, comes when entities are placed into particular categories. As I argue, the epistemic practices of error and uncertainty reduction function to increase the authoritativeness of an ontology—making anomalous data and structural critiques harder to diagnose (Kuhn 1970).

Larger-Scale Databases and Research on Human Evolution

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Paleoanthropology is a highly interdisciplinary field, that requires the use of data collected across several related disciplines to answer key questions about human evolution. Recently, many researchers have called for a move toward broader use of open and larger datasets in human evolutionary studies, guided by the conviction that larger-scale databases are critical for deciphering long-term patterns in human evolution (Henke 2015). The ROCEEH Out of Africa Database (ROAD) is the latest example of such a large-scale research database developed to enhance the knowledge of the human deep past through a data-driven approach (https://www.hadw-bw.de/en/research/research-center/roceeh/digital-resources). This database is meant to "dynamically link scientific data both spatially and temporally", to "integrate geographical, as well as archeological, paleoanthropological, and palaeobotanical data", and to allow "data reuse in ways that were not originally conceived" (Kandel et al. 2023).

Building on the latest developments in philosophy of data (Canali 2020; Leonelli 2023; Leonelli and Tempini 2020), in this talk I will explore several conceptual and methodological issues stemming from the goals that this new database is promised to deliver, including the need for a trans-disciplinary data ontology and the quality assessment of legacy data. Databases that aim to integrate data collected across several disciplines and in different contexts need to develop an ontology of things and relationships to catalog and enter the data. Such categories need to be transversal to allow cross-discipline interactions and can therefore reveal deeper conceptual links and commitments across research fields. The assessment of data quality is also key to preventing information degradation or loss within the process of entering data from the preexisting literature into the dataset. This is of paramount importance in cases when legacy data were systematized in a conceptually different way than the one built into the new dataset. Finally, I will discuss how these issues can potentially constrain the evidential value of data for specific hypotheses about human evolution.

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Colonialism in the Geosciences: A Closer Look at Extractive Practices

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Often people assume that the geosciences, especially paleontology and geology, must operate using extractive practices. The normalization of this assumption has a long history in colonialism and its relationship to scientific research. I start this talk by shedding light on this history and the long-standing relationship between colonialism and the geosciences. I explore what has been termed 'scientific colonialism' by looking at different extractive practices in the geosciences. I separate the extractive practices into four categories: local extractions with no direct influence of colonialism or scientific colonialism, local extractions happening as a result of scientific colonialism, international extractions with no direct influence of colonialism or scientific colonialism, and international extractions happening as a result of scientific colonialism or what has now been termed ‘parachute science.’ My goal in creating these categories is to better understand the effect colonialism continues to have in science production today by comparing whether these effects get magnified either in terms of intensity or magnitude when colonialism is involved. By noting whether unique effects arise from extractive practices that are linked to colonialism, I will be able to give more targeted recommendations to scientists about what ethical practices to follow and to policymakers about what guidelines to enact that will allow for the reliable production of ethical science.

For each category, I will be using an example to highlight different harms that develop from using extractive practices. For local extractions with no colonialism, I focus on environmental and aesthetic harms which are very clearly highlighted through coal mining in the Appalachia mountains. For local extractions that happen as a form of colonialism, I focus on the harms that arise out of ignoring or disrespecting a community’s agency as is often the case with geological extractions on indigenous lands. For international extractions with no influence of colonialism, I look outside of geology and paleontology toward space resource extraction and focus on environmental harms given that some social dimensions are taken away because there is no current interaction with other humans. Lastly, for my fourth category, namely international extractions happening as a form of colonialism, I focus purely on fossil extractions happening in Brazil when the fossils are then taken somewhere else either legally or illegally. For this last category, I list some epistemic as well as non-epistemic harms but focus mostly on why extractive practices in this category can often be a form of injustice.

I conclude my talk by drawing more general philosophical lessons about when the harms produced by extractive practices outweigh the potential for knowledge creation and therefore should be rebuilt or avoided.

Model Systems Across the Lab and the Field: Organismal Samples

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This talk introduces and investigates organismal samples as a type of model system developed for use in both laboratory and field biology settings.

Philosophers and biologists have long noted that the assumptions and pragmatic requirements embodied by model systems used have a strong bearing on how instruments and methods are developed, and therefore on what data are collected. Findings obtained on laboratory model systems do not necessarily or easily map on findings acquired in the field, since the characteristics of the former are adapted to life under controlled conditions (Ankeny and Leonelli 2020). Conversely, models, methods and instruments developed for fieldwork are primarily geared towards robustness vis-à-vis unpredictable and complex environmental stressors such as weather, biodiversity and human interventions (Kohler 2002), making them ill-suited for laboratory work. Extracting field-related insights from laboratory models, and generally comparing and integrating data generated across these settings, remains an epistemic challenge for biological research (Steel 2007; Baetu 2016).

In the first part of the talk, we focus on pest-plant interaction studies as a domain of biological research whose objectives fully depend on effective alignment between methods and results across lab and field. We present a specific example of such work: the Haly.Id project based in Northern Italy, that aims at monitoring and eventually controlling the brown marmorated stink bug Halyomorpha halys (H. halys) – a highly invasive pest that feeds on fruits and seriously harms production in southern Europe, the United States, and eastern Asia (Bariselli, Bugiani, and Maistrello 2016; Giannetti et al. 2022; Ferrari et al. 2023). A key method Haly.Id researchers developed to study H. halys damage consists in first allowing the bugs to infect pears in the field, then transferring the fruits to a lab where they are regularly imaged to document the progressive flesh decay caused by bug punctures. We reconstruct the ways in which pears are grown and modified to fit the investigation. Since they are developed in the field yet destined to laboratory work, pears withstand significant changes in their surroundings, like transitioning from variable to stable environmental conditions and from low to high degree of researchers' control, including exposure to highly standardized instruments and procedures. We illustrate how researchers have tackled this challenge by developing methods to grow and handle the pears so that they reliably exhibit specific phenotypic traits and behaviours – in other words, we claim, making the pears into organismal samples that straddle the lab and the field.

In the second part of the talk, we argue that such efforts have significant implications for the representative power and epistemic function of organismal samples within scientific investigations. We discuss the key characteristics of pears as model systems and compare them to other material models in biology such as model organisms and field specimens, thereby singling out some advantages and problems in their adoption, development and interpretation for research. We conclude by reflecting on the broader implications of focusing on organismal samples for existing understandings of biological research practice, most of which is centered on laboratory settings.

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How HPS can enhance scientific learning and inquiry today: the case of electrolysis

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I argue that the history and philosophy of science (HPS) can provide useful inspiration and material for the improvement of scientific education and research, which will be illustrated through the case of the electrolysis of water and simple salt solutions.

My thinking begins with a pedagogical malaise: a survey of general chemistry textbooks in English at school and university levels show that they present various and mutually contradictory accounts of the decomposition of water by electrolysis, especially about whether the hydrogen and oxygen gases originate from pre-existing H+ and OH- ions taking and giving up electrons, or from the direct reduction/oxidation of H2O molecules. Interestingly, in South Korea all the school textbooks uniformly give one of the accounts found in English-language textbooks, without an explicit refutation of the other accounts. Concerning the electrolysis of aqueous salt solutions, most of the textbooks in both languages give oversimplified views of competing reactions based on standard reduction/oxidation potentials. This confusing and unsatisfactory situation raises many questions about the nature of scientific knowledge, the aims of science education, and the role of philosophical and historical thinking in scientific learning and progress.

I propose a general principle: modern pedagogy can be helped by looking back to the time when the topic being treated was a matter of cutting-edge debate among researchers. This will sharpen critical awareness, recover lost knowledge from past science, and encourage modern extensions of such recovered knowledge. (These are the three main benefits of “complementary science.”) In the case of electrolysis, if we go back to the mid-19th century we find J. F. Daniell and W. A. Miller arguing that the hydrogen and oxygen gases generated in the electrolysis of water are products of secondary reactions following the initial decomposition of the added electrolyte necessary to facilitate the electrolysis. This account makes a lot of sense in itself, and it also produces a naturally unified view of the electrolysis of water and aqueous salt solutions. The Daniell–Miller account was dominant for a time, but became lost in the excitement generated by the ionic theory of Svante Arrhenius arriving at the end of the 19th century. The vast majority of modern textbooks in our survey ignore it, and this constitutes a clear loss of knowledge. Recovering the Daniell–Miller account also generates many new questions, around which new scientific knowledge can grow.

It is understandable that textbooks present sufficiently simple pictures that students at each level can handle; however, this should not be done in a way that shuts down questions. Students should be made aware that textbook accounts are only models, and encouraged to go beyond such models. The plausibility of this recommendation has been shown in a pilot study with secondary school students in South Korea.

This talk draws from my collaboration with Katy Duncan (University of Cambridge), and Seoung-Hey Paik and Kihyang Kim (Korea National University of Education), through which we conducted the textbook survey and pilot teaching study, and sharpened most of the ideas presented here.

Evolution's Bonus: Androcentrism & Lloyd's Feminist Account of Female Orgasm

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Evaluating various hypotheses for the evolution of female orgasm, Elisabeth Lloyd (2005) found that nearly every theory was unduly influenced by androcentrism, adaptationism, or both. She contended that the only empirically adequate explanation was the byproduct account, which postulated that female orgasm was not an adaptation per se, but rather an "embryological bonus" for female primates and cisgender human women (p. 110). While Lloyd maintains that her account is genuinely feminist—in lines with Helen Longino's (1990, p. 188) practice-based approach of "doing science as a feminist"—the byproduct account has been criticized as androcentric for normalizing sexual agency as masculine and passivity as feminine (Fausto-Sterling et al., 1997; Gannett, 2007). Moreover, the byproduct account was originally postulated by Donald Symons (1979), who troublingly defines orgasm as a male-typical behavior, rendering female orgasm an anomaly (Lee, 2013). According, one might wonder if Lloyd's account has a latent androcentrism (see Wakil, 2021), given that "Symons's definition of homology—by claiming orgasm is a distinctly male trait and using male orgasm as the proxy for characterizing female orgasm—is dripping with the androcentrism Lloyd defines as 'not treating female sexuality as autonomous from male sexuality or male reproduction' (2005, p. 236) and 'assuming females' response is like male's response' (2005, p. 237)" (p. 2320).

This paper makes two arguments related to the values embedded in Lloyd's account: First, I contend that, while Symons and Lloyd both avoid adaptationism, only Lloyd's account evades androcentrism because, unlike Symons, she does not gender the standard of human sexual development as masculine. Accordingly, I recommend a distinction between Symons's androcentric byproduct account and Lloyd's "bonus" alternative. Second, I argue that Lloyd's account supports feminist values, not only because it avoids pathologizing female sexuality, but also because her work has guided various research communities toward knowledge of sexual liberation. I do so by situating Lloyd's bonus account among other non-androcentric explanations of female orgasm, including those of Sherfey, Hrdy, and Gould. My analysis contrasts different influences of androcentric values guiding evolutionary theorizing in terms of naturalizing vs. normalizing: while the androcentrism in more adaptation-based explanations involves naturalizing gender hierarchies, other non-adaptationist explanations are nonetheless androcentric in so far as they pathologize female development as an arrested version of "normal" male development.

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Epistemic self-servingness – The corrupting effects of seeking knowledge

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The negative influence of non-epistemic values, commercial interests and other external factors on the integrity of research and publication processes is a well-established topic in various meta-scientific fields. It is commonly assumed that violations of research integrity ultimately result from external motivational factors, while epistemic interests are understood as non-corruptive and even fostering integrity. This assumption can be found in various approaches used to analyze and explain the corruption of research and publication processes (value neutrality (Carrier, 2013), conflicts of interest (Thompson, 1993), epistemic corruption (Kidd, 2020) etc.). The received view leaves a conceptual blind spot regarding the possibility that scientists' epistemic interests might have a corrupting influence on their behavior, too. In this talk, I explores this hitherto neglected issue in detail. I will argue that (i) the search for scientific knowledge involves an inherent risk of corrupting the epistemic and moral integrity of scientists, (ii) the corrupting effect of the search for scientific knowledge is predominantly self-serving with regard to the epistemic ends of scientists, and (iii) currently employed frameworks fail to explain epistemic self-servingness because of their preoccupation with value neutrality, commercial conflicts of interest, and epistemic vices.

The talk is structured as follows: section 1 provides a series of examples of scientists behaving morally objectionable, yet efficiently with regard to their own epistemic ends. In order to capture the distinctiveness of its underlying motivation, I refer to this type of behavior as "epistemic self-servingness." The examples include efforts to exert influence on policy-making (viz., recent proposals for the introduction of bioliberal regulation concerning CRISPR/Cas-based genome editing; Christian, 2022), neglect of parental responsibilities (Boehnke and Hao, 2023), neglect of individual health, as evidenced by rising rates of burnout among scientists (Woolston, 2021; Abraham et al., 2024), and resistance to implement institutional mechanisms to react to harassment in academia committed by influential and esteemed scientists (Witze, 2018). Section 2 then compares the explanatory power of established approaches that reconstruct these cases with a focus on conflicts of interest, violations of value neutrality, epistemic vices, and various forms of discrimination. It will be argued, that these approaches provide no or only partial motivational explanations, since they categorically externalize corrupting influence and thereby implicitly rule out that the striving for scientific knowledge itself can corrupt epistemic agents. Section 3 then explicates the concept of epistemic self-servingness, makes a case for its explanatory value and outlines a motivational model that explains the corrupting influence of epistemic interests in terms of science's utility for satisfying epistemic desires, forming personal identity, and achieving social status. Section 4 briefly provides an overview on various types of unprofessional behaviors that should be analyzed as being epistemically self-serving. Finally, section 5 addresses possible reasons for resistance to the idea that (prima facie legitimate) epistemic interests might corrupt scientists, covering psychological defense mechanisms (i.e. self-protection, retention of motivational incentives), conceptual exclusion by definition, and a commitment to the values of enlightenment.

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A Biological and Philosophical Comparison of Early Fetal Tissue and Tumors in the Post-Roe Era

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The collapse of Roe v. Wade in June 2022 has had many consequences for reproductive rights in the United States, especially increasing restrictions on early abortions under 12 weeks. A New York Times editorial in early 2023, however, questioned our popular understanding of what is actually removed in an early abortion. In an article entitled “Early Abortion Looks Nothing Like You’ve Been Told”, Erika Bliss published images of isolated aborted fetal tissue from five weeks to nine weeks. These photographs did not show the “mini baby” imagery that is often conjured, but, rather, clumps of undifferentiated tissue.

While this raises many ethical qualms, it also opens the door to different questions related to the epistemology and ontology of biology. What is an early-stage fetus? More specifically, how does the early fetus and its maternal home compare to other foreign bodies hosted within a patient, such as a tumor, and to what extent? What attributes could allow them to be viewed and treated similarly?

In this paper, I contend that early-stage fetuses and tumors are biologically and philosophically similar. Therefore, I will argue that a host has no more moral responsibility to an early-stage fetus than they do a tumor. I will begin with summarizing the opposing position, that fetuses are separate biological and philosophical entities with agency, unlike a tumor, and must be treated as individual patients. I will then refute this claim by defining “early-stage fetus” and “tumor” and describing their biological similarities across different subdisciplines, including biomechanics, developmental biology, symbiosis, immunology, and medicine. In biological terms, I will also describe fetal tissue and tumor uniqueness as exceptions to “inclusive fitness theory” due to their somatic self-interest (Stencel & Suárez, 2021).

Moreover, I will synthesize existing arguments regarding the lack of philosophical and historical separation between foreign body and host. This evidence will demonstrate that, similar to a tumor, fetal tissue cannot be treated as its own entity relative to the host, refuting the popular “container theory” and rhetoric of the “fetal patient” (Kukla & Wayne, 2018; Lyerly et al., 2008; Nuño de la Rosa et al., 2021). I will also explain why this does not actually contradict the immunological recognition of the fetus or tumor as a separate entity, bridging the gaps between biology and philosophy and integrating the above ideas into one cohesive understanding.

With both biological and philosophical perspectives, I will also erode the possibility of autonomy and agency for both the early-stage fetus and tumor both in isolation and within their hosts, as well as in medical and social contexts (Chigira et al., 1990; Kukla & Wayne, 2018). Finally, I will discuss the social and political implications of this argument as they pertain to abortion rights in a post-Roe world, especially in the restoration of autonomy and agency to the one true patient, the host.

Sensational cognitive claims

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In public reports of the scientific study of the brain and mind, sensational claims, or claims that appeal "both broadly and salaciously to members of society at large, usually by dealing in base matters of enduring human interest" (Havstad 2022, 295), are regularly made. Rats can drive tiny cars (Klein 2019). Plants remember if you mess with them (Klein 2016). AI has become sentient and should be treated as such (De Cosmo 2022). However, while these are examples of sensational cognitive claims, they do not seem to be sensational for the same reason. Likewise, they do not all receive the same reception by the public or the relevant scientific community. Given these differences, how should we account for these claims?

In this talk, I provide a preliminary investigation of this topic. I argue that answering this question requires that we disambiguate ways that claims can be sensational. Namely, the content of sensational claims can reflect different aspects of the science:

(1) About a study and what researchers do in it

(2) About the extension of a study's outcomes

(3) About the normative implications of a study's outcomes

While it need not be the case that a sensational claim fits only one of these aspects, they provide a starting point for addressing the different ways in which claims will be received by the public and the scientific community. The difference in reception informs my analysis of when sensational claims are inappropriate: inappropriateness can be measured in terms of the degree to which they foment a disparity between how the public views the research and how those in the community view it.

In this talk, I disambiguate sensational claims via a case study: the infamous "memory transfer" research program of James V. McConnell, in which he studied whether memories could be transferred from one organism to another (Colaço 2018). This work drew significant response from the public. It also drew sharp criticism from scientists, many of whom came to view it with "collective embarrassment" (Setlow 1997, 189). I also discuss how my characterization of sensational claims and my distinctions inform my investigation of the inappropriateness of these claims. I conclude by reflecting on further tests of my position, and I discuss why the brain and mind sciences are a domain where we ought to disambiguate what we mean when we talk about sensationalism.

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Scientific Practice & the Metaphysics of History

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Paleoscientists adopt complex, opportunistic ‘methodological omnivorous’ strategies to gain epistemic traction on the deep past. Does this practice tell us anything about the metaphysical nature of history? I think it might do. Some metaphysicians of science have argued that, under the right conditions, features of scientific practice can inform our metaphysical picture, including arguments for entity realism (Khalili 2023), against biology having a general structure (Waters 2007), for Processualism (Dupre 2021) and for views on individuality in biology (e.g., Kaiser 2018). I take such arguments to involve two steps. First, generating what Kirsten Walsh and I have called an "ontic-driven" explanation of scientific method (2018). Such explanations make sense of the strategies scientists adopt by appeal to features of the systems they are interested in understanding. Second, we need some kind of abductive, inductive, or transcendental argument from that ontic-driven explanation to a metaphysical view about the nature of those systems. Examining Nick Butterfield’s arguments in favour of a biota-first explanation of the geochemical-biological systems that arose through the late Ediacaran to the Devonian (2009, 2018), I identify two systemic features challenging scientific knowledge. One I call ‘erasure’: the incompleteness of the various records scientists may draw on. Another I call ‘loss’: the disappearance of the entities, dynamics and processes that Butterfield seeks to understand. I’ll argue that although ontic-driven explanations appealing to erasure fail to have metaphysical consequences (because they too-far intertwine epistemic with metaphysical issues and lead to metaphysical underdetermination), there is metaphysical hay to be made from the grass of loss. In particular, I’ll suggest that loss implies historical processes whereby new entities and dynamics arise and disappear in patchy, often piecemeal ways. This potentially supports metaphysical views such as processualism and the disunified/dappled views favoured by the “Stanford School” (Dupre 1993, Cartwright 1999), but with an added historical dimension, focusing on the processes by which the world becomes disunified.

Evidence in Evidence-Based Management: Challenging Weber et al. (2023)

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Evidence-Based Management (EBMgt) is an approach to management (mainly developed by Barends and Rousseau 2018), according to which managerial decisions, in order to be more

effective, should be based on ‘the best available evidence coming from different sources’ (Ibid. 2). Evidence is defined as a two-place relation between a piece of information and a given claim that needs to be supported/contradicted by that information (e.g. “intervention x will produce the intended outcome y”). Evidence can come from four sources (Ibid. 2):

The scientific literature (e.g. results of meta-analyses or randomised controlled trials);

The organisation (e.g. internal data of the company, like business indicators);

Practitioners (e.g. the professional experience or judgement from managers or consultants);

Stakeholders (e.g. the concerns and the subjective experience of the employees).

Weber et al. (2023) claim that EBMgt leaves room for methodological mismatches and biases when gathering evidence. To improve EBMgt, they propose EBMgt+, in which evidence is defined as a three-place relation between a piece of information, a given claim and a method. The method is a procedural component that ‘describes how the information should be collected and reported’ (Ibid. [1]) so as to count as evidence with regard to a given claim. More specifically, leaving aside evidence of type (a), Weber et al. (2023) claim that, whenever they are possible, probability sampling methods (PSMs) are the best methods to gather evidence of types (b), (c) and (d).

We agree that a method is important. However, we argue against the identity relation between ‘method’ and ‘PSMs’. While we agree that this relation holds most of the time, our stance is that (T): even if PSMs are possible, they are not always the best methods to gather evidence (b), (c), and (d). We substantiate (T) by referring to a case of harassment (example 1) – to show that in some cases PSMs provide insufficient evidence –, and to a case of an innovative decision (example 2) – to show that PSMs do not always provide the relevant evidence. We overcome these shortcomings of EBMgt+ by proposing a new characterisation of evidence, this time between a piece of information, a given claim and a context. The context includes: agent(s) – cf. Munro (2014, 51) –, assumptions, a method, values and some factual conditions (e.g. legal requirements, time constraints, etc.). Importantly, the best method for gathering evidence is determined contextually, i.e. in relation to the elements of the context. By breaking the identity relation between ‘PSMs’ and ‘method’, our new characterisation of evidence extends the domain of application of EBMgt+ also to those circumstances in which PSMs are not preferable. Moreover, it also increases the feasibility of EBMgt+.

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Decoding Sustainability: A comparison between Life Cycle Assessment and the Institutional Compass

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Here we aim to contribute to the methodology of science by undertaking a comparative analysis of two methodologies used for sustainability assessments, namely Life Cycle Assessment (LCA) and Institutional Compass (IC), with a specific focus on the levels of accessibility to information that they promote.

On the one hand, LCA is a method commonly used when evaluating the environmental impacts of a system (product, process, or activity) throughout its entire life cycle, going from raw material extraction to end-of-life disposal. LCA aims at providing a comprehensive perspective on the environmental aspects associated with a particular system, helping to identify potential environmental challenges. While LCA is one of the most accepted and employed methods for sustainability evaluation, it faces important methodological challenges concerning the quality and availability of the data that is used, incommensurability between LCAs, and the lack of accessibility for non-expert audiences (Cf. [1], [2], [3]).

On the other hand, IC is a multi-criteria decision aid that aims at holistically evaluating economic, social, and environmental data about a given system considering the specifics of the region in which such a system is produced, used or discarded (Cf. [4]). Economic, social, and environmental data are amalgamated into a single reading represented as an arrow on a circle (like an ordinary compass). The IC offers unique advantages such as accessibility without requiring specialized expertise, multi-valued and multi-time scale considerations, social inclusivity, comprehensiveness, intuitiveness, and proper objectivity.

By conducting a comparative analysis, we aim to contribute valuable insights into the epistemic implications of employing LCAs and ICs in sustainability assessments. Our main claim here can be summarized by the following two observations:

* An examination of the accessibility criteria endorsed by these methodologies is essential to enable well-informed decision-making regarding the sustainability of given systems. This pertains not only to industry stakeholders but also extends to the impacted society and other pertinent parties.

* Several methodological challenges in LCAs stem from a specific methodological inclination: giving precedence to expert testimony regarding sustainability criteria –and neglecting the input from other relevant stakeholders. IC, however, presents an alternative methodology that facilitates the inclusion of testimony from non-experts and when doing so, allows for more adequate sustainability assessments.

To do this, we proceed in four steps. First, we introduce general criteria for reliable sustainability assessments. Second, we discuss both methodologies, LCA and IC, as well as their advantages and challenges. Third, we illustrate the use of both methodologies with a case study from molecular and materials science. Finally, we draw some conclusions.

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Promises and perils of big data: Philosophical constraints on chemical ontologies

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Chemistry is experiencing a paradigm shift in the way it interacts with data. So-called "big data" is collected and used at unprecedented scales with the idea that algorithms can be designed to aid in discovering novel molecules and materials. As data-enabled practices become ever more ubiquitous, chemists must consider the organization and curation of their data, especially as it is presented both to humans and, increasingly, to intelligent algorithms. In data science, organizational schemas often reflect data ontologies, that is, systems for representing relations among objects and properties in a domain of discourse. As chemistry encounters bigger and bigger datasets, the ontologies that support chemical research will likewise increase in complexity. The future of chemical research will be shaped by the choices made in developing big data chemical ontologies. How such ontologies will work should, therefore, be a subject of significant attention in the chemical community.

Hundreds of chemical databases—each with its own schema—have arisen ranging from field-specific, mid-sized databases to large-scale repositories. As a result, many in the chemistry and chemical information communities are raising the alarm, calling for ontological discussions and, specifically, a shift in time and energy towards the development of a universal ontology. Unfortunately, these discussions are often limited to chemists and data scientists. We believe this is a mistake, and we share how history and philosophy of science can contribute to advancing chemical research by illuminating paths forward through the big-data thicket.

We demonstrate an interdisciplinary approach that draws on the long history of philosophers of science asking questions about the organization of scientific concepts, constructs, models, and theories. In a chemist-initiated collaborative work between chemists and philosophers, we illustrate for chemists how the "blooming, buzzing confusion" of chemical ontologies is merely a feature of advanced chemical thought, and an often-desirable one at that. We ultimately advocate for a shift in time and energy away from the quest for a universal chemical ontology and towards developing context-sensitive pluralistic ontologies in collaboration with philosophers.

In discussing our collaboration, we share elements of our approach for communicating to chemists the utility of history and philosophy of science in addressing contemporary chemical problems. In our context of chemical ontologies, we start by demonstrating to chemists interdisciplinary collaboration's historical value. We overview chemistry's historical leadership in data use and management as well as the disciplinary overlap between philosophy and chemistry, especially regarding chemical classification. We then craft an argument for ontological pluralism with chemist-accessible examples and reasoning. Specifically, we draw on historical precedent for pluralistic conceptions in chemistry, problems with monism in existing chemical ontologies, example ontologies from materials chemistry, and an example of existing pluralism in nanochemistry.

In closing, the presenting author—a graduate student in chemistry—will share how engaging with these arguments has shifted her thoughts on pluralism in chemical ontologies and impacted her research methodology.

Back to the Problem of Space

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One of the major problems generated by the discovery of non-Euclidean geometries was the so-called 'problem of space' — the problem of demarcating which mathematical geometries are candidate physical geometries. By the end of the nineteenth century, a broad consensus had developed around a purported solution: only the constant curvature geometries were possible physical geometries. In this paper, I want to consider what difference it might have made if the significance of affine transport (along straightest lines), independently of metric transport (along shortest lines), had already been appreciated at this time. This is not entirely fanciful — most if not all of the mathematical and conceptual resources were already available in this period, and it appears to have been a historical accident that a non-metrical notion of affine transport1 was only worked out later, specifically following the development of general relativity. Furthermore, as John Stachel has argued, the absence of this notion was largely responsible for the torturous nature of Einstein's path from special to general relativity. With this in mind, I will consider what difference the notion of non-metrical affine transport could have made to the nineteenth century problem of space.

Recall that the nineteenth century consensus was that only the constant curvature geometries were candidate physical geometries. The argument for this conclusion — developed first by Helmholtz and then made mathematically rigorous by Lie — was based on the idea that the possibility of physical geometry depends on the existence of rigid objects (such as rulers and compasses) which can be moved around without changing their dimensions. If such free mobility was impossible, so the reasoning went, then we would not be able to measure spatial intervals at all. And as only constant curvature geometries have the right congruence structure to capture such free mobility, only those geometries were candidate physical geometries.

This purported solution to the problem of space clearly depends on the notion of metric transport — moving a body such that all of its parts maintain their relative distances. What is out of sight is the significance of affine transport — moving a body such that all of its parts move along parallel lines. Once this is appreciated, however, it quickly becomes evident that the general affine transport of an extended figure is only possible in a flat geometry. If the curvature of space differs from zero, we immediately encounter geodesic deviation. I will argue that, when considering the possibility that space might have a constant curvature, the philosopher-physicists of the nineteenth century could have recognized that the existence of tidal forces would make absolute motion — motion relative to space itself — detectable. Although some might have seen this as indicating that space had to be described by flat Euclidean geometry after all, others might have seen this as pointing to a new way to detect motion relative to the ether.

Epistemic Oppression in Open Data

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Advocates for open data argue that with more and bigger data openly accessible, science and society will benefit, but they typically omit to specify how exactly those benefits will or can be justly and fairly distributed. In this paper, we argue that if database technologists pursue open data efficiencies through scale, then by default they will likely advantage already dominant groups due to standard mechanisms of cumulative advantage and organizational path dependency. Put differently, they will likely sustain or exacerbate epistemic oppression (Dotson 2014). We illustrate how epistemic oppression can arise with a case study from biodiversity science given colonialism between countries in the Global North and South (Chan et al. 2019, Kaiser 2023).

In particular, the standards and governance practices adopted by open data infrastructures persistently expose marginalized social groups to higher costs or harms and lower benefits than privileged groups (DiPrete and Eirich 2006). For particular knowledge infrastructures, those standards and governance practices become “locked in” by mechanisms of organizational path dependence (Schreyögg Sydow 2011). This locked in cumulative advantage leads to continuing and widening advantages for those stakeholder groups who already enjoy social privilege and prestige within relevant knowledge or information communities, even in contexts of open science where software and knowledge are available in the public domain for unrestricted access and use. Ultimately, even open data infrastructures advance privileged stakeholder groups, harm unprivileged groups, and coerce unprivileged groups to continue their participation in the unfair institutional governance in attempts to at least minimize the degree of separation between the two two groups.

We make two general conclusions. First, governance standards and practices are important aspects of social epistemology for open data infrastructures and for science more generally. They warrant further study especially as they relates to epistemic oppression and how it implicates moral and political values. Second, to better characterize relations between social mechanisms and epistemic oppression, we need strategies to operationalize the latter construct to enable empirical studies that can document the degree to which open data projects cumulatively advantage dominant groups. We suggest that more general theories of oppression have conceptual tools to help do so (Cudd 2006; Young 1988).

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What Is Epistemic Intimidation, and What Should We Do About It?

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Scientists investigating and stressing the importance and recalcitrance of social, health, and environmental problems, such as anthropogenic climate change, COVID-19, or the issues of sexism and racism, have increasingly come under attack by right-wing populists, even in democratic countries. We argue that this creates an atmosphere of systematic intimidation that affects the dynamics of scientific practice and, thus, epistemic progress. Systematic means that it becomes normal for scientists in targeted research fields to fear that they will face attacks when they defend certain positions or address certain research questions. We introduce the concept of epistemic intimidation, denoting practices of such systematic intimidation, such as harassing, bullying, threatening, marginalizing, ridiculing, etc., which specifically target a person in their capacity as an epistemic agent. We are especially interested in the epistemic intimidation of scientists or researchers, i.e., those who contribute to the production of scientific knowledge. We will spell out how this systematic intimidation of scientists comes at a high epistemic price.

This talk is part of a broader project, in which we aim to (1) analyze strategies of systematic intimidation in science and provide a taxonomy of them, (2) identify and classify the epistemic effects of such systematic intimidation in science, and (3) explore how to deal with these problems. Here we focus on (3), i.e., on how affected scientists, scientific communities, scientific institutions, and the public can effectively counteract epistemic intimidation.

In order to answer this question, we distinguish between individual, communal, and institutional responses. We examine how research institutions (such as departments, universities, and research centers), as well as the institutions that support science (such as professional associations and funding bodies), can (and should) prevent and counteract epistemic intimidation. We will present some promising guidelines for dealing with epistemic intimidation, such as those developed by the American Association of University Professors (AAUP 2017), as well as others suggested in the literature (Denise 2015; Branford et al. 2019). Moreover, we will discuss the requisite changes in society in order to prevent epistemic intimidation.

With our analysis of epistemic intimidation, we aim to provide a better understanding of the negative impacts of the attacks on science and scientists, which are especially prominent today (Branford et al. 2019). In so doing, we will devise new conceptual resources, which we hope will stimulate fruitful discussions and contribute to the acknowledgement of the phenomenon in both scientific and public institutions.

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A pink lie: How France fell in love with a placebo

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Spasfon is a French invention. It is the brand name for the chemical compound called phloroglucinol. Often delivered in the shape of round sugar-coated pink pills, it is commonly prescribed and sold in France for a variety of abdominal, gynaecological, urinary tract pains, and for painful “spasms” during pregnancy. Spasfon is one of the most sold pharmaceutical drugs in the country; between 2002 and 2005, Spasfon went back and forth between the 5th and the 7th place of the most sold drugs . In 2021, 25.3 million units of phloroglucinol have been prescribed in France . Phloroglucinol is also disproportionately prescribed to women. In 2021, 72% of prescribed units have been prescribed to women in France. Despite all of this, you may have never heard of Spasfon or phloroglucinol as it is not available in most country. As of 2024, it does not seem to be authorised in the United States, Canada, the United Kingdom, Germany, or Austria, to cite just a few. My talk will focus on the French case. One reason for this situation has probably to do with the weak state of the clinical evidence in favour of the drug efficacy . This paper sets to explain how one of the most prescribed and sold pharmaceutical drugs in France – Spasfon (phloroglucinol) became so successful in the absence of solid scientific evidence. Integrating an empiricist feminist philosophy of science, the epistemology of ignorance and an engaged approach to history of medicine, my goal is to understand how this case of ignorance was initially constructed and how it maintained itself to this day. I argue that sexism is one key factor explaining how this incidentally pink pharmaceutical drug became so successful. I make this argument based on the scientific publications and administrative archives from the 1960s. I analyse what Richard E. Proctor has coined as “regulatory impotence” in the French administrative decisions regarding the drug. I also aim to define what types of “ignorance” are involved in this case study and argue that the ignorance is such that it is probable that Spasfon is akin to a placebo. Finally, I will lay out the consequences of this situation notably on medical practices and the management of pain in France.

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Mapping the Brain - the tyranny of Hubel and Wiesel

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Why are neuroscientists, working on what is possibly the most complex machine in the universe, susceptible to single discoveries that dictate the course of research and the allotment of resources – financial, human and intellectual – for decades, rather than pursue multiple pluralistic pathways to understanding the brain? There are numerous examples of this in the fields of Alzheimer’s, epilepsy, Parkinson’s and other neurological disorders. And of course phrenology always springs to mind. One of the most lasting, indeed still current, and significant examples of this is the idea of maps in the brain as representations of the outside world. In spite of significant conceptual difficulties this model has continued to attract the attention of neuroscientists for the last 75 years. It makes an interesting example of monistic and overly focused research efforts. It is also an exmple where philosophical inquiry could contribute significantly.

The history is surprisingly simple. In the 1940’s neurosurgeon Wilder Penfield discovered during neurosurgeries that stimulating small areas of the cortex would elicit characteristic sensory and motor responses. In 1951 he published his now famous homunculus which would become an iconic symbol of brain architecture. Subsequent work on 'cortical columns' and visual representation by Mountcastle, Hubel and Wiesel (the last two awarded a Nobel prize for their work) cemented this idea of a neural representation of the world mapped onto brain space. It propelled this research program, to the virtual exclusion of all other possibilities, eventually involving many laboratories and thousands of post docs, graduate students ... and animals.

Lurking just below the surface, and surprisingly ignored by the experimental community, is the troubling question of who is reading the map? Is there a tiny executive homunculus watching the sensory maps and sending out instructions to muscles in accordance with the picture of the world it is receiving? Surely no one subscribes to this. But then ... who is reading the map?

This is a deeply philosophical question and ignoring it has led to decades of scientific effort chasing down a singular model that now seems to be in need of substantial revision, assuming it is worth preserving at all. It is a prime example of the perils of ignoring philosophical questions, especially when they might upset an admittedly large cache of experimental data. It is also an example of a severely monistic approach common to neuroscience research. Other alternatives were and are available as possible models. Data from other than the accepted systems (vision, audition) or model organisms (cats, monkeys, humans) could have suggested alternative models that were never imagined, let alone ignored. Instead, the work of Hubel and Wiesel and the Nobel committee's recognition directed brain research along a narrow one way path for 50 years.

As an example of an alternative I will use recent discoveries in the olfactory system – a sensory system fundamentally different from vison and audition that requires alternative representational schemes. I will also use a short example from embryology and current GPS map making to suggest alternatives to brain maps.

Dissent as an Internal Self-Control Mechanism

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In recent years, dissent has been mainly discussed as a driver of scientific progress, but less as a form of self-control in science. In this paper, I focus on dissent to understand similarities and tensions between "social" and "institutional" forms of quality control. In recent years, the concept of mutual internal scrutiny within science by ""epistemic peers"" has become central to various philosophical approaches (see Oreskes 2021). Besides the peer review process (see Heesen and Bright 2020) there are various other mechanisms of self-regulation, such as scientific conferences and discussions, as well as the social organization of dissent in general. An traditional example of this idea can be found in Merton's (1972) norms, where ""organized skepticism"" plays a central role for preserving scientific quality. Wilholt (2012) emphasizes that mutual criticism and control within the scientific community is the foundation for maintaining this asymmetry. However, despite being referenced at various points in the scientific literature these different control mechanisms, their roles, points of intervention, and actors are often only marginally discussed. My paper will meet this challenge against the background of recent calls for epistemic pluralism, which argue that incorporating diverse perspectives is advantageous

for solving epistemic problems (see Šešelja 2021). This assumption goes hand in hand with the belief that epistemically rich dissent and discussion are indispensable for gaining scientific knowledge.
To identify and analyze the self-control functions of scientific dissent, including the different roles of scientists and the limits of quality control, this study examines the scientific progress and concept of quality through a case study on the HIV-Aids-Debate around Peter Duesberg. I will argue that dissent can be characterized as a "social" form of quality control, which is implemented in and accompanied by several "institutional" forms of quality control, which may create tensions between these two forms (e.g., between epistemic values like consensus and innovation). Moreover, I show that scientific dissent and discourses are often influenced by non-epistemic values and internal power-structures of the scientific community, which can limit but also significantly strengthen the self-control abilities of science. With this I want to clarify under which exact conditions diversity can be a driver for better quality control.

Toward an Affective Account of Distrust in Medical Science

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While there is plenty of philosophical interest in distrust in medical science and science-based interventions, most of this literature has focussed on epistemic and value-based considerations. However, when we look carefully at cases of actual medical distrust, we see that these are affectively heightened for all involved. Even in the best-equipped hospitals with the most impressive medical staff, patients and their loved ones are often afraid, anxious, lonely, despondent, angry, frustrated – pick your favourite negative emotional state, or combination thereof. Similarly, medical staff can be frustrated, afraid, anxious, and burnt out. These heightened affective states are relevant for building and maintaining trust, and will impact the kinds of interventions that are appropriate. If distrust is affectively driven, then having an information campaign that targets epistemic considerations will miss the mark.

This paper looks at two cases of medical distrust, which are chosen precisely because of the extremely heightened affective states involved, thus allowing us to better pick out the role that emotions can play. The first case is the introduction of Anti-Retroviral Therapy (ARVs, the medication used to treat HIV/AIDS) in South Africa in the mid-2000s. This intervention was initially treated with suspicion, and many were unwilling to go out to be tested and thus access life-saving treatment. The second case is the international interventions that took place in West Africa during the 2013 – 2016 Ebola outbreak, where many refused the intervention and others responded with violence. These are cases in which negative affective states, like fear, in both community members (regardless of whether they were unwell) and medical providers have been identified as playing a key role in the way these epidemics unfolded. But what is the connection between negative affect and distrust?

I argue that the negative affect involved in these cases impacts the way that risk is perceived and interpreted, and the persistence of distrust over time. Further, given that both medics and patients experience these emotional states, this further adds to the negative affective stew. This

disrupts the empathetic connection that is important for the successful transfer of medical information and the negotiation of value disagreements when they occur.

Overall, there has been surprisingly little talk of emotion in contemporary Philosophy of Science, despite the “affective turn” having taken place in cognate disciplines, such as sociology and feminist philosophy. This paper argues that paying close attention to these medical cases requires that we take emotion more seriously in our philosophical work on trust, which can start a conversation about affect in Philosophy of Science more broadly.

When is it rational to distrust scientists?

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Hugh Desmond (2022) recently attempted to show that the conspiracy theorist’s stubborn distrust of scientific expertise can be rational. We will show that while Desmond’s attempt to provide a ‘how-possible’ argument fails, it points the way to two different circumstances in which the distrust of scientific authority is both stubborn and rational. The talk proceeds as follows.

First, because Desmond does not define rationality, we draw on recent philosophical work (Fogal & Worsnip, 2021) to introduce the distinction between structural rationality (i.e., logical consistency) and substantive rationality (i.e., reasonableness). We show that given what else he says, Desmond must mean that the conspiracy theorist is being substantively rational in their stubborn distrust of scientific expertise. We next provide an analytic argument that shows his definition of stubborn distrust is incompatible with being substantively rational and thus his “how possible” argument fails. However, this failure is instructive because we do think it is possible for stubborn distrust of scientific experts to be substantively rational.

To ground this intuition, we consider the case of Love Canal, a working-class community built atop a chemical waste dump. We examine the nature of the exchanges as scientists assured residents that their homes were safe, and residents repeatedly distrusted these assurances. For example, resident advocates and a state epidemiologist found that while they agreed on the importance of drawing conservative conclusions, they disagreed on the reason for caution: “To him, ‘conservative’ meant that we had to be very cautious about concluding that Love Canal was an unsafe place to live. The evidence had to be compelling because substantial financial resources were needed to correct the problem. To me, 'conservative' meant that we had to be very cautious concluding that Love Canal was a safe place to live. The evidence had to be compelling because the public health consequences of an error were considerable. (Paigen 1982, 32, emphasis in original).”

Drawing on this and similar exchanges from our case study, we argue that, with a suitably relaxed definition, residents of Love Canal’s repeated refusal to defer to the judgments of scientific experts can plausibly be considered a case of stubborn distrust. Our analysis of these exchanges suggests two different circumstances where stubborn distrust is substantively rational. In the first case, stubborn distrust is warranted because the experts are being irrational. In the second, there is a stable disagreement which is driven by differences in how one weighs risks of error. Accordingly, we argue that this case illuminates how different values can lead to rational distrust in scientific expertise. Additionally, the rationality of residents at Love Canal helps put in relief what is epistemically problematic with conspiracy theorist's distrust of scientific expertise.

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Loving Ignorance, Expert Trustworthiness, and Patient Integration in Psychiatric Research

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It is commonly assumed that ignorance among experts undermines the public's trust in their expertise. This assumption, for instance, motivates Owen Whooley's (2019) sociological study of psychiatry, which traces the history of American psychiatry as a record of the profession's collective attempts at "managing" its ignorance of the nature of mental illness. It is to these attempts that Whooley attributes psychiatry's professional resilience despite its numerous crises. Whooley's discussions, although primarily descriptive, raise important normative questions about ignorance and expert trustworthiness that merit consideration in light of the widespread public distrust in psychiatry. Does expert ignorance necessarily undermine the public's trust in them? How should the psychiatric community respond to its ignorance?

Contrary to common assumption, I argue that experts' ignorance does not, and should not, undermine the public's trust in them. However, certain dispositions among experts toward their ignorance may undermine or, in some cases, even enhance their public trustworthiness. This paper develops an account of a normatively appropriate disposition toward one's ignorance that can improve one's trustworthiness and facilitate responsible knowledge production. For this, I build on the feminist notion of "loving ignorance" (Tuana 2006), which is a disposition of acceptance of the limitations of one's situated knowledge and acknowledgment of one's epistemic dependence on differently situated others. Therefore, it is a form of epistemic humility attuned to the relational aspects of knowing and concerns the epistemic and ethical responsibility involved in producing knowledge about others. I argue that the psychiatric community should cultivate this epistemic virtue to improve its public trustworthiness and epistemic practices.

Considering the variety of sources of public distrust in psychiatry, improving psychiatry's trustworthiness would require a multi-pronged approach. This paper focuses on the public distrust evident in contemporary advocacy movements, such as the Mad Pride and neurodiversity movements, that have been challenging psychiatry's professional authority. I argue that these movements should be understood as epistemological movements seeking to reclaim epistemic authority and agency for those to whom these have historically been denied. Therefore, improving psychiatry's public trustworthiness and knowledge production would require acknowledging this and respecting patients' epistemic authority and agency.

Recent arguments for integrating patients in psychiatric research and decision-making, while a step in the right direction, fall short of capturing what it means to respect the epistemic agency of minoritized groups in the context of relations of power, and they overlook the necessity and difficulty of establishing trust in participatory research. Acknowledging the epistemic agency and authority of others, I argue, goes beyond regarding them as mere sources of information or evidence—it also involves active engagement in the form of scrutinizing, checking, and questioning one's perspective. In other words, patient involvement in research and decision-making should be substantial for patients' epistemic agency to be respected. Moreover, public trust in experts is both an outcome and a precondition for the meaningful integration of patients in collaborative or participatory research. Therefore, establishing relations of trust between professional psychiatrists and patients requires ongoing efforts, which the collective disposition of loving ignorance can facilitate.

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Crafting Scientific Narratives

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How do particular sets of experimental data become evidence for particular scientific claims? In this talk I call attention to the narrative constraints that shape data interpretation within individual publications and the larger research trajectories they comprise. I present a case study involving a particular C. elegans laboratory in which I worked for several years. I analyze the crafting of explanatory narratives during two different research processes that took place over different timescales. The nature of the complex decisions at both scales suggests that data interpretation is a fundamentally creative process, yet one that remains susceptible to philosophical analysis.

First, over the course of a decade, the lab changed its research focus from reproductive stress to social signaling. I reconstruct this transition as a series of creative decisions that could not have been predicted in advance, but which can be understood in retrospect using available philosophical tools. Certain procedures, originally designed to prove one target phenomenon, revealed something relevant about a different but related phenomenon. Through several iterations of this process, the lab found its way to a markedly more fruitful line of research.

Second, I analyze the production of a single early paper on which I was the first author. Using my own personal archive of laboratory notebooks and raw data, I map the relationship between the experimental timeline of data production and the narrative order of data presentation. This relationship is highly non-linear, largely because it took time to determine the best way of specifying the target phenomena and determining the relationships between them. There are suggestive resemblances between this process and much larger-scale ""data journeys"" that have been documented in recent data epistemology (Leonelli and Tempini 2020).

As a whole, this case supports recent claims that narrative plays a constructive role in the formation of scientific knowledge, akin to a ""general-purpose technology"" (Morgan et al. 2022).

After Haraway: Re-examining Feminism and Primatology in 1970s USA

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A longstanding concern for philosophers, historians and sociologists of science is to assess the ways in which broad contextual changes, such as the rise of social and political movements, come to impact science (Oreskes, 2014). One well-known study is Donna Haraway's Primate Visions (1989), which in part examined interactions between feminism and primatology in 20th Century USA. Haraway's key claim was that second wave feminism played a pivotal role in destabilising established narratives around female primates and gender within the discipline. While grounded in detailed historical research, Haraway's analysis was provocatively postmodern. Haraway's explicit aim was 'not' to provide a disinterested or objective account of events but rather to playfully blend multiple genre's, including science fiction, cultural studies, and political activism to further challenge conventional Western accounts of primates, science, and gender.

In re-examining this case, my purpose and approach differ. My aim is to systematically assess the impact of second wave feminism on both the practices and products of primate science. To achieve this objective, I combine a 'philosophy of science in practice' approach with micro-historical research to closely map interactions between the social movement and the scientific discipline. I specifically focus on the first phase of engagement, spanning 1970 to 1975. This phase marks a crucial period of context-science interaction that was initiated independently by Sally Slocum, a graduate student in anthropology at the University of California Berkeley, and Jeanne Altmann, a laboratory assistant to her husband Stuart in the biology department at the University of Chicago. Slocum was the first to publish work explicitly incorporating concepts from the radical stream of second wave feminism to critique existing scientific practices and propose feminist-aligned alternatives (1971; 1975). In contrast, Altmann's inclusion of feminist ideas was more subtle and her critique of current practice was not published until 1974, but the impact of her work was substantial, and her ideas circulated widely in the preceding years (1974). These initial feminist-inflected interventions were then extended by two recent post-docs from UC Berkeley, Adrienne Zihlman and Jane Lancaster (e.g. Lancaster, 1973).

In analysing this initial phase, I first outline primatology's research repertoire (Ankeny & Leonelli, 2016) before the influence of feminism. I then examine the interventions produced by these four feminist-scientists, reviewing the motivation, production, and reception of their work. I also demonstrate how, over time, the outcomes of these interventions lost connection to their feminist roots as they became normalised into the revised repertoire of the discipline. From this analysis, I make two key claims. First, I argue that, despite being primarily viewed as a social and political movement, second wave feminism's intellectual, epistemic, and cognitive dimensions must be appreciated to understand its impact on primatology. Second, I contend that, contrary to expectation, there is substantial empirical support for Haraway's most controversial claim - that second wave feminism affected primate science in more profound ways than even its central actors have supposed.

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Organized Entanglement: A Case Report of Casual Interdisciplinary Organization in a Biological Research Project

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This talk reports on a fine-grained analysis of a common research practice: a discussion among a group of academic scholars of a pre-circulated publication. The empirical focus is a single such meeting which took place online in the context of a large, hierarchically organized collection of 24 geographically distributed interdisciplinary research projects in the biological sciences (broadly speaking). The "Agency, Directionality and Function" project is itself part of an even larger research program exploring the possibilities for a "Science of Purpose" funded by the John Templeton Foundation.

The 1.5 hour meeting observed by one of us in June 2022 and reported here included researchers from a variety of disciplines: mathematics, physics, biology, and philosophy. The paper to be discussed is highly mathematical, but the project for which it was discussed sought to bring people of diverse kinds of expertise and experience together to collaborate on a shared project of considering concepts of "agency" applicable to biological phenomena.

We describe the meeting as taking place in three “acts” — “Getting Started,” “Presenting a Model,” and “Scheduling a Meeting” — to highlight an emerging empirical finding of our work: that ontological, epistemic and administrative aspects of the conduct of research are intimately entwined and linked in emerging and developing social organizations involved in scientific discovery. This entwinement of concepts, knowledge and administration in research “thickets” and how they are rendered manageable and are managed through the emergence of effective social organizations is a key phenomenon we seek to understand.

The aim is not only to report what happened in a single meeting, but to characterize proceedings of this and a variety of related forms in terms of a methodological “template” for constructing narrative case reports from data. A further aim is theoretical: the template is designed to aid discovery of social processes and mechanisms driving what we might call “casual organization” in the early stages of the emergence of interdisciplinary research pursuits. The overall aim of this project is to understand lifecycles of interdisciplinarity: how some ephemeral interactions may lead to productive, sustained collaborations across disciplinary lines while others don’t; how casual organization emerges; how some casual organizations become more formalized into stable, ongoing projects; and how novel research specialties emerge from such projects and practices.

Perception, Belief, and the Laws of Appearance

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Adam Pautz (2020) argues that representationalists about perception face a puzzle: the puzzle of the laws of appearance. There are some laws that restrict what kinds of things we can perceptually represent. Those laws do not apply, however, to beliefs. To be a representationalist is to hold that there is a similarity between perception and belief. If this is the case, why do the laws apply to one kind of mental state, but not the other? I argue that the puzzle is not a puzzle for representationalists in general, but only for some forms of representationalism that hold excessive analogies between perception and belief. I will consider three kinds of views: a view that identifies perception with belief; a view that claims that perception and belief share the same kind of content, viz. propositions; and the view that belief and perception are both intentional states, but do not share the same type of content. I will argue that the No Logical Structure law of appearance rules out views that identify perception with belief, and that consider propositions to be the contents of perception. If perceptual experiences were to have propositions as their contents, then we should be able to perceptually represent all kinds of propositions, such as “there is a blue triangle, or a red square in front of me.” Since we cannot represent perceptually such a disjunctive content, the format of perception is such that it is not a proper vehicle for disjunctive propositions. Therefore, propositions are not the contents of perception. I will suggest that the third kind of view, such as Tim Crane’s (2009), that can account for the No Logical Structure law. Since the contents of perception on this view are not propositions but objects, the law does not pose a puzzle that demands an answer.

This paper has three main goals that differ in their scope. More narrowly, I intend to show that Pautz’s puzzle is not really a puzzle to representationalism. Second, I want to use the puzzle to

refine how we should think about representational theories of perception. The third, and most broad, goal is to contribute to the discussion of the distinction between perception and cognition. The laws of appearance mark a significant distinction between perception and belief. If we can identify where the distinction lies, this will help us in understanding the border between cognition and belief, and how those processes might interact. One such distinction may be drawn around propositional mental states: such states may be part of cognition, but not perception.

The rise of the robots: automation, AI, and the future of research

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The rapid development and uptake of new AI-based workflows in science is transforming what research looks like and how it is done. A popular example of this development is AlphaFold, a deep learning-based algorithm that has quickly reshaped how scientists study the structure and function of proteins (Perrakis and Sixma 2021).

However, when scientists speak of the future of research they are usually not just thinking of AI, but also of what could be called “robot science”: the use of AI-enabled mechanised systems that perform individual experiments or whole research projects in a closed-loop setup. These systems are no longer “dumb”, in the sense that they simply follow a user-defined and pre-programmed sequence of steps. Rather, they generate and – crucially – learn from their own experimental data. First-generation robot scientists include “Adam” (King et al. 2009); “Eve” (Williams et al. 2015); the “mobile robotic chemist” (Burger et al. 2020); and Maholo LabDroid (Yachie and Natsume 2017).

Even though the term “closed-loop” is often used to describe robot scientists, they don’t work in isolation. Robot science consists of human-robot teams that co-create new research outputs. What is particularly intriguing about this teamwork is that there are two systems that learn, in different ways, from the data that is being produced. In my talk I will explore the epistemological challenges this new form of team-based learning poses, with a particular focus on the problem of experimental troubleshooting. This process is central to experimentation, as it is here that researchers make key decisions about artefacts, noise, and novel experimental setups. Using the case of Maholo LabDroid and the philosophical literature on artefacts and error in science I will argue that robot science risks undermining existing strategies for troubleshooting and thereby risks increasing the production of potentially irreproducible outputs. I will end by reflecting on how these problems might be avoided or mitigated.

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Experiments as the target of models: On the validation of computer simulation models in drug regulation

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As computer simulation models have increasingly replaced traditional experiments in many scientific fields, the validation of computer simulation models has sparked significant philosophical interest (e.g., Oreskes et al., 1994; Parker, 2008; Winsberg, 2010; Morrison, 2015; Lenhard, 2019; Beisbart & Saam, 2019). While each account of computer simulation model validation differs, some assumptions are widely shared: first, computer simulation models represent phenomena in the world; second, experiments can be used to effectively validate computer simulation models by intervening in the target phenomena and thus generating data about the phenomena.

Against these prevalent assumptions, I will argue that in certain situations, such as drug regulation, computer simulation models have experiments as their target of representation rather than phenomena. And to validate computer simulation models that target experiments, we need a different approach to validation that focuses on how well the computer simulation models represent specific experiments rather than phenomena in the world. Of course, the representation of experiments and the representation of phenomena are connected, and computer simulation models that properly represent experiments may represent the associated phenomena as well, albeit indirectly. Nevertheless, shifting our focus from the representation of phenomena to the representation of experiments has important consequences. That is, it will enable us to shift our attention from learning about certain phenomena through experimental intervention to learning about specific experiments through interaction with some phenomena.

My argument will be guided by a case study on the validation of (Q)SAR ((Quantitative) Structure-Activity Relationship) models for the regulation of mutagenic impurities in pharmaceuticals. (Q)SAR models refer to the computer-based models that predict the biological activities of certain compounds based on their chemical structures. To improve the regulatory use of (Q)SAR models, the OECD developed the principles for the validation of (Q)SAR models for regulatory purposes (OECD, 2004, 2007), and the ICH adopted the OECD principles to publish a guideline on the regulation of mutagenic impurities in pharmaceuticals (ICH, 2017). Importantly, these regulatory guidelines explicitly require that QSAR models be associated with specific experiments (e.g. bacterial mutagenicity assay) with standardised experimental protocols, rather than with specific biological effects (e.g. mutagenicity). Consequently, the validation of QSAR models depends on their relationship with specific experiments.

My view of experiments as targets of computer simulation models is partially consistent with Beisbart's (2017, 2019) and Winsberg's (2003) views of models as "simulated experiments". Nonetheless, my paper will throw light on at least two important yet overlooked issues. First, while much of the existing philosophical work on computer simulation models has focused on fields where traditional experiments are notoriously difficult to conduct (e.g., climate science, particle physics), I will highlight the practice of using computer simulation models when traditional experiments are highly feasible but possibly undesirable for epistemic and non-epistemic reasons. Second, by highlighting the case, I will encourage a reassessment of the relationship between experiments, phenomena, and computer simulation models.

Mathematics and the meshing of plans: Doctrine, Drill, and Shared Agency

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What is mathematics? Realist scientific ontologies like Basic Formal Ontology (BFO) leave open when, how, and even whether mathematics might be formally specified in a realist framework. Yet there is good reason to suspect that a satisfactory realist ontology of mathematics is possible using existing tools. Building upon the social ontology of Searle and Smith, this contribution attempts to account for mathematics in terms compatible with BFO, starting with arithmetic. In short, mathematics is a large mesh of meshable plans.

I draw from Smith's notion of document acts and the ontology of military doctrine for most of the components required to make this case. Smith views doctrine as a collection of information artifacts that confer capabilities via drill (training). These capabilities enable effective individual action and, most importantly, the construction of modular, meshed plans. Smith notes that battle plans and the practices that surround and enable them accomplish what Shapiro (in another context) called massively shared agency.

I argue that mathematics is amenable to a close analog of this treatment. I present mathematics as providing doctrine for the practice of planning and executing mathematical acts such as proof and calculation. Consider the symmetry: mathematical information artifacts confer capabilities via training, enable effective individual action (calculation), and the modular, meshed plans accomplish the massively shared agency called applied mathematics. I demonstrate the depth and breadth of this approach using examples from and inspired by Wittgenstein, the Online Encyclopedia of Integer Sequences, and Feynman's Caltech lectures. I broach some potential consequences for the ontology of 'pure' mathematics.

Finally, there are reasons to believe that the social ontology of mathematics must be similar in many ways to the social ontology of information systems. If so, mathematics could be seen as a sociomaterially enacted system of record able to produce mathematical doctrine, plans, drills, and facts as needed. Perhaps mathematics is applied information.

Natural Selection, Scientific Practice, and the Boundaries of a Biological Individual

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For decades, philosophers of biology have quarreled over the causal power of natural selection. The "causalists" argue that natural selection is a genuine causal process, whereas the "statisticalists" argue that natural selection is an epiphenomenon—the mere appearance of a causal process. Meanwhile, other philosophers are disillusioned with the debate altogether. Their disillusions are grounded in their belief that such abstract debates are too far removed from biological practice; whether we call natural selection a causal process is irrelevant to the practical considerations of evolutionary researchers. In this talk, I will argue that this dismissal of the causalism/statisticalism debate is unwarranted; it indeed has practical implications. More specifically, I will argue that how one conceives of natural selection can have consequences for their views on how to demarcate biological individuals.

Many biologists endorse the view that a biological individual is an entity upon which natural selection can act. Therefore, how we understand natural selection (that is, as a causal process or an epiphenomenon) partially determines how and where we draw the boundaries between individuals. I play out my argument with a current and contentious case study: the "holobiont" concept. Some biologists and philosophers argue that the holobiont—defined as the sum of a plant or animal and its entire microbiome—is subject to natural selection and is therefore an individual in the biological sense. Others argue that, because holobionts are composed of symbiotic partners that are independently subject to selection (such as a coral and its photosynthetic algae), holobionts are made up of individuals but are not individuals themselves. I will argue that much of the controversy surrounding the holobiont concept is fueled by ambiguous accounts of natural selection. This controversy can be deescalated with closer attention to previous work on the metaphysics of natural selection. Finally, I will conclude by considering implications of my argument for the possibility of a practice-centered metaphysics of science.

From metaphysics to epistemology of causal production: using variation to trace transmission of information

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Causal talk across the sciences and everyday life is very diverse; a point made at least since Anscombe (1975). Because of this, pluralistic strategies have been tried in many places, including Cartwright (2004), Weber (2007), and evidential pluralists (see e.g. (Russo and Williamson 2007; Campaner 2011; LaCaze 2011; Clarke et al. 2013; Reiss 2015; Parkkinen et al. 2018; Pérez-González and Rocca 2021)).

Illari and Russo (2014) have defended their causal pluralism, the causal mosaic approach. We adopt their view that causality is not to be reduced to one philosophical question or one scientific problem. Within this context, the sciences and everyday life are replete with cases in which we, epistemic agents, are interested in how causes produce effects (Illari 2011; Illari and Russo 2016; Vineis, Illari, and Russo 2017; Vineis and Russo 2018). Here we will offer an account of information transmission as causal production as a concept that needs to be added to the library of useful causal concepts.

We think we need a concept of production (information-transmission), and a concept of difference-making (variation), but in this paper we focus on causal production beginning with metaphysics but moving to exploring its corresponding epistemology. This metaphysics is not a priori, but is always the product of an agent's perspective, and in this sense our approach aligns with perspectivism (Giere 2006; Massimi 2022), constructionism (Floridi 2011a; 2011b), and ontoepistemology (Barad 2007) and aligns with the lengthy discussion of Russo (2022).

Illari and Russo (2016b) and Russo (2022) formulated a number of desiderata, which we will adapt. A concept of production should:

- [scientific domains:] make sense across sciences, including physics, social sciences, life sciences, and particularly for cases of causal relations across these levels.

- [levels:] help us understand causal relations across micro and macro causes (and vice-versa) and across factors of different natures (sometimes called 'inhomogeneous variables').

- [technology:] be able to return a meaningful metaphysics for highly technologized contexts, in which there is arguably an important element of construction (so causal relations are not in any simple way 'out there').

In this paper we first present information transmission as a thin and general metaphysics of production, with advantages and complementarity with respect to other production accounts. In line with the constructionism of Floridi (2011b), and the ontoepistemology of Barad (2007), and Russo (2022) we need to understand how human epistemic agents come to establish that there is transfer of information. Then, we will explore the epistemology of information-transmission, distinguishing between 'information-transmission' and 'variation' epistemological strategies. We will show that, despite information-transmission being a key concept of production, evidential pluralism still holds. To know about information transmission requires both epistemological strategies that seem allied with our concept of production (such as mark transmission and process tracing) and epistemological strategies that seem allied with our concept of difference-making (variational strategies such as observational studies, studying variation across similar and different things). Ultimately, causal pluralism is a complex but rich approach to deal with causality in the sciences in their full diversity.

Against standardising mean differences

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In applied areas of science, researchers often measure and report the effect sizes of tested interventions to inform decision-makers. For continuous outcome variables (such as reading test scores), researchers usually measure an intervention's effect size using the mean difference – measuring how much, on average, a tested intervention increased the measured outcome – or using the standardised mean difference – measuring by how much of a standard deviation the tested intervention, on average, increased the measured outcome. Both of these effect size measures quantify how effective an intervention is, but they provide decision-makers with different information.

Should scientists report the mean difference, the standardised mean difference or both to decision-makers? Various concerns bear on which answer is correct. In line with recent work on effect sizes for binary outcome variables (e.g., Sprenger and Stegenga 2017), we will focus on just one: When do standardised mean differences or mean differences inform agents sufficiently that they can tell which intervention is best?

This question gets bite when we consider the widespread uncertainty concerning measurement that besets much of applied research (e.g., Rosnow and Rosenthal 2009). Researchers are often uncertain about a) how the measures used in various studies relate to each other (e.g., how different reading scores compare to each other) and b) whether these measures even measure the same empirical property (e.g., whether these scores all measure the same reading ability). The measurement uncertainty described in (a) and (b) makes interpreting and comparing mean differences difficult; researchers cannot say how big the change in the empirical property is that a mean difference represents. To deal with this problem, researchers often follow the advice of textbooks (e.g., Grissom and Kim 2011): standardise the mean differences. Standardised mean differences can be compared and interpreted with respect to an empirical property of interest, even given measurement uncertainty, or so it is claimed.

However, as I will argue, to inform decision-makers sufficiently, researchers should not standardise the mean differences when facing measurement uncertainty. Even given measurement uncertainty, mean differences inform decision-makers sufficiently to choose the best intervention exactly when mean differences do so absent such uncertainty. By contrast, standardised mean differences do not sufficiently inform decision-makers to choose the best intervention, either given or absent measurement uncertainty. Hence, mean differences, not standardised mean differences, facilitate good evidence-based decision-making – even in light of measurement uncertainty.

To draw these conclusions, I provide a formal decision model for choices between interventions involving measurement uncertainty. Based on this model, I propose 1) a criterion for choosing the best intervention given measurement uncertainty and 2) a corresponding criterion for when an effect size provides sufficient information to make such a choice. Both criteria will make commitments on how to rationally respond to measurement uncertainty, which other philosophers may challenge. My adjacent aim is to set the ground for such debate.

I conclude with implications for the widespread practice of standardising mean differences in applied science. Overall, this paper adds normative considerations to methodologists' debates on standardised effect sizes (e.g., Baguley 2009; Cummings 2011).

Reconciling Process and Structure: Towards a Process-based Ontic Structural Realism

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The recent years have witnessed the rise of two prominent metaphysical views that are fundamentally contradictory to each other in the field of the metaphysics of biology. One is Mechanistic Metaphysics (MM), which is based on an ontology of objects (Glennan 2017; Krickel 2018). The other one is Processual Metaphysics (PM), asserting that all that exists are dynamic processes (Dupré 2021). The debate between the two views has reached an impasse because both sides are supported by reasonable motivations that are not addressed by the other.

On the one hand, much of the practices in life sciences are driven by the search for mechanisms that consist of the interaction of objects organized in a certain way. MM aims to ground the successes in life sciences, achieved through manipulating objects involved in mechanisms, and to accommodate causality associated with mechanisms. On the other hand, PM finds its motivation from the observation that living systems never exist independently but are interdependent and always interact with many other things, challenging the idea that living systems are discrete objects with intrinsic properties.

In this paper, I dissolve this debate by developing what I call the process-based Ontic Structural Realism (OSR). I argue that my view can break the impasse because it is able to ground the success of life sciences and accommodate causality associated with mechanisms without committing to the existence of objects, thus retaining the central commitments of processual ontology.

OSR was initially motivated by certain concerns in the debate of scientific realism, and it quickly became a prominent metaphysical theory in the philosophy of physics. Similar to PM, OSR rejects objects as a part of our ontology and argues that all that exist are structures which can be broadly understood as modal relations (Ladyman and Ross 2007; French 2014).

I propose a reconciliation between processes and structures as a solution to the debate between MM and PM. It leads to a processual structural realism or process-based OSR in which structures are understood as dynamic processes or processes are understood as possessing inherent modal or causal force and being associated with modal or causal relations. I then demonstrate how these structured processes can serve as the metaphysical underpinnings of mechanisms through reconceptualizing relevant objects, which enables my view to explain the success of life sciences and accommodate the causality related to mechanisms.

Afterwards, I apply my account to the field of genetics as a case study. There are also two competing views regarding the metaphysics of genes. One is to understand the gene as an object that possesses intrinsic properties and acts as the 'seat' of causal power (Austin 2016), while the other, inspired by the so-called Developmental Systems Theory, is to interpret the ontology of genetics in a processual way (Griffiths and Stotz, 2018). In my account, genes are conceived as 'nodes' of structured processes instead of objects with intrinsic properties and identity, and genetic causation is grounded by causal structures understood processually rather than the intrinsic dispositional properties of genes. The unique advantage of my view is that it is consistent with a processual reading of genes while also being able to accommodate genetic causality, and thus makes my view best-fitting with the practices of geneticists.

Beyond One-Size-Fits-All: Reframing Cognitive Variations and Individual Differences in Cognitive Science and Neuroscience Research

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This paper addresses emerging concerns about the generalizability of scientific explanations and the reliability of scientific results in animal experimentation. The use of rodent models has traditionally operated under the assumption of homogeneity among the subjects. However, emerging evidence shows that these animals exhibit widespread heterogeneous behavioral responses, thus challenging this assumption. For instance, female rodents appear to be more vulnerable than male rodents in the chronic mild stress model (Dalla et al., 2010), and female rodents display lower levels of freezing than male rodents in fear-conditioning tasks (Shansky, 2015; Colom-Lapetina et al., 2019). The varied responses in these findings have cast doubt on the representativeness of traditional experimental subjects (e.g. male rodents). This variability, often overlooked as experimental noise, undermines the generalizability of research findings and thus raises serious concerns about the heterogeneity within populations and the reproducibility of results.

In this paper, I aim to provide a framework for resolving these concerns. It consists of two steps. The first step distinguishes two kinds of variations in experiments. The second step argues that a clear understanding of these notions plays an essential role in enhancing the reproducibility of scientific results.

To begin with, two kinds of variations are cognitive variations and individual differences. Cognitive variations refer to the variance in cognitive performance across different behaviors or processes, such as social decision-making (Ward, 2022). Individual difference, conversely, denotes biological differences among individuals within a population, including sex and age. I argue that individual differences serve as the explanatory basis of cognitive variations. For instance, the explanation of adults' social behaviors should take their sex differences into consideration. By delineating these two concepts, we can facilitate a more nuanced consideration of the roles that variations play in scientific research and also the relationship between these two variations.

The second step of this paper advocates for a reexamination of scientific methodologies. Typically, traditional approaches dismiss individual differences as confounding factors and attribute cognitive variations to a range of local causes in experimental contexts. This conventional emphasis on a narrow selection of high-performing subjects, to generalize cognitive capacities for entire species, results in a significant underrepresentation of cognitive variations. Drawing on McAllister's (1997) interpretation of phenomena as patterns in data sets, I argue that cognitive variation should be considered to be meaningful patterns rather than mere noise. This viewpoint is reinforced through a case study on estrogen's impact on mice behavior and brain health. This study exemplifies the significance of individual differences in manifesting cognitive variations and challenges the conventional view that such factors are merely local causes in specific experimental contexts. Instead, based on this case study, I offer a new perspective for the interpretation of scientific data: if we consider cognitive variations as patterns, then these biological factors are no longer external to the mechanistic explanation of specific cognitive functions. If my argument succeeds, this paper will contribute to a comprehensive understanding of variations in scientific findings and provide a conceptual framework for future research and applications.

Data Integrity Through Validation Method Revelation in Real-World Evidence

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Real-world evidence (RWE) and randomized control trial (RCT) are considered to be mutually complementary clinical evidence in the context of drug effectiveness and safety in healthcare (Kim et al., 2018; Morales & Arlett, 2023). RWE is relatively crucial for some populations, for example, most clinical trials exclude pregnant women, older adults, and children due to safety and ethical concerns. However, there are some methodological flaws in RWE's data analysis especially those from electronic health records (EHRs) due to the gap between medical practice and research. Some epidemiologists publish their research with a detailed validation paper as a supplement to explain how data are analyzed while some appeal to clinical insight in the discussion section of their research paper. This article aims to analyze how epidemiologists draw causal relation from RWE with the empirical method of text mining. We will take the instance of metformin usage during pregnancy and the risk of congenital malformation as a case study to show that how epidemiologists analyze data from the diagnosis of ICD code in electronic health records with different criteria. The differences of the criteria would thus influence the causal relation of the research outcome but may not be noticed if the validation method is not revealed. Lastly, since RWE plays critical roles in medical practice or even drug approval in the populations in which RCT cannot be conducted, we will claim that data integrity through validation method revelation in RWE should be emphasized in peer review.

Limit-Making and Limit-Breaking: Debating Cosmological Knowledge in 20th-Century Britain

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This article explores the debates in cosmology that occurred during the 1940s–1960s through the lens of the limits of scientific knowledge. Specifically, I argue that many physicists during this time were engaged in two activities that have so far received little explicit attention in PSP literature: limit-making—constructing limits on the domain of possible scientific knowledge—and limit-breaking—rejecting these limits. These activities played a central role in debates

between big bang and steady state models of the universe, and in the making of cosmology a science.

The first part of this project outlines conceptions of limit-making and limit-breaking. Adopting a framework of analysis that focuses on practices and questions, I posit limit-making as the process through which agents identify certain questions as unanswerable through scientific means; vice-versa, limit-breaking is the process through which agents identify previously-rejected questions as answerable through scientific means. The second part identifies these practices in mid-twentieth-century British cosmology. Physicists on all sides of the debates—supporters of evolutionary models, supporters of steady state models, and critics of one or both—engaged in limit-making and limit-breaking. Questions that scientists pursued or rejected included, 'what is the age of the universe?', 'is the universe as a whole evolutionary or in a steady state?', and 'what were the conditions of the early universe?', among others. Ways that agents argued for or against these limits varied: for instance, steady state supporter William McCrea and cosmology-skeptic Martin Ryle agreed that the question 'under what conditions was the universe created?' was unanswerable, but McCrea contended that this was because we cannot extrapolate known physical laws into the past, whereas Ryle argued that 'the universe as a whole' was not subject to scientific inquiry. I identify prevalent ways agents went about limit-making and -breaking, and demonstrate that these were complex activities, subsuming a number of other practices, and often took place in interdisciplinary and popular forums: debating in radio broadcasts, reviewing science, philosophy, and theology texts, and educating the public and professionals during symposia. Finally, I turn to the practical effects that limit-making and -breaking had on scientific research: how did these practices constrain, impel, and direct scientists' other activities? The key role they played, I argue, was compelling physicists to alternatively pursue and abandon different lines of research on species of universe models.

This paper is a project in integrated HPS: the concepts of limit-making and -breaking emerge from studying the historical episode, are refined based on previous work on limits and practices in philosophy, and then are applied to the episode to gain further insights. The intended upshot is twofold. First, it hopes to contribute to the historical literature on mid-twentieth-century cosmological debates, in which discussions of scientists' practices have so far been largely absent. Second, it aims to highlight limit-making and limit-breaking as two interesting activities scientists partake in, both in this and potentially other historical and present-day cases.

Scientific Unity in Street's Darwinian Dilemma

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Street's Darwinian dilemma claims that anti-realist theories are more parsimonious explanations of moral evaluative attitudes. They claim we have evaluative attitudes because having them was evolutionarily beneficial. Unlike the realist, the anti-realist does not need to appeal to moral facts in this explanation. However, parsimony is not the only way a theory can be simpler. Realists may appeal to theory unity to respond to the anti-realist's dilemma. The kind of unity the realist may appeal to is consistency of explanation. Consistency of explanation

is when the same account explains more observed phenomena (evaluative attitudes). Realists may do this by using a tracking account to explain both moral and epistemic evaluative attitudes. Some anti-realists use a tracking account to explain epistemic evaluative attitudes and an adaptive link account to explain moral evaluative attitudes. The realist position would then be more unified by comparison. Anti-realists defending such a position must explain why their theory elegance and ontological parsimony is preferable to the realist's unity. However, there is no agreement as to how to go about this. Unless this can be done, this complicates Street's preference based on the simplicity of the anti-realist position over the realist position on the second horn. Further, this implicates other similar anti-realist accounts claiming simplicity over realists. However, through an objection, we will show the anti-realist who claims to explain all evaluative attitudes with an adaptive link account possesses the simplest theory. Determining which simplicity comparison is the most useful will require arguing for the most plausible realist or anti-realist account. I will avoid doing this and will only mention the various measures of simplicity they comparatively do well or poorly on. Whatever the most plausible position is, my paper will inform it as to how well it fares in terms of simplicity when compared to either the opposing realist or anti-realist.

'Instruments of Our Own Species': How Observational Elitism in Astronomy Delayed the Discovery of Personal Observational Differences

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The discovery of the personal equation in astronomy, usually traced to the work of F.W. Bessel, is ordinarily presented as a narrative of success. Bessel detected a phenomenon that his predecessors and peers had overlooked. He began his discussion of the problem of involuntary constant personal differences in observation by referencing the dismissal of David Kinnebrook, an unlucky assistant to Astronomer Royal Nevil Maskelyne. Kinnebrook persistently marked transit times nearly a second later than did the Astronomer Royal. Bessel conjectured that Kinnebrook would have done everything in his power to align with his superior's observations. That Kinnebrook's observations continued to differ from those of Maskelyne demonstrated the reality of the personal difference phenomenon.

Despite Kinnebrook's central role in Bessel's conjecture, little interest has been shown in Kinnebrook's actual predicament, a predicament that is vividly portrayed in Kinnebrook's correspondence with his father, which resurfaced in 1985. The letters provide convincing proof that Kinnebrook defended the accuracy of his own observational practice to Maskelyne and that other parties were privy to the dispute for the seven months preceding Kinnebrook's dismissal. Additionally, Kinnebrook alleged that observational logbooks revealed differences of a similar order had existed between Maskelyne and earlier assistants. Finally, the original observatory's logbooks show that Maskelyne passed off months' worth of his supposedly unreliable assistant's observations as his own.

As Christoff Hoffmann (2007) has shown, the epistemology of the observer underwent a radical, if unnoticed, transformation from the late eighteenth century to the middle of the nineteenth century. In the days of Maskelyne and his eminent predecessor James Bradley, the experienced observer was regarded as the proper measure of things: training, diligence, and focused attention empowered the experienced observer to see things exactly as they were. The prevalence of this doctrine enabled Maskelyne to dismiss Kinnebrook without remorse, reflection, or reconsideration. From the late 1830s, when the personal equation emerged as a scheme for data reduction, the human observer was acknowledged as a source of error, but the errors were regarded as innocuous since their effects were accounted for.

By taking Kinnebrook out of his typical historical portrayal as a passive object of Bessel's investigations and placing him as an active scientific practitioner, this case study uncovers an image of the ideal observer that was quietly deteriorating, largely escaping the notice of the astronomical elite of that time. Kinnebrook's recollection of the conflict and the defense of his observational practice – a defense that was unheeded by his superior – thus serve as a direct challenge to the conventional historical narrative surrounding the concept of the personal equation.

Revitalizing the Value-Free Ideal of Science: The Temporal Role of Values

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This discussion of values has led to a standoff between two broad perspectives on the nature of scientific inquiry: the Value-Free Ideal of science (VFI) and the Value-Laden Ideal of science (VLI). The VFI perspective holds that the scientific enterprise can, at least in principle, be free from human biases. In contrast, VLI, abandons the attempt to eliminate human biases in science and instead embraces the potential contributions of non-epistemic values or, at the very least, acknowledges the inevitability of their pervasive existence in science.

In this project I will argue for a rapprochement of sorts between the VFI and VLI perspectives. I call the Sophisticated Value-Free Ideal (SVFI). This framework aims to integrate the VFI, which sets the goals for science, with the VLI, which provisionally guides aspects of scientific practice. The SVFI model is composed of two components. The first concerns the overarching objective of science—achieving objective knowledge—which sets the stage for the second component, the practice of science, which the SVFI holds should be as free from non- epistemic values as possible.

First, I examine various philosophical perspectives concerning the value dynamics within scientific practices. I then introduce the differentiation between epistemic or cognitive and non-epistemic or contextual values, raising questions regarding both the validity and necessity of this distinction. In the second section, I establish the connection between the demarcation issue and the issue of values in science (VIS). I argue that both the VIS question and the demarcation problem revolve around our desire to discover trustworthy knowledge, and it is thus illogical to apply different value sets to distinguish science from non-science and to assure its epistemic reliability. I introduce a new approach, a multi-dimensional criterion for demarcation, where methodology, history, and epistemic values are interrelated. Under this framework, the VFI of science is required to establish science's aspirational goal and define its purpose and practice from the outset. In the final part, I extend this argument and promote a

new account, namely the temporary roles of values, to understand the role of values in science. According to this perspective, as science advances and matures, the influence and function of non-epistemic values should gradually diminish, giving way to the overarching epistemic goals that science aims to achieve. This view reflects a dynamic understanding of science wherein values are intertwined yet temporary, guiding the practice but not altering the fundamental, objective quest for truth.

On Generalizing Stipulated Functions

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Functional localization has been a foundational thesis in neuroscientific practice. Although only tenuously supported, neuroscientific researchers often assign a function to patterns of neural activity to explain experimental results. For representational pragmatists, a function is stipulated based on the experimental paradigm used to isolate a pattern of neural activity (Cao 2021). This constrains the scope of function ascriptions and explanations based on them to the particular experimental contexts in which the neural activity was intervened on or observed.

I argue that this practice is insufficient for ascribing functions. Researchers draw on the broader research domain to inform their ascriptions of function to patterns of neural activity. This means that determining whether or not a stipulative function is justified depends on how the broader research domain is appealed to and how research results are generalized. In the account I develop here, neuroscientists assign functions to neural activity not only to make the system intelligible or to connect their research with other work in the brain region, but also to communicate their results more broadly. When researchers offer their interpretation of experimental results, they are entering their results into the storehouse of background information that can be drawn on by other researchers to establish the functional architecture of brain.

One worry with this practice is that the brain may very well be the kind of system whose parts have contextually-determined functions (Burnston 2016). If areas of the brain perform functions that are specific to particular contexts, then it may be appropriate to constrain the scope of function ascriptions to the particular context in which the function is performed. That is, patterns of neural activity within a certain area couldn't be said to perform the function typical of that area. The worry is that results and explanations based on them will not generalize. Burnston's solution is to treat functional localization as the null hypothesis, thereby assuming an accepted functional ascription for a given area unless experimental results suggest otherwise. This solution involves structuring experiments around determining the appropriate contexts for eliciting various functions.

But the null functional ascription of any given area—which will inform the function to ascribe to a pattern of neural activity—will already contain a disjunction of various accepted functions. And multiple functions in a given set may be used to guide interpretation of experimental results, thereby neither ruling out nor confirming a given function ascription. Deciding among them will require extrapolating from previous experimental results that were performed in non-identical contexts, which is the generalization practice described above.

Stipulating functions based on research in the broader research domain is an apt practice to garner inductive support for the function localization hypothesis. Thus far, evidence for the thesis is tenuous, but there is incontrovertible evidence that some areas of the brain are required for certain functions. I suggest that the practice of stipulating functions would benefit from analysis of the functional units that provide generalizations of function ascriptions that best meet neuroscientists' epistemic aims.

Two Kinds Of Conceptual Openness In The Operationalization Of Schizophrenia

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Let's say that schizophrenia is at one point in time best understood and defined in terms of some factors (a, b, c) but at a later point in time we discover (d) and learn that (a) is irrelevant and so schizophrenia is now best understood and defined in terms of (b, c, d). Later still we might find that we were describing two different conditions all along that we now wish to differentiate even if they overlap: (a, b, c) / (c, d, e). Seeing scientific concepts as open to such ongoing revisions was the decisive metatheory that allowed Paul Meehl to overcome resistance in the psychological community of the 1930s and 1940s to bring a systematic and quantitative approach to schizophrenia and other psychiatric categories: We should be open to the possibility that schizophrenia will ultimately be best understood in terms of an entirely different set of factors than at the outset: (d, e, f). If the scientific subject is inherently inexact, operating with a perpetually revisable, quasi-exact concept is not a bug but a feature of good methodology. (Meehl 1991b, 15)

Meehl found this theory of open concepts in the works of logical positivists, in particular as it was developed by Rudolf Carnap and Arthur Pap. It led up to Meehl's introduction of construct validity to psychometrics in his seminal 1955 paper with Lee J. Cronbach as a guiding idea for how to deal with the precise meaning of imprecise scientific objects and psychological constructs. Meehl and Pap also identified the underlying idea with that of another member of the Vienna Circle: Friedrich Waismann's notion of the open texture of empirical concepts. In this presentation I want to distinguish these two kinds of conceptual openness that we find discussed both in mid-20th-Century psychology and philosophy of science. I will reconstruct how the theory of open concepts indeed helped Meehl to motivate the measurement of phenomena such as schizophrenia that would at the time have been deemed too poorly understood to allow for an operational definition. But I will also note the limitations that came with the adoption of Carnap's and Pap' specific idea of open concepts, namely (under Carnap's physicalism) the assumption that such concepts will be "closed" at some point, for example when we will find the gene that causes schizophrenia. By contrast, Waismann understood open texture to be an irreducible property of empirical concepts with no obviously preferable closure. With hindsight on the search for a gene that would cause schizophrenia, open texture today seems to offer a better theory of the perpetual revision of psychiatric categories.

The Evidential Significance of Model Disagreement

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Parker (2011) and Lloyd (2015) both address the question: What can we learn from model agreement? They disagree on the answer, with Parker arguing basically "nothing" and Lloyd arguing that models are confirmed by their agreement. Focus on agreement, especially among philosophers of climate science, has detracted from attention on the evidential significance of model disagreement. This paper fills that gap by drawing on recent work in the epistemology of disagreement to show that model disagreement is evidentially significant, including in some cases identified by Parker (2011) where model agreement is not epistemically significant.

Conciliationists with respect to peer disagreement argue that disagreement is an evidentially significant form of higher-order evidence (Feldman 2009; Kelly 2010). Someone faced with an epistemic peer who disagrees about some proposition P, should lower their confidence in P, perhaps to the point of suspending judgment on P. The conciliationist's main insight is that disagreement is a form of higher-order evidence. Higher-order evidence, evidence about the status of our evidence, can undermine or weaken positive reasons to believe, in this case, the reliability of our models. The basic argument is that, if we have as much reason to trust one model as we do the other then, if they give conflicting projections, we cannot have high confidence that either of the two has given us the correct projection. Put in terms of probabilities, we know that our credence in the output of model M1 is equal that of model M2, but the sums of the probabilities of the outputs must be less than or equal to 1, therefore, our credence in each of the two projections must be equal to or less than 0.5. Similar results hold for more than two models and models of unequal reliability, the only difference will be the level of the ceiling put on our credence in the projections.

As Lloyd (2015) focuses on the evidential significance of agreement for the models themselves, not their projections, we may also wonder about the significance of model disagreement for the evaluation of models. Peer disagreement also holds lessons for us there too. Using insights from Begby (2021), I show that, given reasonable assumptions about the models, disagreement gives us reason to lower our assessments of the model's reliability.

Parker (2011) argues that when models are dependent we have less reason to think that agreement is evidentially significant. This is relevant for climate models as they have been derived from just a few original models and embody 1 many of the same assumptions, thus, Parker argues, they are not independent and do not give good reason for increasing our confidence. The opposite, however, holds for model disagreement: probabilistic dependence increases the strength of evidence from disagreement. This holds both for the outputs of the models and the models themselves.

Multiverse analysis as a plausible solution to the replication crisis

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Short abstract:

In our presentation, we argue that replication crisis emerges due to (1) researcher degrees of freedom in research design and statistical analysis and (2) different but plausible methodological decisions leading to conflicting results. We analyze when alternative methodological decisions are plausible and develop a concept of plausibility threshold to respond to some arguments from psychologists (DelGuidice and Gangestad 2021) claiming that the choice of a superior (even only slightly) commitment is possible. Finally, we suggest that a wider use of multiverse analysis (an approach to data analysis relying on estimating models resulting from all permutations of permissible methodological decisions) might solve the crisis of replicability.

The fully-fledged abstract:

So far, the replication crisis is mainly seen as resulting from honest errors and questionable research practices such as p-hacking or the base-rate fallacy (Bird 2021). We discuss the malleability (researcher degrees of freedom) of quantitative research and argue that a significant proportion of conflicting results emerge from two studies using different but plausible designs (e.g., eligibility criteria, operationalization of concepts, outcome measures) and statistical methods. Our analysis develops Feest's (2019) observation that some replication attempts are not identical to the original studies, and the differences between studies account for the heterogeneity of reported results.

We also show, using case studies from economics, psychology, and medicine, that conflicting results emerge from the permissible malleability (researcher degrees of freedom, see Wicherts et al. 2016) in statistical analysis and research design. Our argument relies on the discussion of methodological decisions involved in designing a study and analyzing data. We develop the notion of a threshold of plausibility to respond to DelGuidice and Gangestad (2021) criticism relying on the distinction between principled equivalence and uncertainty. Our proposal supports multiverse analysis (Steegen et al. 2016) by showing that the choice of a 'better' statistical analysis is often impossible. To conclude, we discuss several philosophy of science questions emerging from the multiverse analysis approach and indicate plausible responses.

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The construction of GE mosquitoes with gene drives as the solution to the high incidence of mosquito-borne diseases: An epistemically and ethically responsible scientific practice?

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

GE mosquitoes with gene drives are being developed such that the genetically engineered insects and their GE offspring are compromised in terms of survival ability or their capacity to function as disease vectors. If GE mosquitoes with gene drives are released in the wild and if they mate with their wildtype counterpart, then depending on the type of gene drive introduced in them, presumably, if everything goes as intended, over multiple generations they could cause the population of their wildtype counterpart to either collapse or not act as disease vectors.

Scientific papers that construe the high incidence of a mosquito-borne disease in poorer communities in regions of the global South as a primarily biological phenomenon frame the disease vector or the infectious agent as the logical point of intervention. Those scientific narratives create the impression that the only (or most) effective way to reduce the prevalence of the disease is to use patented high-tech scientific-technological interventions that target the disease vectors or the pathogens. Such narratives obscure the role of political and economic inequities in creating and maintaining public health problems that disproportionately affect the poor.

This presentation will analyze the epistemic and ethico-political significance of such technology ‘constructions’ endeavours in the pages of scientific journals.

Science and societal problems

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Science is supposed to produce knowledge that is empirically verifiable, reproducible, and allows for the explanation of regularities and the prediction of events in the world. Because of its reliability, scientific knowledge holds great potential for addressing societal problems.

In the past decades, this potential is increasingly recognized. There has been a growing expectation, or even pressure, for science to contribute to solutions for societal problems. At the same time, examples have been surfacing where scientists tried to contribute to real-world problem-solving, or others use their findings to do so, and the results were disappointing.

For example, in 2006, commercially kept bee hives started to die off at alarmingly high rates. Toxicologists were asked to determine whether 'imidacloprid', a pesticide that had recently been brought on the market, could be responsible for the collapsing beehives. The scientists performed a randomized control experiment where hives were kept in a controlled environment for three weeks, and then observed for several months (Dively et al., 2015). The study found no significant correlation between exposure to imidacloprid and beehive collapse. However, beekeepers argued that the results were useless. First, because the study did not account for cross-reactions with other synthetic chemicals and pathogens, and second, because the natural living conditions had been distorted by replacing the natural diet of the bees with artificial pollen and housing them in combs coated with chemicals (Suryanarayanan & Kleinman, 2017).

In this talk, I reflect on what it means for science to contribute to solutions for societal problems and how it can do so. The case of beehive collapse is used as a working example.

First, I address the ambiguity of 'science' in calls for "science for society" by mapping different understandings of science and scientific knowledge.

Next, I explore different ways in which science can contribute to real-world problem-solving. I zoom in contributions to two aspects of problem-solving processes: (i) explaining events or phenomena to develop a better understanding of the problem, and (ii) determining interventions by means of which the problem can be solved.

Finally, I outline a strategy for formulating scientific research questions that facilitate contributions to real-world problem-solving. The strategy builds on the 'PICO' model, which is used to guide the formulation of research questions in evidence-based medicine. The acronym points to different variables that need to be specified in research questions: 'Population', 'Intervention', 'Control' and 'Outcome' (Richardson et al., 2002).

Depending on the societal problem being addressed, variants of PICO can be used to mediate the production of useful knowledge via strategically formulated research questions.

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Distributive creativity, epistemic environments and scientific progress

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This paper approaches scientific progress as an activity occurring in an epistemic environment through distributed creativity—a relational feature of scientific results. It emphasizes the role of practice and products for scientific progress.

In recent decades, scientific progress has become a mainstream topic in both philosophy and sociology of science. However, as Dellsen suggests, it has been associated with scientific theories and their abilities to accumulate knowledge ("epistemic" progress, in Bird), solve problems more efficiently (the "pragmatic approach" of Laudan, Kitcher, etc.), get "closer to truth" (the "semantic" ap- proach, due to Niiniluoto), or contribute to our understanding of the world (the "noetic" approach see Dellsen, Bangu, etc.). The pragmatic approach also offers a functional component of progress: to solve multiple problems in a "useful" way (Shan, Douglas). In this sense, scientific progress is an activity resulting from scientific practice.

In line with the pragmatists and Shan's functionalist approach to progress, the current paper associates scientific progress to the way a scientific result (as a scientific product, see Cartwright et al.) modifies the dynamics of an 'epistemic environment' (rather than theories) characterized by a 'landscape' (Weinsberg& Muldoon). The two processes under scrutiny here are discovery and problem-solving. The present argument relates them to scientific creativity as a distributed property.

How do we relate discovery to creativity? Discovery is traditionally defined as a creative and primarily unconscious process related to the intuition or insight of an individual scientist (Weisberg, Popper, Laudan, etc.). Others dissented and considered scientific discovery a rational and logical process (Nickles, Simon, etc.). However, more recently, a "new wave" of approaches to scientific discovery reframes discovery in an evolutionary framework or as a problem-solving process (Ippoliti, Nickles, Cellucci). This paper takes discovery as a process occurring within the epistemic environment.

Problems thrive in such epistemic environments, and scientific "products" are designed to solve them. In this variant of the Weisberg-Murdoon model, this problem-solving is an activity distributed among different points of the epistemic landscape.

This is where creativity enters the present argument: the project is to evaluate the advantages of rethinking creativity as (i) a distributed property over the pair of a scientific result and an epistemic environment (rather than a psychological skill of an individual scientist), and (ii) strongly related to discovery.

Then, a scientific product is creative when it entertains the dynamics of the scientific environment towards a goal over more extended time periods. This goal can be problem-solving or a more epistemic or noetic goal. Rather than being the attribute of a lone genius, this paper discusses creative scientific products. The idea is to take creativity as a distributed property over scientific products (results, ideas, proofs, etc.) and multiple epistemic environments. In this interpretation, creativity operates as a "feedback loop" within the dynamics of a scientific discipline. The result is an increase in the reliability of the epistemic environment.

As a case study, we discuss an interesting possible consequence of this approach: the ability to accept the “artificial” creativity and discovery in science (algorithms, machine learning, AI, robots) as a collaborative activity among collectives of humans and artificial advisors. We briefly discuss the case of protein folding as a problem solved by algorithms such as DeepMind (Callaway).

To conclude, distributed creativity as a dynamic factor of scientific progress adds to the existing literature on progress and creativity.

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Dynamic Monism for Biological Function

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Function has been a central topic in the philosophy of biology for more than half a century. Since the 1960s, various ‘theories of function’ have been proposed. Discussions between proponents of these views often end up turning on nothing more than the “dull thud of conflicting intuitions” (Bigelow and Pagetter 1987: 196). Is digging, for example, a function of turtle flippers if it is important to their reproduction even if they haven’t been selected for it? Usage in the scientific community doesn’t seem to settle such questions and intuitions differ. Thus, various alternatives have been proposed to the project of finding the one correct definition of function for biology. Most philosophers of biology now endorse some form of pluralism. They accept that function is used with different senses within biology and that different theories of function account for different senses.

I propose an alternative approach: Dynamic Monism. This account can be seen as a meta-theory of function: a theory about theories of function. I draw on Ludlow (2014) concerning the Dynamic Lexicon and the underdetermination of meaning. I argue that there is one skeletal ‘core’ meaning of function as used within the life sciences. This skeletal meaning is largely constituted by a few canonical judgments, such as ‘hearts have the function of pumping blood’. It allows function to be applied to new cases through implicit or explicit analogy.

The skeletal meaning is heavily underdetermined. For many cases, it does not determine whether ‘function’ applies. Hence, scientists implicitly and explicitly sharpen it. They do so, primarily, by adding new canonical judgments, such as ‘making a thumping noise is (/is not) a function of hearts’. In different contexts, the meaning is sharpened differently. While reducing the underdetermination of function's meaning, such sharpenings never completely eliminate it. There will always be cases that cannot be decided based on the agreed-upon canonical cases.

Theories of function, like Cummins-style accounts, selected-effect accounts and organisational accounts of function, are also ways of modulating the meaning of function. Rather than introducing new canonical judgments, they introduce an explicit rule according to which function is to be applied. Thus, they further reduce its underdetermination.

Theories of functions serve various roles. They allow us to categorise different ways of modulating the meaning of function by adding canonical judgments according to which theories of function they are compatible with. They further allows us to clarify (apparent) disagreements in which scientists use function with differently modulated meanings. Finally, they allow us to justify certain practices in science. For example, the selected-effect account vindicates the practice of explaining the existence of traits by appealing to their function.

I will contrast Dynamic Monism, with other meta-theories of function implicit in the literature: Hard Monism, Fuzzy Monism, Hierarchical Monism, Between-Discipline Pluralism and Between-Discipline Pluralism. I argue that only Dynamic Monism can account for the diversity in which function is used in successful biological practice, and avoid making prescriptions that are likely to hinder the future development of function-discourse in biology.

Of Opaque Oracles - AI poses no new problems for social epistemology of scientific knowledge

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In modern collaborative science, relations of trust between scientists are often regarded as both ubiquitous and unavoidable. Since researchers regularly face practical limits of double-checking the work of others, they need to trust in the integrity of their peer's work in order to employ it in their own. In a recent paper, Inkeri Koskinen (2023) coins this general position widely held in the literature of social epistemology as the necessary trust view (NTV). According to her, the NTV has important implications for the usage of scientific instruments, e.g. microscopes, DNA sequencers, or NMR spectrographs. Here, she argues, scientists trust in the integrity and expertise of the instruments' engineers, which subsequently licenses them to depend on these instruments for their own epistemic work even without understanding every detail of how the instrument works.

With artificial intelligence (AI) systems in general and deep neural networks (DNNs) in particular, a new class of potent instruments has recently emerged and been made available for diverse scientific use cases. Groundbreaking applications range from microbiology to neuroscience to cosmology and many more. While the potential utility of such systems has been evident, concerns have been put forward regarding the epistemic status of these systems since DNNs are often considered to be 'black boxes' whose inner workings are unintelligible to human investigators. Koskinen (2023) claims that scientists' epistemic dependence on black boxes in scientific practice challenges the consensus of the NTV. Specifically, she argues that since DNNs are essentially epistemically opaque, the NTV cannot account for scientists epistemically depending on DNNs. Accordingly, if in principle there cannot exist someone who understands how a given DNN works, then there cannot be a trustee who takes responsibility for it in an epistemically acceptable way. Trust, as the customary way of managing relations of opaque epistemic dependence, is therefore not available in the case of this newfangled kind of opacity.

This paper offers a critical response to Koskinen's argument. After first defending her point, I will subsequently attempt but ultimately fail to find supporting examples of her argument in scientific practice. Instances of current AI use in science appear to be used in largely epistemically unproblematic ways that pose no special problems for the NTV. At best, her argument thus applies to a highly limited subset of use cases. Second, I construct a thought experiment of AI use in an attempt to better capture the conditions of epistemic dependence on AIs Koskinen has in mind. I conclude that Koskinen assumes too strong of a version of the trust view and proponents of it need not agree with Koskinen that trust is necessary in every case of opaque epistemic dependence. Another option I propose is gauging 'whether' a given system is reliable, which matters more for epistemically responsible use than knowing 'why' a system is reliable. This option is compatible with the weaker trust views usually defended in the literature of social epistemology and compatible with reliance on other 'classical' scientific instruments that are not AI. I close with speculations about whether future developments in practical use of AI in science may challenge this conclusion.

Using Scientific Knowledge to Democratize Authoritarian Societies

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The interplay between science and society is usually examined in the context of Western democracies, the focus being on the democratic engagement with science, commercial and political pressures upon science, and the proper place of science in democracies. This perspective ignores that significant research is done in anti-democratic societies, where science-society interactions are different than in democracies. I examine the public pronouncements of scientists about the identity of the language spoken in the Moldavian Soviet Socialist Republic (MSSR) and argue that they used their scientific knowledge to help democratize this authoritarian society.

For geopolitical reasons, Soviet authorities forbade any public claims that the language spoken in the MSSR, officially called Moldovan, was identical with Romanian, spoken in the neighboring Romania. To further mark the distinct character of Moldovan, Soviet authorities mandated that Moldovan uses the Cyrillic alphabet, whereas Romanian used the Latin script. Despite political pressure, a few linguists attempted to assert in academic journals the Moldovan-Romanian identity, yet censorship blocked their attempts. In the mid-1988, a group of linguists, natural scientists and other intellectuals, published an open letter that challenged authorities to accept what linguists understood as "the scientific truth": Moldovans speak Romanian and the Latin alphabet should be adopted. They also demanded that the language of the Moldovans become official to prevent its total Russification and challenged great-Russian supremacy. To support these causes, linguists published dozens of progressively more courageous articles in the press publicizing their arguments. Rallies began taking place and their speakers publicized linguists' arguments. Eventually, opposition of Soviet authorities and the authoritarian regime broke down.

To assess the significance of how linguists used their scientific knowledge and the support of the natural scientists, one needs to consider that none of conditions of democracy existed in the mid-1988 in the MSSR. Saying in public what has been unsayable is a key manifestation of the freedom of expression. Linguists' numerous publications in the press offered citizens sources of information alternative to the official view and allowed them to have an enlightened understanding of issues of public interest. This ensured citizens' equality with government officials in the first public debates. Linguists also initiated and controlled the public agenda by raising the language issue and participating in public debates and offering arguments for them. Furthermore, they also asserted independence from political control of science as an institution when they challenged Soviet authorities to accept the scientific view and not to use political reasons to suppress it. By challenging great-Russian supremacy, linguists helped ensure equality of citizens of all ethnicities. Thus, the use of scientific knowledge in public realized some of the conditions of democracy and helped pave the way to further democratization.

I show in conclusion that the lesson of this case is that scientists can contribute to democratization of authoritarian societies by finding ways to assert the scientific truth even when it is politically prohibited, to assert it in public, to challenge authorities to respect it and the autonomy of science as an institution that checks the other branches of power, and to assert and defend human rights.

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Science and Value: An Empirical Analysis

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Introduction

Normative philosophical discussions of science and values tend to reflect theory-driven approaches focusing on objective and subjective goals for science. Our project intends to supplement this philosophical discourse on values in science with a bottom-up, empirical analysis of scientists' responses to survey data about the nature of science.

Overview

The primary dataset for our project was collected by Brandon Vaidyanathan et al for their project ""Work and Well-Being in Science: An International Study."" In addition to collecting survey data from nearly 3500 scientists in four countries (India, Italy, UK, and USA), Vaidyanathan's team collected additional qualitative data through follow-up interviews with two hundred and fifteen survey participants about a range of topics, including how participants defined "science," and the role, if any, that the concept of beauty played in their scientific work. Our results clearly show that scientists' themselves understand science as a value-laden enterprise. Some values are seen as having epistemic significance, whereas other values are motivational and part of the context in which scientists approach and undertake their work.

Methods and Results

Correlated topic modeling was used to summarize the text of interview transcripts. With respect to defining science, results indicated prevalent themes related to scientific objectivity (the importance of science for describing the world, the need for using appropriate scientific methods) and themes related to one's individual values (one's personal connection to/love for science, the good of improving society). We further probed the use of value-laden language by applying the personal values dictionary. Results indicated substantial variance in the use of value-laden language, with the most prevalent values in scientists' definition of science were self-direction (independent thought and action), universalism (the welfare of all people and for nature), achievement (personal success through demonstrating competence), and stimulation (excitement, novelty, and challenge).

With respect to scientists' understanding of how concepts of beauty intersected with their work, topic modeling revealed three themes endorsing the value of beauty in science. One theme involved equating beauty and elegance with simplicity and clarity which aids science communication and pedagogy. A second theme referenced beauty as a guide to truth, with some caveats about possible biases leading scientists astray. A third theme emphasized the motivational aspect of beauty, including the beauty of experiments, theories, and nature.

A follow up study showed that undergraduates (N=2,225) accurately predicted the prevalence of values in scientists' definitions of science. Furthermore, the extent to which students believed science was objective versus driven by subjective values predicted their judgments of scientific credibility. Multiple regression analyses indicated that it mattered less which values drove science ($R2=0.002$) than if science was value-driven in general ($R2=0.064$).

Summary

In addition to normative philosophical reflection on values and science, empirical analysis shows that regardless of whether science should be value-laden, scientists themselves are value-driven. The values held by scientists shape how they view the scientific enterprise and influence their perceptions about the role beauty plays in their work.

Mound Builders and Mound Blunders: 19th Century Archaeology and the Image of Science

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What do we do when science blunders? This is the question at the heart of my talk. To answer this question, I will explore an episode from the history of archaeology commonly referred to as the Mound Builders myth. The myth emerged in America in the late 18th and early 19th centuries as settlers migrated west following the American Revolution and re-discovered human-made earthworks in what are now the eastern states. Many of the earthworks required incredible feats of engineering that involved substantial modifications to the geography and were suggestive to observers of complex civilizations. However, because it was widely believed that Indigenous Americans were inherently uncivilized and thus not intelligent enough to have produced the mounds, another more advanced lost race was invented to account for them. This notion of an ancient mysterious race—The Mound Builders—dominated popular thought for decades. Eventually, the ancestors of Indigenous Americans got the credit they deserved when the myth gave way to proper scientific archaeology in the late 19th century, or so some have claimed.

In his detailed history of the Mound Builders controversy, Robert Silverberg writes: “The myth took root, flourished and grew, even spawned a new religion; then the scientists took over from the mythmakers and hacked away the luxuriant growth of fantasies” (1970, pg. 15). There is a sense here that, in the absence of science, mythmaking was allowed to flourish; once proper scientists became involved the myth was easily undone. If only science was being done all along!

One problem: science was being done, during the Mound Builders “mythmaking” phase and after. In fact, contemporary and modern archaeologists agree that some of the best archaeological science of the 19th century certified the myth. These facts problematize the demarcation as Silverberg has drawn it. What is cast as science versus mythmaking is, in actuality, a complex exchange between emerging archaeological practice—newly focused on site-based meticulous data-gathering, with omnivorous integration of evidence and tools from multiple disciplines—interacting with very traditional elements of theoretical interpretation, scientific consensus forming, and cultural feedback.

There are two reactions to this blunder: On the one hand, we can develop epistemic criteria for science that justify striking the Mound Builders from the historical record (as Silverberg has done) and in so doing cast some of the best archaeological science aside as well. Or, we widen our scope of what is to be deemed scientific and accept that some of the best science significantly erred and was culturally influenced. In so doing, we abandon the search for an epistemic demarcation criterion and rely on the social dimensions of science to do the work. I argue for the second of these reactions as it preserves an image of science that, while uncomfortable, is a more accurate portrayal with an acknowledgment of the reality of scientific blunders. In this way, we can also come to understand and monitor the undue cultural influences on science when they occur, as they are expected to.

Reversing Gene Drives: Insights from the Art of Restoration

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Gene drives are a technology designed to bias the inheritance system of wild organisms by altering their genetic makeup, usually with the aim of serving some human purpose—e.g., eradicating malaria. This is unprecedented. Typically, a trait that does not provide some advantage to an organism—or to a population of organisms—doesn't spread, but gene drives can propagate a trait neither for the benefit of the organism, nor for the benefit of its population, but for the benefit of humans. Indeed, humans can now engineer a specific trait and drive it into a population of organisms and do so entirely for our benefit. Even so, predicting whether a gene drive will function as intended is challenging. In recognition of this challenge, researchers have also engineered reversal drives, which use the same technology to drive the original trait back into the population. If a gene drive falls short of its intended effect, reversal drives can put things back to how they were prior to the intervention.

But what does it mean to say that gene drives are reversible? This question invites philosophical reflection. How should we conceptualize reversibility as it applies to gene drives and what is it that we hope reversal drives will restore? Finding answers requires identifying what we want these drives to restore, and that hinges on understanding the value of what we are trying to bring back. Once this foundational understanding is in place, the challenge lies in ensuring that our chosen method of restoring lost value doesn't inadvertently undermine what we value. At any rate, I will argue that there are better and worse ways of understanding the meaning of reversibility as it pertains to gene drives and that the values used to guide to process of art restoration can offer guidance. Indeed, what restorationists care about when restoring damaged art aligns well with what researchers developing reversal drives should care about.

The presentation will unfold as follows: I will begin with a brief overview of gene drives, emphasizing the inherent uncertainty in predicting their ecological effects; next, I will delve into the significance of reversal drives in light of this ecological uncertainty; I will then draw a parallel between ecological restoration and art restoration, arguing that since both rely on similar standards to assess the success of restoration, the principles of art restoration should guide thinking about ecological restoration using reversal drives.

The Epistemic Risks of Regulation, Side Effect Reporting and the Risk of Wishful Speaking

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Medical-pharmaceutical science stands out among other scientific disciplines due to the intricate web of communication among various stakeholders. Communication flows between various stakeholders such as manufacturers, regulators and patients during the phases of approval, marketing and pharmacovigilance (the science of monitoring of drug harms post-approval). In this talk, I aim to add some nuance to the debate about drug harm communication, taking into account patients' interest in drug harms that are considered acceptable from a regulatory perspective in relation to the drug's benefits, but which are nonetheless harmful. In addition, I provide understanding to what feels uncomfortable to many patients when they seek contraceptive counseling.

Drug harms are considered to be underregulated, but also underreported. The inductive risk of accepting the hypothesis that a drug is effective and safe has a strong tendency to be false positive. There is also reluctance to change and communicate judgment when new evidence emerges post-approval. Philosophers have proposed an epistemic risk framework to reflect on the ethical obligations associated with post-approval information about drug harms (Bavli & Steel 2020, Due 2022). They argue that the consequences of the acceptance of new safety data and the corresponding regulation and reporting should be considered in the light of the ""public safety"", as pharmacovigilance serves to protect potential users of interventions. It is assumed that even in cases where the drug harm is generally acceptable and doesn’t require regulatory action, public safety and health considerations should favor providing more, albeit uncertain, information rather than withholding information.

I will argue that there are problems with the application of the proposed epistemic risk framework to information about ""acceptable"" drug harms. By focusing on public safety as the value that pharmacovigilance serves, philosophers tend to mistakenly limit the role of pharmacovigilance to regulatory functions. This conflates post-approval regulatory and reporting decisions. I will use the example of minor adverse symptoms associated with contraceptives to show that this two-in-one balancing of risk can support reporting decisions that raise ethical concerns, such as wishful speaking (John 2019), rather than promoting ethically informed reporting as intended.

When we use regulatory decisions as the basis for reporting decisions about drug harms that we don't intend to regulate, we allow our reporting decisions to be influenced by assessments that do not privilege truth over falsehood, but are solely depending on evaluative risk-benefit assessments. I propose to base the epistemic risk assessment of non-regulatory drug harm information on the intentions associated with an intervention, rather than on public safety which is already reflected in the regulatory decision.

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A cross-cultural study on the ethics and moral psychology of HPV vaccination

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Every year, more than 300,000 women die from cervical cancer worldwide. Human Papillomaviruses (HPVs) are one of the main causes of this and other types of cancer. A vaccine against HPV, recommended for children and teenagers, can prevent future infection and its consequences. Although this vaccine is effective and safe, vaccination rates remain well below the 90% target set by the World Health Organization for 15-year-old girls. Despite the abundant literature about vaccination determinants, there has been less exploration of the moral dimension of vaccine intention and vaccine hesitancy and its relationship with other factors, such as informedness, risk appraisal, or trust in doctors and medical institutions.

Here, we present a cross-cultural survey performed in Germany, the United States, and the United Kingdom to study the factors influencing parents' intention to vaccinate their children against HPV, with a focus on the ethics and moral psychology of vaccination. In the three countries, parents reporting stronger intention to vaccinate their own children rated vaccination as ethically better and expected more protection from vaccines for their own and other children. They also were more likely to consider that parents are ethically required to have their children vaccinated for their own benefit and for others. Parents with extreme moral evaluations were more likely to trust their ethical position. Responses to perceived pressure to vaccinate (yielding/conformity and resisting/reactance) differed in the three countries, as well as the correlation between perceived pressure to vaccinate and trust in science, medical professionals, and institutions. However, in the three countries, the negative correlation between risk perception and intention to vaccinate was lower for the parents who trusted medical and scientific authorities than for low-trusting parents, which could indicate an effect of trust mediating this relationship. Finally, our results showed higher correlations between informedness and ethical rating of vaccination than with vaccination intention itself. This analysis supports the hypothesis that ethical arguments and messages, as well as campaigns that jointly target distrust and misinformation about vaccine risks and benefits, could beneficially influence decisions about vaccination. We highlight the need to perform confirmatory and complementary analyses and discuss possible interventions based on these results.

Direct and Circumstantial Traces: Reconstructing Early Life and its Environment

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This paper introduces a distinction two different types of traces that feature in historical science investigations: direct and circumstantial traces, akin to talk about evidence in legal contexts. While existing understandings of ‘trace’ in the philosophy of historical sciences emphasize either their causal (Cleland, 2002), informational (Turner, 2007), or evidentiary nature (Currie, 2018), they agree that a trace needs to be directly downstream of a given long-past event under investigation. I argue that this misses an important type of trace used in historical reconstructions. Existing views of traces focus on what I would propose to call direct traces. By contrast, what I call circumstantial traces (i) share a common cause with the historical event under investigation, and (ii) allow an inference to (features of) said event via an additional inferential step. This enables key insights into the wider historical and environmental context. I highlight that an important part of evidential reasoning in historical science is checking for alignment between direct and circumstantial traces, especially when direct traces are ambiguous.

I develop my understanding of direct and circumstantial traces in reference to debates over the provenance of putative traces of early life. Determining what the very first forms of life looked like, and which environments they inhabited is crucial for understanding early stages of evolution and might yield insights into the origins of life. However, traces of early life are scarce, minute, and often ambiguous with respect to their biological origin. This is both because abiotic processes can mimic traces of early life, and because putative traces of early life are easily transformed, altered, or contaminated by subsequent processes. I focus on two case studies investigating putative stromatolites, sedimentary structures caused by the activity of ancient microbes, in different locations. I contrast what is taken to be a very strong case for the presence of early life – the stromatolites of the Strelley Pool Chert Formation in Western Australia – from a more heavily debated case – the finding of stromatolite-like structure at a location in Greenland called the Isua Greenstone Belt. In both cases, what I call circumstantial traces were essential for establishing whether the rock structures resulted from early life form’s activities.

Values in Psychosocial Measurement

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This work is based on my participation over three years in a collaboration between philosophers of science with health-outcome researchers and mental health clinicians at a youth health services organization in British Columbia, Canada. My work shows how non-epistemic values play a constitutive role in modulating the purpose of use and interpretation of measurement outcomes within a given context. I argue that the development and use of measures is overly rigid. What is required is a non-standardized, dynamic measurement practice that can adjust for various purposes in light of shifting contexts and values that ought to be constantly reassessed. I characterize the elements of a 'measurement practice' and point to a procedure that will help ensure the continued assessment of values in psychosocial measurement that I call 'ethical iterations'. The outcome of ethical iterations is a measurement practice that is fit for its purpose.

Health assessment questionnaires are routinely used in a variety of contexts from drug development research to patient-clinician interactions. My case study focuses on the use of patient-reported outcome measures (PROMs). A PROM is a kind of health related outcome measure that, "…is any report of the status of a patient's health condition that comes directly from the patient, without interpretation of the patient's response by a clinician or anyone else" (Food and Drug Administration, 2009, p. 2). PROMs have received some philosophical attention of late given their touted dual epistemic and ethical promise. They appear to focus on what really matters to patients regarding their own health by focusing on the health aspects that patients have themselves identified as important, an epistemic gain. And they appear to give voice to patients by respecting their autonomy in expressing and interpreting their own state of health, an ethical gain (McClimans, 2024).

Philosophers have noticed and worried about how to make explicit, or adjudicate the value-ladenness of psychosocial measurands (often called constructs) and their methods of measurement in psychometrics (the science or measuring mental traits and attitudes) more broadly, and in PROMs and health-related quality of life (HRQL) outcome measures in particular (Alexandrova, 2017; Alexandrova & Fabian, 2021; McClimans, 2010, 2017; Rodriguez Duque et al., 2023). More recently, philosopher Cristian Larroulet Philippi has argued that the quality of any measure cannot be made sense of without reference to a given purpose (Larroulet Philippi, 2021).

Eran Tal argues (in progress) that psychosocial measurement is best viewed as a 'social technology.' As such, constructs and their methods of measurement (e.g. the questionnaires) intervene on people and society by virtue of their design and use. Measures such as PROMs do not simply extract information from those being measured to reveal the degree of some property inherent to the patient. Instead, measures intervene by communicating values and goals, and affect the therapeutic outcomes of patients (Truijens et al., 2022). I analyze the intrinsic role of non-epistemic values in the operationalization of psychosocial measurands and their methods of measurement and the importance of assessing their role in the measurement practices that create such interventions.

Scientific Practice and a Fourth Dogma of Empiricism

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Quine's (1953) challenge to empiricist dogmas of analyticity and reductionism helped inaugurate a turn toward naturalism and pragmatism. Davidson (1984, essay 13) discerned a third, supposedly final empiricist dogma as a dualism of conceptual schemes and their empirical content. Work on science in practice displays and undermines a more encompassing fourth dogma dividing an anormative nature from normative concerns introduced by human rationality or social practices. This fourth dogma is inscribed in familiar presumptions that modern sciences understand nature as normatively inert and bereft of human significance. Widely familiar studies of scientific practice and scientific understanding instead show the natural/normative dualism as specifiable only with reliance on what each supposedly excludes. Understanding this work as a general challenge to a residual empiricist dogma highlights its broader philosophical import.

Philosophical efforts to understand nature as anormative typically take three forms: laws, natural kinds, or causal structures. Davidson, for example, specified the natural world as a closed, "homonomic" system of laws. Work on scientific practice challenges such conceptions from two directions. First, traditional conceptions of natural laws have often been replaced by a plurality of models, shaped by human purposes, cognitive capacities, and idealizations (Giere 1988, ch. 3; Teller 2001; Woody 2004; Wilson 2006; Weisberg 2013). Second, lawfulness itself has been understood as domain-constitutive norms of reasoning (Lange 2000; Haugeland 1998, ch. 11, 13; Rouse 2015, part II). Laws differ from accidents not singly, but only as collectively comprising interdependent patterns which remain mutually invariant under all counterfactual suppositions consistent with one another. Moreover, their invariance in turn depends on norms of experimental or observational practice in the relevant domains, which then incorporate more encompassing norms of inductive or epistemic risk (Douglas 2000; Biddle and Kukla 2016).

A second route to identifying the natural world as anormative appeals to an ontology of natural kinds. The sciences, however, classify natural entities in diverse ways, differing according to scale (Bursten 2016), irreducibly cross-classifying levels (Dupre 2021), or purposes (Chang 2022, ch. 3), suggesting that 'natural kind' is not an anormative kind (Hacking 2007). Amid this relentless, overlapping pluralization of natural kinds, those who still defend an anormative conception appeal to their causal role (Khalidi 2023) or their recognition in natural laws (Lowe 2006).

A third route to identifying nature as anormative appeals to causal relations, with a massive literature on causal modeling and causal metaphysics. Causal models nevertheless require appropriate specification of causal background or boundary conditions, in different kinds of causal systems and causal roles (Cartwright 2004; Hitchcock 2003), or embody conceptual norms specifying causal reproducibility (Barad 2007). Causes are irreducible to conditional probabilities, because they must condition on other causes (Cartwright 2004). Causes are thereby unpromising bases for the intelligibility of nature as anormative.

Philosophy of scientific practice thereby undermines dominant "scientific" and "liberal" conceptions of philosophical naturalism (de Caro and Macarthur 2010), which rely on the fourth dogma. The task then remains to articulate conceptions of the natural world and philosophical naturalism consistent with our best accounts of scientific understanding in practice.

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No Cause for Concern: Why even indefinitely many potential confounders do not undermine randomized experiments

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John Worrall has argued that randomization in clinical trials does not perform the most important function often ascribed to it: to balance known and unknown confounding causes evenly between an intervention group and a control group. While it is true that randomization would, on average, distribute all confounding causes evenly between intervention and control if we repeated the experiment an indefinite number of times, any actual study is randomized only once. And since any individual randomization can produce an imbalance of confounding causes as well as a balance, there is no reason to regard randomization as a guarantee for the validity of individual experiments. What is more, Worrall argues that the number of potential confounding causes in most experiments is indefinite. And the more potential confounders there are, the more likely it is that at least one of them will be meaningfully imbalanced. Thus, Worrall argued, randomization has no unique epistemic power and does not ensure the validity of any individual experimental inference. Worrall's critique has elicited an extended philosophical debate about the nature of a "balance" of potential confounders in experimental trials, the necessity of such balance for valid inferences, the role of randomization in promoting or even ensuring balance, and -- more generally -- the power of randomization in experimental science. In this contribution we argue that the debate has largely neglected the actual practice of the statistical analysis of randomized experiments. We show that our routine statistical procedures do not require balance. Instead, they quantify the influence of confounding causes -- even of unknown confounding causes -- on the outcome measured in an experiment. On this basis, they can then quantify the probability that an estimated intervention effect could be produced by confounding causes of the measured magnitude. We show that this procedure is applicable to individual experiments. In addition, the statistical procedures are structured such that the threshold for inferring causality is higher in systems in which the influence of confounding causes is more pronounced. Because of this property of our inference procedures, worries about an indefinite number of potential confounders are unwarranted. We have created simulations of randomized trials in the R programming language to show that even as the number of potentially confounding causes increases, the proportion of invalid inferences does not increase. Thus, it is not required for confounding causes to be balanced; randomization's purpose is not to create balance; individual randomized experiments can be evaluated rigorously; and even indefinitely many unknown confounders do not threaten the validity of our inferences. We offer this debate as an instance in which philosophical confusion has been created by the neglect of scientific practice.

Scientific explanations in the wild: on learning from case studies in the philosophy of science in practice

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In recent discussions, there is wide agreement that philosophy of science should be rooted in actual scientific practice. This trend is, perhaps, particularly evident in discussions around scientific explanations, where philosophers are moving away from idealized textbook examples to scrutinize real-world scientific explanations through an in-depth engagement with case studies across various fields.

However, the extensive use of case studies has elicited concerns regarding the risk of bias and overgeneralization. Drawing on a concrete example, this paper addresses such concerns and underscores how case studies can play a valid and relevant role in philosophical examinations of science. The example chosen comes from the field of molecular biology and concerns the biological function of aquaporins. Aquaporins are channel proteins located at cell membranes. Whereas aquaporins allow for a rapid flow of water molecules through membranes, they strictly hinder protons from passing. Despite the electrochemical importance of proton exclusion, the biological function of aquaporins gave scientists an explanatory puzzle, leading to vivid discussions in the respective community. Various scientific groups using different methods and tools undertook efforts to explain the selectivity of aquaporins.

Looking at the example of aquaporins, I here examine the role of case studies in the philosophy of science. Building on a proposal by Hasok Chang, I suggest that rather than acting as pieces of evidence for inductive generalizations, one way in which case studies can figure in philosophical examinations is as resources for abstraction. Based on a discussion of the aquaporin example, I further spell out this idea and demonstrate how detailed analyses of patterns of scientific reasoning can offer nuanced insights into explanatory practices. I demonstrate how the suggested perspective shifts the focus away from searches for general theories of scientific explanation, and rather towards an examination of the reasoning schemes and tools that figure in certain explanatory practices. I conclude by outlining how the suggested perspective on case studies also contributes to a more comprehensive picture of normativity within the philosophy of science in practice.

The Value of "Research" in Osteopathic vs Allopathic Medical Education: A conceptual analysis in context

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Like science more generally, medical research is heavily influenced by the values and interests of institutions, funding organizations, and the contingent features of the complex, temporally and socially situated environment in which it functions. A striking example of this can be seen in the changing role of medical research in the average, everyday practice of medicine, following the introduction of “evidence-based medicine” (EBM). In the relatively short period of time since the early 1990s when it was introduced, EBM has not only had a substantial impact on the focus and nature of medical research, but on the role of research in medical education, as well.

Many questions and concerns regarding the uncritical acceptance of EBM in standard practice have been acknowledged and addressed elsewhere. Less attention, however, has been devoted to evaluating the potentially disparate role and impact EBM has had on the two established schools of medicine in the US: the more familiar, “MD” degree-granting, allopathic medicine, on the one hand, and the less ubiquitous, “DO” degree-granting, osteopathic medical practice, on the other. These two schools of medicine both produce practicing, state-licensed physicians, and therefore overlap considerably in their content and curriculum. The underlying values and theoretical approach to medical practice and patients that these schools employ, however, diverge considerably.

Additionally, there is a notable divergence in the participation and production of medical student research in each of these schools of medicine; one recent study, for example, estimates that 43% of recent MD graduates produced PubMed-indexed publications, in comparison to 24% of recent DO graduates. From the perspective of an EBM-oriented approach to medical practice, this disparity might be viewed as concerning, and potentially could be construed as a deficiency in DO training and/or physicians. However, to conclude this would suppose that research is not just valuable – but necessary – in medical education.

In this presentation, I briefly outline the evolution of research as an activity in medical education, generally, as well as the stated values and mission of each of the competing schools of medicine. I provide an analysis of the concept "research" – of its role, function, and referents – in medicine and medical education. I move on to compare the results of this analysis to the values and mission of allopathic and osteopathic schools of medicine, suggesting that in its current, preferred form, "research" is more suited to an allopathic approach to medical practice. I leave as an open question, however, whether this suitability is good.

How to Get Pregnant (Or: Pregnancy, Methodology, and Metaphilosophy)

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Reflective equilibrium (henceforth, RE) is an iterative process of negotiating between scientific evidence and scientific theory to come to an improved understanding of a phenomenon (Cummins 1998, 113; see also Goodman 1955). And Suki Finn argues that RE, if done well, ought to be the methodology used in our philosophical inquiry into pregnancy (Finn 2021). But RE, as Finn describes it, risks circularity. This is because no independent criterion is provided for distinguishing a conception of pregnancy from a theory of it—for distinguishing explanandum from explanans. Call this the problem of target-theory collapse (henceforth TTC), discussed, though not named as such, by Robert Cummins (1996; 1998) and by Joseph Rouse (2015, 229-231). TTC occurs in Finn's account because none of the features, such as calibration, that prevent TTC in the scientific context, are meaningful in the philosophical context. And unlike Cummins who describes and evaluates RE differently in these contexts, Finn draws the dialectic of RE across science and philosophy. For RE to be a sound methodology we must prevent TTC. This requires engaging broader questions in the philosophy of science which regard the nature and purpose of scientific practice—questions which Finn sets aside (2021, 6-7, see especially Footnote 15).

In what follows I engage those questions in order address TTC, reformulate RE accordingly, and vindicate pregnancy's role as a useful case study for methodological questions. While Finn notes that RE "may not answer all our questions" about pregnancy (2021, 15), I aim to show what RE must be like to answer at least some questions meaningfully. What's at stake in preventing TTC is retaining, and possibly expanding, our ability to intervene in pregnancy for purposes of either starting, sustaining, preventing, or ending it. My discussion, therefore, is framed by the following background assumptions: (1) that a methodology should include a holistic and multidisciplinary understanding of pregnancy (the synthetic assumption), and (2) that a methodology should enable our interventions into pregnancy, thereby enabling reproductive agency (the feminist assumption).

The paper proceeds as follows. First, I provide some definitions and brief discussion of my assumptions (Section II). Then, I furnish the requisite background for understanding Finn's discussion of pregnancy and her methodological proposal (Section III). I critique Finn's version of RE and argue that it risks TTC, and I show how this problem undermines the proposed methodology (Section IV). Next, following Rouse, I propose an understanding of target systems (henceforth, target(s)) as salient patterns, which are recognized patterns that are significant for scientific practice and that are conceptualized in a dually normative way (Rouse 2015, 232); this prevents TTC. Finally, I develop RE*, a new version of reflective equilibrium. This version prevents TTC and vindicates pregnancy's role as an informative case study for methodological questions across science and philosophy (Section V). In Secion VI, I conclude.

A comparative computational analysis of epistemic markers in astrophysics and particle physics using contextualized word embeddings

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The roles and epistemic characteristics of observation, experimentation and simulation have been discussed not only by philosophers of science but also by scientists themselves. Inspired by recent empirical work on the actual usage of epistemic concepts in science (Malaterre and Léonard 2023; Mizrahi 2022; Overton 2013), we compare how the concepts of observation, experimentation, and simulation (and their associated terms) have been used in astrophysics and high-energy physics (HEP) over the last three decades. A comparison between these two fields is apt, since astrophysics and HEP seem to differ in their epistemic strategies, especially relating to the concepts we investigate (Heidler 2017). The increasing use of simulations in astrophysics has been interpreted as a way to circumvent the challenge of having limited access to experimental data, which triggered a lively debate on the question of whether or not simulations are experiments (Ableson 2023; Jacquart 2022). HEP, on the other hand, has been said to be characterized by a "relative loss of the empirical" due to indirect measurement (Knorr-Cetina 1999) as well as an increasingly interconnectedness between theoretical, experimental and simulation models, resulting in a model-based characterization of the theory-ladenness of experimental results (Karaca 2023; Morrison 2015). All of this suggests that we might find different meanings of these epistemic concepts in the two fields. At the same time, Elder's (forthcoming) recent analysis of the distinction between indirect and direct observations suggests that there are important similarities between the epistemic strategies of astrophysics and HEP. At any rate, in all these debates, the meanings of concepts such as observation, experiment and simulation, are considered good indicators, or "markers", of the epistemic strategies used in astrophysics and HEP, and a systematic comprehensive survey suggests itself. As an initial step towards such a survey we trained a large language model on more than 600K articles from astrophysics and particle physics, uploaded to the arxiv preprint server since the late 1980s, and extracted contextualized word embeddings from it. With these embeddings we will assess semantic similarities and differences in how the epistemic markers were used in specific contexts. In particular, we explore how the nuances of observation, experimentation and simulation have undergone shifts over a 30-year period, spanning from 1992 to 2022. Despite our basic confidence in the fruitfulness of our computational and AI-based approach, we also critically discuss its applicability and its usefulness for the future of an empirical philosophy of science.

Standpoint Theory & Science Policy: Indigenous Peoples and Wildfire Management

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The Province of British Columbia recently passed Bill 31 – 2023: Emergency and Disaster Management Act. This bill, among other things, establishes a federal requirement for “consultation, engagement, and cooperation” with Indigenous peoples when making certain emergency and disaster laws and policies. In this paper, I will argue that such a requirement will yield both better science and more just policies and should be widely adopted by other governing bodies. More specifically, I will argue for a standpoint approach to the role of Indigenous peoples (and other marginalized groups) in emergency and disaster management policies and laws.

Feminist philosophers of science have convincingly shown that more diversity among scientists yields more empirically successful science (Harding, 1991, 2015; Intemann, 2010; Kourany, 2010; Longino, 1990; Solomon, 2007). A greater number of perspectives and backgrounds allows for both a wider range of ideas to be proposed and for assumptions made by dominant groups to be challenged by other groups, leading to more (socially) objective outcomes. This has been shown in several contexts, from primatology to research on gender differences to archaeology. Recently, there has been growing acceptance of the beneficial role collaboration with Indigenous peoples can play in Western science (Whyte, 2013; Whyte & Cuomo, 2016; Wylie et al., 2020). This can be seen in controlled burns and wildfire management (Avitt, 2021; U.S. National Park Service, 2023).

Indigenous peoples in the United States have been doing controlled burns – deliberate burnings of a particular species and/or areas – for thousands of years and are called prescribed burns or cultural burns depending on their purpose. Unfortunately, colonizers often saw these practices as harmful, suppressed them, and sometimes outlawed them (Schelenz, 2022). It wasn’t until the 1940s-1960s that the usefulness of controlled burns was reconsidered. Mainstream sources, such as the U.S. National Park Service, now recognize that Indigenous peoples implement controlled burns “to clear areas for crops and travel, to manage the land for specific species of both plants and animals, [and] to hunt game” (U.S. National Park Service, 2023). Some tribes, including the Lakota, Salish, and Pend d’Oreille tribes, have long used controlled burns for the explicit purpose of protecting themselves against large wildfires.

This story is not surprising when viewed through a standpoint lens. The Indigenous peoples in the U.S. have lived in their environment for thousands of years, giving them a unique standpoint, allowing them to gather immense amounts of knowledge about the land and how to live with it, including how to prevent large wildfires through controlled burns. Starting from the Indigenous peoples’ knowledge when making wildfire management policies and laws makes sense from a scientific perspective as they are the community with the most relevant situated knowledge. It also makes sense from a justice perspective, as marginalized communities are the ones most affected by emergencies and disasters (Zack, 2009, 2012). So, starting from the perspectives, values, concerns, and knowledge of Indigenous peoples for wildfire management policies will not only yield more scientifically accurate practices, but also more just outcomes.

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Pragmatic Pluralism

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Many pluralists have argued convincingly for the benefits of plurality in science, showing how having multiple systems of practice allows scientists to explore and explain different aspects of phenomena. But how do these benefits play out in practice and how do different systems of knowledge come together to address particular questions or issues?

As we saw in the covid-19 pandemic, this consideration is vital in times of crisis where policy makers rely on scientific expertise to inform their decisions. In that context there is a need to appeal to the right set of expertise and make best use of available resources to overcome the crisis. However obvious this statement might seem, I will show that it raises many questions about the way we think about the benefits of plurality in practice when we consider the wider social, economic, and political concerns. One might accept the epistemic benefits of plurality, yet still deem plurality undesirable based on pragmatic considerations. The argument I build here lays out and responds to this potential objection to pluralism on pragmatic grounds. This potentially devastating objection tries to pull apart pluralism and pragmatism, assuming that pragmatic demands can supersede the epistemic benefits of pluralism based on the problems at hand.

However, I will argue that this objection to pluralism fails because it assumes that problems are given and independent from the inquirers. Instead, I will argue that problems are defined by inquirers through a series of judgments. Building on classical pragmatism, especially Dewey's theory of inquiry, I will show how inquirers, when faced with an indeterminate situation, have to make a series of judgements based on what they already know about the situation, informed by their disciplinary tools and wider assumptions. These judgements can lead to different conclusions on what the problem is, depending on who is included in this process. I build an argument for the pragmatic benefits of plurality, where plurality is needed in the community of inquirers making these judgements that define problems.

I will make my arguments concrete in an analysis of the early responses to the UK covid-19 outbreak, showing how the outbreak was initially seen as a biomedical problem by the community of inquirers focusing on biological aspects and neglecting other social, logistical, and psychological aspects. I will show that the lack of plurality among the community of inquirers affected the way the problem was conceived, only considering limited aspects of the outbreak, and relying on a limited set of practices to solve the problem.

Building on this case I show that lack of plurality among the inquirers will lead to the judgements that potentially neglect certain aspects of the situation and overlook the complexities. Plurality will help us study different aspects of the situation and enrich judgements on the problem. I show how pragmatism demands pluralism. Pragmatism shows that plurality must be promoted in practice, building on but going beyond the much more widely recognised epistemic benefits.

Pragmatic Idealism

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I would like to advocate for a position I call 'pragmatic idealism' by using Chang (2022) as a starting point. Not only are many dimensions of Chang idealist, but it is only by pushing him further towards idealism, I argue, that Chang can subvert a dualism that haunts his account. I offer two suggestions: 1) to conceive of concepts as pragmatic acts, and 2) that these acts are coherent in virtue of an ideal norm, which I claim is Kant's synthetic union of apperception.

Chang's revolutionary pragmatic realism subverts many of the major problems of traditional "correspondence" realism. This is due to Chang's Kantian influence. Chang thinks 'realness' must be construed as first and foremost a meaning. To elucidate this, Chang brings in Kant's conception of a priori concepts to explain how we 'frame' our reality. Chang relies on the fact that these meanings and framings are transcendentally 'ideal,' not real: ways nature should be, ideally.

Chang strays from Kant when he divorces these concepts and meanings from pragmatic acts. First, according to Chang, we create a concept to frame some piece of reality. Then, we create conceptually coherent activities relying on this piece of reality framed by the concept. If these activities are successful, then the concept purports to represent reality. This story leads to a question: is the construction of concepts the same kind of act as other 'conceptually coherent activities,' or is it unique? If it is unique, it seems like we must admit a dualism that Kant and Chang seem to want to deny: that there are the constructions that go on in the head and that they are affirmed when we do stuff with them in the outside world. This brings us back to correspondence realism.

To get out of this dualism, we must conceive concepts as the acts of science. If the aim of these acts is to intersubjectively capture some feature of the world, then experimentation, theory design, and measurement are conceptual acts. However, if this is the case, then validation of a conceptual framework boils down to coherence between conceptual acts: measurement, experiment, and theory are all aspects of conceptual frameworks that can be used to ground each other holistically. These acts abide by norms intrinsic to science: they must construct the world in an intersubjectively accessible way, apart from all idiosyncratic whims.

Our conceptual scientific acts must therefore by necessity be synthesized into one ideal, intersubjective consciousness (= Kant's synthetic unity of apperception). When our frameworks lead to contradictions (e.g., our experimental results contradict our hypotheses), they fail, which we resolve with new framings. This ideal lines up nicely with Peirce's pragmatic definition of truth: that which is true that is ultimately agreed on by all investigators in an indefinite future. This 'end of enquiry' serves precisely the same function as Kant's synthetic unity as unifying our knowledge in an ideal norm. To recognize the pragmatic ideal activity of concepts and their unity in apperception is to take up pragmatic idealism.

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Searching for the Big Picture in Philosophy of Scientific Practice

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This paper is meant to spark conversation and reflection about the bigger picture of what philosophers of science are trying to do when studying scientific practices. It may also be useful for people entering the field to parse the field's current methods and intellectual program. There is an inherent risk of shallowness for an endeavor that mainly talks about what other people are doing, but the study of scientific practices has great potential to unlock new insights into the social nature of collective inquiry and its outcomes. In particular, I suggest that philosophers are uniquely positioned to contribute to an institutional theory of scientific practice, one that recognizes a continuum of degrees and modes of formalization in the research process and clarifies the relationship between the ongoing scientific specialization and novel transdisciplinary approaches. I also suggest that philosophy of science could benefit from the ideas and history of the social science tradition of symbolic interactionism, which in fact has already strongly influenced philosophy of scientific practice through Science and Technology Studies but has been overlooked even in major histories of Pragmatism.

I argue that philosophers' use of practice today is continuous with the pragmatic aims of logical empiricism despite rejecting its major philosophical positions. When philosophers today appeal to practice in their arguments, it serves the same purpose that appealing to symbolic logic did: to transform what people do (including what they say) through making the form and significance of their activities more linguistically explicit. Practice is more capacious than logic to this end because it encompasses activities beyond discursive reasoning and permits analysis relativized to community norms.

But if practices are relative to a community, what can be said across communities and about their interactions? Most prior work has tried to anchor the study of practices at a particular level of social organization, e.g. the field, discipline, lab, or project. These are helpful but not sufficiently general to support the range of comparisons required to synthesize a bigger picture of how science works and what we can learn from it. John Dewey's (1938) view of inquiry as a response to a problematic situation is helpful, and the idea of a "research problem" can serve to mark awareness of a situation subject to shared norms of inquiry. Where Dewey emphasized the continuity of common sense and scientific inquiry, however, philosophers have developed useful concepts for comparing and contrasting norms of inquiry across cases. An interactionist theory of scientific inquiry could serve to integrate philosophers' scattered insights into how scientists institutionalize their problems: what scientists take to be their research problem has real consequences for how they conduct their work, and negotiating a common understanding of the problem — including what counts as a solution — is essential to shared judgments of progress. This approach has special promise for illuminating if and how progress is possible for "wicked problems" (Rittel 1972), such as climate change and biodiversity loss, where a simple separation of method from problematic situation is impossible.

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Artificial Intelligence Benchmarks and Degenerating Research

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The move toward creating massive benchmark datasets, starting with ImageNet, ushered in the rise to prominence of deep learning, and along with it the proliferation of benchmarks for evaluating AI models at performing tasks like recognizing faces, translating languages, and captioning images. Although benchmarks have undeniably been key tools in the rise of deep learning, STS and AI researchers have critiqued an overly enthusiastic embrace of benchmarks (aka benchmark chasing) as a harmful practice, both ethically and epistemically. Benchmarks reify narrow views of correct task performance, distort research incentives, and favor researchers at wealthy institutions that can most afford to train state-of-the-art models. Recent empirical evidence has begun to track and quantify the effects and magnitude of the bias toward industry-sponsored versus academic research in areas like natural language processing, raising concerns about corporate capture of research.

This paper first illustrates how AI benchmarks solidify norms of correct task performance. What a facial expression coveys, what the appropriate caption for an image is, and what a sentence means are all deeply context-sensitive, culturally-specific, and value-laden questions, however, AI benchmarks for these tasks assume that the answers a few Amazon Mechanical Turk workers give are ground truth.

We look at the case of AI work on adversarial examples (images that trick deep learning networks into misclassifying objects) as an illustration of how benchmarks can mislead research. When it was discovered that examples can easily be constructed that are classified very differently by AI models than in the benchmarks, those anomalous classifications were initially assumed to be mistakes. However, what turned out to be the case was that deep learning networks were picking up on cognitively meaningful signals the labels missed. It was the benchmark that was incorrect, not the AI.

A forceful criticism of the assumption that WEIRD observers can declare correct answers to questions like these was made by Autistic blogger, Mel Baggs, in their takedown of the Seeing the Mind in the Eyes task, a psychometric test that purports to show that Autistic people lack empathy. As Baggs points out, instead of demonstrating this result, the test simply assumes that neurotypical answers are ground truth, so anything else must be incorrect. Seeing the world differently does not imply seeing the world incorrectly.

Criticism of benchmark chasing is often met with pushback in AI. Defenders of AI insist that critics are trying to move the goalposts each time models manage to surpass a benchmark, and critics respond by pointing out flaws that still remain. Critics of benchmark chasing are called Luddites and technophobes. While benchmarks undeniably serve a useful purpose in AI research, they are not built in such a way as to establish them as timeless standards. Rather, they should be thought of as moving targets if AI is to be a progressive research project (in the Lakatosian sense).

Signatures: A core concept from particle physics practice and its philosophical significance

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We argue that the concept of signatures, which is prominently used in particle physics measurements, albeit widely neglected by philosophers, represents an important complement to the much-discussed concept of phenomena – not only in physics. By considering phenomena and signatures as steps of experimental inquiry, we argue that signatures are the final step of the measurement process and phenomena are the first step in the theoretical interpretation.

To begin with, we critically analyze two prominent philosophical assessments of phenomena in particle physics, to wit, those of neutral currents by Bogen and Woodward and the J/ψ and Higgs boson by Massimi, and show that there remain important ambiguities that cannot be resolved by reference to formal-statistical methods in the sense of Glymour’s criticism of the concept of phenomena, or by the notion of data model that figures prominently in Suppes’ hierarchy of models.

Phenomena do not follow automatically from data. As we shall elaborate at examples, initially from particle physics and then more generally, to establish or define a phenomenon one has to combine different instances along common properties – a step that we call conceptualization. As a result, what a specific phenomenon entails is subject to changes in the underlying concepts and theories. It is signatures qua measurement that represent the stable objects of experimental practice and only contain minimal theoretical assumptions. We propose the following general definition of signature.

1.Signatures are derived from raw data using established empirical procedures. They can be obtained without a deep theoretical understanding of the detection process. Instead, theory can be black-boxed.

2.Signatures are stable and repeatable results of measurements of natural processes. They are tools to characterize, organize, and classify outcomes of measurements and thus tools to conceptualize these into a phenomenon. Signatures can be obtained from a multitude of different measurements.

3.Signatures can be signatures of an established phenomenon. Different signatures of the same phenomenon can be shown to stem from the same kind of object qua measurement. A specific signature can often be used for a large range of different phenomena.

4.Different levels of signatures can be constructed by combining elementary signatures into complicated and more encompassing ones. Since elementary signatures are remote from theory, so are combined signatures.

Based on this generalized definition, we discuss how signatures differ from data models and Leonelli's relational data. In conclusion, we provide examples for signatures in other disciplines that suggest that signatures can be broadly used to understand scientific measurements.

Computational Explanations as a Guide to What is Worth Explaining

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We can give perfectly good causal explanations of many uninteresting things. For example, why do I have a mole on my thumb? Presumably there is a causal explanation of this phenomenon, but it wouldn’t be worth your time to read it. The question “what is worth explaining?” takes on greater significance in science, where researchers pour years of their lives and millions of taxpayer dollars into their pursuit of explanations. Let phenomena of interest refer to phenomena that are worth explaining. For example, a phenomenon of interest in genetics is inheritance.

Neuroscientists are still trying to determine their phenomena of interest. Researchers have variously argued that the phenomenon of interest lies at the level of synapses, action potentials, or oscillations of neural populations. I will look at computational neuroscience as a case study in how researchers identify their phenomena of interest. Computational neuroscientists argue that we can identify phenomena of interest based on the optimality of their information processing abilities. In other words, if a part of the nervous system optimally processes information, then it is a phenomenon of interest. For example, V1 neurons that behave like Gabor filters are phenomena of interest because Gabor filters optimally extract information, while minimizing uncertainty. Two interlocking questions arise: What exactly do computational explanations explain? How do they guide us to the phenomena of interest?

Chirimuuta recently gave an account of computational explanations in neuroscience (2014, 2018). She reconstructs computational explanations as optimality explanations that explain why a feature of the nervous system is as it is. Her account is an excellent starting point, but is incomplete because it fails to explain how computational explanations guide us to phenomena of interest. I supplement her account with two arguments. First, I argue that a trait's optimality is prima facie evidence that it was selected for. If a trait is optimal, then by definition, it must provide an advantage relative to the other variants of the trait. So, an optimal trait would be selected for against other phenotypic variants. This is prima facie evidence that the trait is indeed the product of natural selection. Second, if a trait is selected for, then it is a good candidate for a phenomenon of interest. Optimizing processes, like natural selection or development, shape organisms so that they can better survive and reproduce. This suggests that a trait is only optimized if its role in the organism's internal causal economy is significant enough to affect the organism's survival and reproduction. Given that optimal traits are significant to the organism itself, a trait's optimality is evidence that it is worth studying and explaining, a phenomenon of interest.

Putting this all together: if we can give an optimality explanation of why a trait exists, then we have evidence that the trait is a phenomenon of interest. In this way, computational explanations guide us to phenomena of interest in neuroscience; they reveal both what is worth studying about a trait and why it exists.

Explainability with Large Language Models: The Meta-Explainability Problem

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Contemporary machine learning (ML) models are often considered opaque, namely, it is difficult for users to understand how and why they produce their outcomes. The Explainable Artificial Intelligence (XAI) research program aims to counter this issue by focusing on explaining opaque ML models behaviours and outcomes in humanly understandable terms. One major challenge in XAI involves formulating natural language explanations that are understandable to users with limited expertise in AI and related fields.

New generation large language models (LLMs), such as ChatGPT, seem to be particularly suitable for this task. Recent research shows that LLMs can be trained to analyse the behaviour of an opaque ML model and generate natural language explanations that are comprehensible to its users (e.g., Bills et al. 2023). However, using LLMs as XAI tools presents us with new challenges. Unlike conventional XAI techniques, LLMs themselves are equally, if not more, opaque than the ML models they should explain. This raises an additional issue: how can one place trust in an explanation provided by an LLM if one does not understand how this explanation has been generated?

One may argue that an explanation can be effective even if the mechanism that generates it is opaque. For instance, we tend to trust the explanations of experts even if we do not comprehend the mechanism by which they are formulated: what matters to us is that the expert is reliable, i.e., they provide correct explanations most of the time. However, ensuring that an LLM is a reliable XAI tool is a challenging task. This is due to the phenomenon of hallucination, i.e., the tendency of LLMs to produce explanations that seem accurate but include a plethora of false and potentially misleading information (Ye et al. 2023). To date, there are no effective methods to verify whether the explanation generated by an LLM is the result of a hallucination. Then, to ensure the reliability of the explanations provided by an LLM we need to understand the mechanism by which they are generated. However, this approach creates an undesirable infinite regress in which the reliability of each explanation-level is grounded on a meta-explanation of the computational mechanism that delivers it. This is what we refer to as the meta-explainability problem.

In our talk, we will explore the epistemological consequences of the meta-explainability problem and discuss three solutions to it. A conservative solution consists of limiting the use of LLMs to only translation tasks. That is, explanations are generated through verifiable XAI techniques (e.g., SHAP) and thus translated into natural language through LLMs. Another solution is inspired by the social epistemology of expert-disagreement and consists of adopting a "community of LLM-experts" that generates a collective explanation through a process of debate. The third solution is based on a coherentist approach (Olsson 2023) to reliability. It consists of checking the degree of consistency between LLM-based explanations and the evidence provided by standard explainability techniques whose reliability is certified. In this approach, LLM-based explanations can be iteratively optimised until the degree of consistency reaches a given desirable threshold that determines when the explanation is sufficiently reliable.

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The Animal Leyden Jar: Analogy and Exploratory Experimentation in the History of Electrophysiology

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In this paper, I consider a novel role for analogy in scientific practice. While the importance of analogy as a force for conceptual change and argument by analogy have been widely studied, I argue that analogical reasoning is uniquely suited to guide researchers engaged in exploratory experimentation. To help illustrate this point, I provide a careful analysis of a seminal work in the history of electrophysiology and neuroscience: Luigi Galvani's De viribus electricitatis in motu musculari, Commentarius.

Galvani published De viribus in 1792, putting into writing two decades of research on a phenomenon he called "animal electricity." While controversial at the time, Galvani's work on animal electricity provided the foundation for two of the most important concepts in neuroscience and electrophysiology: the action potential and the membrane potential. But De viribus is notable for at least two further reasons. First, while contemporary discussions of De viribus tend to focus on two significant and highly influential experiments, the text itself recounts — often in exhaustive detail — a litany of experimental manipulations which took place over the course of the preceding decade. Further, very little (if any) of what Galvani describes of his experimental practice can be easily interpreted as testing a well-defined hypothesis. This, I argue, makes De viribus an excellent example of what contemporary philosophers of science call exploratory experimentation. While there is some disagreement concerning how exploratory experiments should be characterized, they are generally distinguished from theory-driven research in that they (a) do not set out to confirm or falsify a well-defined, predetermined hypothesis; (b) tend to involve a large volume of open-ended investigations; and (c) are typically undertaken with an aim to characterize rather than explain phenomena.

The second, and perhaps more striking feature of De viribus, is the prominent role that analogy plays throughout. Galvani introduces a number of analogies, the most important being the comparison between muscle and the Leyden Jar (a precursor to the capacitor). Traditionally, philosophical work on the role of analogy in scientific reasoning has tended to focus on argument by analogy. While analogical arguments do appear in De viribus (mainly as a means to answer a particularly thorny series of objections), I argue that the Leyden jar analogy actually plays a more significant and less appreciated role in Galvani's research. Specifically, analogies to the Leyden jar and tourmaline both directly inform Galvani's exploratory experimental practice and provide a means to help interpret his findings. I conclude by reflecting on how my analysis fits with a recent trend exploring the myriad ways beyond argument by analogy that analogical reasoning figures into scientific theory and practice.

The explanatory power of network dynamics

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A widely endorsed view in the philosophical literature on scientific explanation subscribes to the idea that the explanatory power of scientific explanations resides in their ability to track counterfactual dependencies (e.g., Woodward 2003; Saatsi and Pexton 2013; Rice 2015; van Eck and Wright 2021; Kostic and Khalifa 2023). What also appears to be widely endorsed is the notion that models used for explanatory purposes should articulate and enable tracking (only) robust dependency relations (e.g., Woodward 2003). The robustness of the modeled dependency relations is taken to be a proxy for the explanatory power of a model-based explanation.

In this contribution I argue that a number of scientific modeling cases do not fit this view. In these cases, things are exactly the opposite: the more a model enables adapting and shaping explanations across different contexts (by highlighting changing dependency relations), the higher its explanatory power.

I build my analysis by relating it to examples drawn from modeling in developmental psychology (van Eck 2018), psychopathology (Russo and van Eck 2024) and work on modeling dynamic mechanisms (Bechtel and Abrahamsen 2010). Consider e.g., network models in psychopathology. The network theory of mental disorders conceptualizes mental disorders in terms of networks of causally connected symptoms. Mental disorders are understood as dysfunctional states in which such networks can get locked. One major aim of network theory is to (statistically) estimate such networks on the basis of data and build computational network models of mental disorders (Borsboom 2017).

A key insight is that mental disorders/network structures change and evolve across time and across agents. Hence, counterfactual dependencies between symptoms are also dynamical and prone to change. The point of network modeling is to identify the relevant symptoms, and to clarify how they may change, as conditions change, or to clarify how these symptoms and their relations differ across cases (Robinaugh et al., 2019). For instance, in the case of panic attack and disorder, clarifying how the impact of arousal on perceived threat is different across cases in light of different beliefs about the threat of a specific stressor (e.g., a growling dog) and/or different appraisals of arousal levels across cases. In this example, the symptoms composing the causal structures differ across cases, i.e., different beliefs and/or different appraisals. So solely focusing on (a model enabling the tracking of) robust dependencies as a proxy for explanatory power of the network model runs counter to explanatory purposes in the case of network modeling. If anything, the contrary is the case: the more a network model is able to adapt and shape explanations across different dependency relations, the higher its explanatory power. Tracking changing (context-sensitive) dependencies is also key.

I argue that this point generalizes and applies to other cases and types of explanation, to wit: specific dynamical models in developmental psychology (Thelen et al. 2001) and specific models of dynamic mechanisms in neuroscience (Bechtel and Abrahamsen 2010). The bigger point is that philosophical theories of explanatory power should be extended to also cover these cases.

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Deux clefs de la Physique : Calculation and Experiment in Du Chatelet’s Physics of Fire

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Today, identifying the route by which one can gain access to the means of the practice of science is relatively straightforward. To be sure, there is gate-keeping of this access. But the channels one must pursue to become a trained, credentialed scientist endowed with the resources to do research are mostly clear.

In the eighteenth century, by contrast, access to theoretical knowledge, technical know-how, and experimental equipment was left to the individual to figure out for herself. She would have to leverage connections and negotiate patronage, apprenticeship, instruction, and peer-criticism wholly through informal networks. Only the lucky gained entrance to the relatively new Academies. Women, in particular, who aspired to do science would have had to find ways to bring their theorizing in contact with the empirical world despite gender barriers to accessing advanced instruments as well as instruction in their use and in mathematics.

It is a small wonder that Émilie Du Chatelet, in the face of these obstacles, not only found ways to practice science – specifically the science of fire, heat, and combustion – but that her attempt gained wider scientific attention. In 1737, she wrote her Dissertation on the nature and propagation of fire in secret and submitted it to the Prize Competition of the Royal Academy of Sciences in Paris. In 1739, although it did not win, it was selected for (anonymous) publication in the collections of the Academy, and then published independently in a revised edition in 1744. Her Dissertation on fire went on to garner significant – if often highly critical – attention in Europe's learned community, including discussion of it (by Jean d'Alembert) in the Encyclopédie.

My primary goals here will be to exhibit her efforts to find means to do science practically, and show how they can inform our understanding of her methodology of scientific inquiry. Specifically, I will be concerned with two questions about the Dissertation on fire. One, given the barriers to cutting-edge resources which Du Châtelet faced, how and in what ways did she come to leverage 'le calcul et l'expérience, ces deux clefs de la physique' (calculation and experiment, these two keys to physics)? And two, what do her efforts tell us about her philosophy of science?

I exhibit some of her most interesting conceptual and empirical arguments in the text, and argue that they go significantly beyond the work of others in her milieu. For example, Du Châtelet leveraged theoretical calculation using Leibniz's concept of vis viva as applied to hypothetical heavy particles of fire in order to bring that hypothesis into contact with our everyday experience of being bombarded by the sun's rays of 'fire' (the sun was then generally viewed as a source of fire). This allowed her to marshal evidence without sophisticated instrumentation that she could not access. I also compare her work to the winning entries of the Prize Essay Competition – selected over her work – and argue that she goes significantly beyond them in terms of the sophistication of her handling of hypothetico-deductive reasoning.

Counteractive Mechanisms and Effect Indeterminacy in Evidence-based Policy

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Evidence-based approaches to policymaking have been growing in popularity over the last decades, covering a variety of domains: health, education, criminal justice, poverty, etc. The special focus of EBP on the effectiveness of policy – revealed by its central slogan 'what matters is what works', has initiated reflection of philosophers (especially philosophers of science). A few philosophers have written extensively on, e.g., evidence, causality, and mechanisms, mainly contributing to the policy effectiveness prediction (PEP).

An optimal PEP should address two questions: 'Will the policy work?', and 'To what extent will it work?'. The first question involves a qualitative assessment, while the second demands a quantitative one. To illustrate, consider the Plastic Shopping Bags Ban Act implemented by the Australian Capital Territory (ACT) in 2011. The policy aimed to reduce the usage and

environmental impact of plastic bags. However, a study in 2020 indicated mixed results: a notable decrease in single-use high density PE bag consumption was offset by an increase in other plastic bags, leading to a modest overall reduction. This case illustrates the quantitative complexities in evaluating policy effectiveness: although the policy was effective in this case, there was a disparity between anticipated and actual outcomes.

In this paper, I argue that such gaps in policymaking are often rooted in an epistemological challenge: the indeterminacy of the net effect of counteractive mechanisms triggered by the policy. Counteractive mechanisms are mechanisms that are triggered by the same action or process but produce effects which work against each other. They can result in problems when a clear causal relation needs to be established. A prominent challenge is 'the problem of masking', which highlights the inadequacy of relying solely on mechanistic evidence for causal inference.

In the context of policymaking, insufficient understanding of counteractive mechanisms leads to reasoning errors in forming accurate PEP. I illustrate these issues using the ACT's plastic bag ban and a second example – the bicycle helmet case. In the latter case, despite robust medical evidence supporting the effectiveness of bicycle helmets in preventing head injuries among cyclists, some studies in regions with implemented bicycle helmet policies have not shown a significant decline in head injuries. This paradox can be attributed to the phenomenon of 'risk compensation', where the perceived safety provided by helmets leads to less cautious behavior of both cyclists and drivers, increasing the risk of accidents and cyclists' head injuries.

I use both cases to show that insufficient understanding of counteractive mechanisms can lead to suboptimal PEPs, failing to accurately address both qualitative and quantitative assessments of policy effectiveness. Additionally, the net effect of counteractive mechanisms cannot be epistemically determined solely using mechanistic evidence. Difference-making evidence is indispensable in solving this complication. Conclusively, the paper advocates an approach of evidential pluralism in PEPs, suggesting that embracing multiple forms of evidence can lead to more accurate and effective policy evaluations.

Interdisciplinary Research & Integration in Social Robotics

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The protagonist in this paper is Probo, a social robot with an elephant-like appearance. Being a robot, Probo can reason about tasks and their execution by relating perception to action. As a social robot, it also has the capacity to communicate with humans.

Probo was built as a platform for robot enhanced therapy (RAT), with a special focus on therapy for children with autism spectrum disorder. It can communicate both verbally and non-verbally. Verbal communication is mediated via a touch screen in its abdomen, and a voice with associated lip-syncing mechanism. Non-verbal communication occurs via reproductions of human facial expressions.

Including the functionality of facial expressions posed a major challenge in the development of Probo. The team wanted the robot to be able to adopt 6 standard facial positions (defined as a combination of position of head, eyes, ears, mouth and trunk) that can be recognised by children and adults as expressing 6 emotions: joy, sadness, disgust, anger, surprise, fear.

Making the robot expressive required an interdisciplinary effort that involved combining knowledge and know-how from three scientific disciplines: computer science (to produce a virtual model of the head), mechanical engineering (the development of a physical prototype) and psychology (recognition tests for the 6 emotions).

In our paper, we perform a detailed analysis of how this interdisciplinary integration was brought about. We explain what contributions the different disciplines made, and how they were integrated. More specifically, we describe (i) how research in the different disciplines interlocked, i.e. how research in one discipline influenced that in others, and (ii) how interlocking led to iterations, i.e. how interlocked results were fed back to the disciplines and led to new research.

The case study takes up the first part of the paper. In the second part, we investigate what lessons can be drawn from the case:

- Can we abstract a model from Probo that is applicable to some other cases?

- Can Probo be seen as a ‘paradigmatic example’ of the type of integration that occurs in a certain kind of interdisciplinary research?

- If so, what kind? And what are its ‘typical features’?

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A Framework for Evaluating Behavioral Traits

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How should biological traits be conceptualized? Philosophers have grappled with the question of how to conceptualize traits primarily in the context of morphology, especially regarding homology and evolutionary novelty (DiFrisco, Love, and Wagner 2020). However, the conceptualization of behavioral traits has been largely neglected (Wenzel 1992). This neglect has become more visible as social and behavioral sciences utilizing molecular data sets to explore questions about human behavior face increased criticism over their trait conceptualization and measurement practices (Riskin and Feldman 2021). Is educational attainment (EA) or social mobility a genuine behavioral trait?

I present two existing theoretical frameworks for answering the question of how to conceptualize a behavioral trait and develop a novel intermediate framework. The first, conventionalism, supposes that behavioral traits are simply those that can be measured. Since EA can and often is operationalized in terms of number of years of schooling, there is no issue about its status as a genuine behavioral trait. However, this framework is too permissive and does not facilitate meaningful comparison across different traits. The second framework is mechanistic, conceptualizing behavioral traits in terms of underlying shared molecular mechanisms, as is sometimes done with morphological traits. Although potentially feasible in some areas of behavioral science where there is a large body of experimental work and molecular mechanisms are better understood (e.g., in model organisms), underlying mechanisms for behavior are mostly unknown or experimentally inaccessible in human behavioral research. Inspired by non-human model organism research practices, my novel intermediate framework characterizes behavioral traits by drawing analogies to how researchers characterize morphological traits. For example, scientists often identify spatial boundaries ("joints") for morphological traits; temporal boundaries can be identified for some behaviors and serve a similar role in individuation. I offer criteria that are neither necessary nor sufficient but provide a reasonable guide for identifying behavioral traits for the purpose of research. Criteria include relationship to biological function, temporal boundaries, regularity of occurrence, and appearance at specified life history moments. These criteria also connect with the practical questions relating to trait measurement in empirical research, while also permitting one to acknowledge that there may be behavioral traits that are uniquely human and not comparable to other animals.

This intermediate framework provides a preliminary basis for researchers across many different areas of behavioral research to conceptualize traits for study in both non-human and human-focused inquiry. Importantly, this framework may be particularly relevant to disciplines like behavioral genetics, which have been criticized for conceptualizing traits simply due to being able to measure them (i.e., conventionalism). Some of these critiques challenge the idea that behavioral traits can be clearly conceptualized in general. By adopting the intermediate framework, one can shift the conversation from the conceptualization of behavioral traits in general to specific traits of interest according to specified criteria. This yields a basis for normative evaluations, suggesting that learning and memory in fruit flies could count as a well characterized behavioral trait while EA in humans does not, with attendant consequences for different social and behavioral sciences.

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How to Ask Biologists Metaphysical Questions Without Explaining Metaphysics

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In this paper I demonstrate how philosophical accounts can be operationalized for use in semi-structured interviews (hereafter "interviews"). This operationalization is the first step towards filling a gap in data available to philosophers which currently focuses on published works. Specifically, this paper prepares for interviews aimed at adding data to the current scientific realism/anti-realism debate (hereafter "the debate") over what metaphysical commitments our best scientific theories support.

Often beliefs of scientists are taken to be what is found in written works, like published papers. The first place many look for scientific information is in peer reviewed publications with the understanding that acceptance in a publication means that the scientific community (or at least their peers) accept the assertions in papers as scientific knowledge. The use of published works is helpful since one can be somewhat confident that the assertions in scientific papers are the scientific knowledge scientists want to disseminate. However, what beliefs scientists hold and what assertions they make in published papers supported by evidence may be different. The only way to know what view of reality biologists personally support and what they believe their best scientific theories tell them about reality is to ask them directly.

Surveys can and have provided valuable information to the debate (Beebe and Dellsén 2020). However, the problem with survey data generally is that it is unclear why a participant answered the way they did. Would participants answer differently if they had the chance to ask questions or explain their answers further? Possibly. This is why I propose interviews as a way to dive more deeply into why scientists answer questions the way they do. But before interviews can be completed, philosophical commitments need to be operationalized or translated into language that can be used to ask biologists questions they will understand. This paper provides an example of this operationalization by going through a sample prompt and demonstrating kinds of responses biologists may give in their "language", so to speak, that correspond with philosophical commitments often discussed in the debate.

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Rethinking Time in Heterochrony

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Heterochrony, understood as changes in developmental timing, has long occupied a central position in evolutionary developmental biology (evo-devo). The term has undergone several rounds of conceptual modifications since its coinage by Haeckel in the nineteenth century. de Beer, dismissing Haeckel's idea that "ontogeny recapitulates phylogeny", disentangles heterochrony from recapitulation theory and adopts the former to describe any differences in the timing of developmental events in related taxa. Gould, in his classic revitalisation of the concept, focuses almost exclusively on the changing relations between morphological size and shape. While this last formulation of heterochrony has been influential, there has been a growing consensus that we need to refocus on developmental events and map evolutionary changes of morphology to changes on the molecular and genetic levels. This multi-level approach, made possible by advances in molecular tools and imaging techniques, allows us to study patterns of gene expressions and cell activities in impressive spatiotemporal resolutions, as well as develop non-traditional model organisms for comparisons of developmental mechanisms between closely related species. While this mechanical approach provides exciting new links between ontogeny and phylogeny, I argue it exacerbates an important problem in heterochrony research: the absence of a 'standardised' time in different developmental systems. Using the development of neural crest as a concrete example, I will show the conceptual and practical challenges that arise with different methods to standardise development – for example, by constructing staging tables – when we attempt to identify the molecular and cellular mechanisms of heterochronic phenomena. To overcome this issue, we need to rethink the fundamental concept of time in the context of embryology. I suggest that sequential heterochrony, a concept proposed to study developmental events not in terms of a universal "clock" time but of the relative order of occurrence, is on the right path. Nevertheless, it is too coarse-grained, which raises concerns about its compatibility with our increasingly high-resolution and pluralistic approach. Moreover, sequential heterochrony carries the connotations of linear causation that is often exclusively bottom-up, leaving no room for future studies to integrate top-down influences, such as temperature and functional requirement, into our theorising. Therefore, after supporting the idea of replacing absolute time with a relative order of developmental events, I further suggest we replace scalar thinking with a form of vector thinking. In other words, not only is developmental timing best understood as event-based, but it is also locally defined by the concordant coming together of different processes, each defined by its dynamic magnitude and directiveness. One of the differences vector thinking may make is to show how changes in developmental rates and spatial patternings (heterotopy) can be conceptually related. It may also render it more intuitive for us to understand how heterochronic effects are unavoidably mediated by other developmental processes, such as canalisation exerted by the dynamic system as an integrated whole. As a result, we need to reevaluate the emphasis placed on the identification of precise onset/offset and duration of developmental events, the search for "heterochronic genes", and the role of "time-keeping" mechanisms.

Why is it (still) Difficult to Understand Black-Box Models? Explainable Artificial Intelligence and the Experimenters' Regress

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Machine learning models based on artificial neural networks are increasingly used in science as an efficient tool for data classification and prediction. Meanwhile, they are also notoriously opaque, as scientists do not have sufficient knowledge about how and why they assign labels to the input. Opacity undermines the trust of such models and limits their application in science. In reaction to such criticisms, the explainable artificial intelligence (XAI) project develops network interpretation strategies that reveal how those models work. This is expected not only to help identify potential errors and enhance trust, but also to identify real features or causal factors in the target system and thus refine existing scientific representation.

However, the status quo of XAI practices does not match its bright expectations. Interpretation strategies proliferate, with different algorithms providing discordant results for the same purpose, but there are no agreed-upon standards to select among them. With this, it is doubted whether XAI can ascertain the features employed by machine learning models and contribute to scientific practices as anticipated.

I argue that the notion of the experimenters' regress (ER) is useful for understanding and navigating through this status quo. ER refers to a situation where an untested new method is employed to detect an unobserved novel phenomenon, such that the correctness of the result and the validity of the method can only be determined by each other (Collins 1985). This leads to a regress where scientists can only employ "non-scientific criteria" to make decisions about the quality of the method and its results. I identify a variant of ER in the design and application of XAI algorithms: the validity of interpretation strategies and the correctness of the machine learning model in performing a task cannot be secured without assuming the other, so it is impossible to find a justified universal standard for XAI strategies. I identify two features that make this variant harder to manage. First, strategies of breaking the regress are context-specific, so that solutions to the regress in one context cannot be transferred to another. Second, metrics of evaluating interpretation strategies face a second-order regress, so that no reliable metrics can be built without solving the original regress.

The philosophical literature on ER provides lessons for XAI. First, by comparing XAI with a historical case of ER in 19th century microscopy (Schickore 2009), I argue that instead of searching for universal standards, practitioners using XAI should form explicit pragmatic agreements in each specific context of its application after long-term empirical tests. Second, as various epistemological and social factors can participate in scientists' decisions under situations of ER, I argue that practitioners should be aware of the actual considerations involved in the development, selection, and application of XAI algorithms. Only with this, potential biases can be identified, and better decisions can be made. Philosophers can contribute to this by analyzing XAI's "living standards", the non-universal and unjustified rationales adopted by practitioners. I exemplify this by identifying three living standards and present their underlying social or epistemological considerations.

Specialisation of stem cell biology and its downside: Three case studies

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Stem cell biology has arguably become a successful repertoire in biology (Ankeny and Leonelli 2016), anchored around stem cells as an epistemic thing / technical object (sensu Rheinberger 1997) maintained through a specific ensemble of technologies, experimental settings and standards, institutional platforms and regulatory frameworks. This has many advantages as well as drawbacks, some of which have been extensively documented by authors such as Landecker (2007) and Fagan (2013). In this paper we build on such scholarship and our own analysis of published literature, combined with interviews with relevant scientists, to examine the impact of the entrenchment of stem cell research as a repertoire on related fields, most notably developmental biology. We contend that the hyperspecialisation and standardisation that has come to characterize stem cell research has curbed opportunities for generative hybridisation with relevant parts of developmental biology, thereby limiting opportunities for discovery. We provide three cases of this, coming from plant, primate, and organoid research. In the first case, there is little interaction between stem cell biology and studies of plant meristems, even though the latter fosters understanding of the biological phenomenon of stemness. The second case concerns how studies of stem cells of non-human primates, such as the chimpanzee and Japanese macaque monkey, are not appreciated by the community of stem cell biologists in Japan. The third case illustrates that the self-assembling and self-organizing capacities of cells--a phenomenon that has interested embryologists and developmental biologists for decades--was neglected by stem cell biologists in the dawn of organoid research. We argue that the high degree of standardisation and relatively rigid conceptual expectations within stem cell biology function as constraints on exploratory research in at least two different ways: (1) by limiting the exchange of insights between researchers working on different taxa (primate versus human, plant versus animal) and (2) by restricting the potential of unexpected experimental artefacts to generate novel discoveries (e.g. organoids, specific traits of primate cells).

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Revisiting Hempel's Statistical Inconsistency

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In studying probabilistic explanation, Hempel realized that it is possible for an actual event to be assigned a high probability, while had an alternative result occurred, that could be assigned a high probability too. It seems no matter which result occurs, that could be explained. This situation greatly worried Hempel, which he calls statistical inconsistency. What really worries Hempel is the “reference class problem”: probability claims are relativized to reference class, the choice of which seems often epistemically arbitrary. In this paper, I examine how reference classes are determined in statistics. I argue that statistical practice relieves Hempel’s worry and paves the way for a better account of probabilistic explanation, at least in some cases.

In section 2, I clarify what a “reference class” is. Formally, any probability claim presupposes a probability distribution over some sample space, so a reference class has two components: (1) sample space: the set of all atomic events; (2) probability function: a mapping of each atomic event into a real number in a way that obeys the probability axioms. Relativity can therefore arise in two forms: relativity to sample space, and relativity to probability function. When a coin is consecutively tossed by Anne and Bob, how probable will the next toss be heads? In answering this question by establishing a probability model, I might admit of two atomic events, {H, T}, and you might admit of four, {HA, TA, HB, TB}. This is a relativity to sample space. Or we might choose the same sample space, but due to available data being finite, make an inference to different probability functions. This is a relativity to probability function. I argue the two forms of relativity are independent and of different natures.

In section 3, I turn to statistical practice to see how each form of relativity is handled. Suppose our goal is to accurately predict a future event; which sample space should we use? To begin with, it’s not wise to list every condition that will hold in the future, consider possible variations of each, and create the most fine-grained space, for two reasons. First, probability distributions on such a huge sample space can never be learned reliably. Second, what we really need is the relevant conditions. In cases like coin tossing, which conditions are relevant might be intuitively clear, but this isn’t often true in scientific studies. To handle this, statistics employ data collection and correlation analysis, e.g., covariation analysis and high-order correlation analysis. They function to isolate a manageable size of dependent variables, which are used to delineate appropriate sample spaces.

Given a sample space, the next task is to infer about its probability function. Estimating this function in every detail may be infeasible, especially in continuous variables. Here, a common strategy is to estimate conditional expectation: given the value(s) of the independent variable(s), what is the expected value of the dependent variable? This directs us to look for a function minimizing predictive inaccuracies. To handle this, statistics employ model selection techniques, e.g., linear regression, logistic regression, and kernel regression. Estimation is made with certain regression technique and instructed by model selection methods, which are evaluated by epistemic standards like consistency and unbiasedness.

In conclusion, it is possible to choose reference classes in a theoretically motivated and epistemically feasible way. Note, explanation needs causal model, which goes beyond probabilistic model, but the establishment of which, I suggest, is similarly governed by objective epistemic norms. In these cases, Hempel’s worry is relieved, and a better account of probabilistic explanation is possible.

Disentangling Domestication: Are Humans Domesticated?

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The idea that humans are domesticated goes back at least to Darwin, who considered then rejected it, and continues to this day in various forms. Science popularizers, such as Noah Harari and Michael Pollan (among others), have provocatively claimed that common conception of domestication has got it backwards, humans have not domesticated plants and animals, rather they have domesticated us. “The word ‘domesticate’ come from the Latin domus, which means ‘house.’ Who’s the one living in the house? Not the wheat. It’s the Sapiens” (Harari). This may be chalked up to rhetorical flourish, but there are alternative and more respectable defenses of the claim in the scientific literature. More recently, some have claimed that humans share a constellation of traits that other domesticated mammal species share. These shared traits, collectively called domestication syndrome, are present in many domesticated mammals, and it is claimed that humans share many of the same suite of traits. In this work, I assess the whether the claims that humans are domesticated or self-domesticated are justified, ultimately concluding that they are not.

The first step in assessing whether humans are domesticated is to get clear on the biological processes involved in the domestication process itself, in other words, what exactly is domestication? This would appear to be a straightforward task, given the work on the domestication process over the past 150 years, and that Darwin famously used domestication as a model to understand natural selection. However, this is not the case. I will argue that there are three distinct senses of domestication. The first sense corresponds with Darwin’s conception of domestication, which is that certain beneficial traits were either intentionally, or unintentionally, selected for. Under this account, species that possess traits that arose through artificial selection or that were “actively managed,” as opposed to natural selection, are considered domesticated. A second conception is tied to the domus, such that species that have evolved in response to human sedentism are considered domesticated. For example, house mice, sparrows, and some weeds would count as domesticated in this sense, but not the first. A final sense of domesticated refers to species, or individuals, that have certain pro-social behaviors, and this sense of domestication can be contrasted with feral. I argue that this sense of domestication does not correspond to a biological process, but is a social construct.

Returning to the question of human domestication, I argue that from either of the two biological senses of evolution, we should not consider humans as being domesticated, or self-domesticated. From the vantage of the first perspective, domestication is necessarily a mutualistic process, with asymmetric benefits, thus humans cannot be “self” domesticated. From the second (domus) perspective, if we consider adaptation to particular niches as evidence of domestication, then we would have to consider many, if not most, species as domesticated, which is an unintuitive result. While there is value in exploring domestication syndrome, we should not conflate this with the domestication process.

Academic Discussions on Human Enhancement Meet Science: A Quantitative Analysis

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Advancements in genetic manipulation and related technologies enabled researchers to alter and replace DNA in organisms, as well as transfer genetic material between organisms. In particular, the development of the CRISPR/Cas9 method in 2012 is considered a revolution due to its efficiency and cost-effectiveness (Jinek et al. 2012). In 2015, CRISPR/Cas9 germline modifications were first used in non-viable human embryos, opening a real possibility of making permanent, heritable changes to the human genome (Liang et al. 2015). Simultaneously, other technological developments (e.g., pharmaceutical, cybernetic, nanotechnological) are (or may be soon) used to challenge human physical, cognitive, or emotional limitations.

Although radical human enhancement had been widely discussed for years before it became practically possible (Evans 2002), one could expect that recent scientific developments have reignited academic debates on banning, limiting, or controlling some types of scientific research. More than a decade ago John Harris claimed that “Moral enhancement is coming to the forefront of bioethical scholarship for an interesting combination of reasons”. He observed that the discussions on this topic combine “cutting-edge science with mainstream philosophy and with the hopes and fears of ordinary people” (Harris 2011, cf: de Melo-Martín 2022). In contrast, some other authors complained that “the enhancement debate… suffers from a lack of empirical input” (Kourany 2014) or that “the human enhancement discourse is now untethered from any specific technoscientific research programme” (Schick 2016).

How to decide which commentators are right? Our paper addresses this question systematically. First, we use a ‘distant reading’ approach based on topic modeling (a computational text-mining technique aimed at discovering hidden thematic compositions in large text corpora) to identify in the corpus of 19,488 papers published in published since 1971 in seven leading bioethics journals those that deal with human enhancement (n=1360). Second, assuming that the analysis of citation flow outgoing from a collection of scholarly articles might provide valuable insights into their thematic focus and the genealogy of their main concepts, we conduct a comprehensive analysis of almost 11,000 references cited in that subcorpus to quantitatively examine, from a bird's eye view, the degree of openness of this part of scholarship to the specialized knowledge produced in biosciences. Thus, our study systematically evaluates the scale of use of empirical data produced in biosciences to influence academic discussions on, among other things, the regulations of scientific practice (cf. Pradeu et al. (2021), who analyzed to what extent philosophers of science directly contribute to scientific knowledge or Khelfaoui et al. (2021) who analysed the visibility of philosophy of science in the sciences). Although almost half of the analyzed references point to journals classified as Natural Science and Engineering (NSE), in our paper we discuss why this cannot be treated as a strong evidence for the cognitive influence of recent discoveries in biosciences on academic discussions on human enhancement. We conclude that a big part of the academic discourse surrounding human enhancement is inflected with “science-fictional habits of mind” (Clayton 2013) and our findings point to the need for a more science-informed approach in discussions on enhancing human life.

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Abstracts of Posters

(alphabetical by last name of lead author)

When Can Absence of Evidence Be Evidence of Absence?: A Case Study of Geological Practice

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A common aphorism says that "absence of evidence is not evidence of absence." Nonetheless, scientists often appeal to the absence of evidence to support hypotheses about the absence of entities or processes in the world. In this paper, I ask, "when can practicing scientists rely upon absence of evidence as evidence of absence?" To answer this question, I consider two case studies from the practice of historical geology, or the scientific task of reconstructing the Earth's geological history. I argue that even when conditions identified by "standard" accounts of absence of evidence are not met, appeals to absence of evidence might still be warranted, insofar as they advance scientific practice by prompting further investigations into the implications of auxiliary and alternative hypotheses.

My first case is the late-nineteenth century debate among glaciologists regarding the theory of "submergence," which proposed that "erratic" rocks were deposited on land when floating icebergs melted above the submerged land below. An alternative theory posited that the land was not submerged and icebergs instead deposited erratics when they made direct contact with the land itself. In this case, glaciologists appealed to the absence of important lines of evidence for submergence to argue that submergence theory was false. This might be considered a "standard" case, insofar as it exemplifies standard accounts of absence of evidence that have been offered by philosophers of science thus far: absence of evidence can only be evidence of absence when evidence would be highly expected had the entities or processes in question occurred. The inference in this case was warranted, because the evidence would be highly expected had submergence occurred.

My second case is the contemporary debate among geologists regarding the timing of the onset of plate tectonics on Earth. There is no consensus regarding the timing of the onset of plate tectonics, with proposed dates ranging billions of years, from the Neoproterozoic to the Hadean. While there is a conspicuous absence of certain key lines of evidence for plate tectonics prior to the Neoproterozoic, most geologists maintain that plate tectonics initiated long before that era in Earth's history, because early evidence for plate tectonics cannot be highly expected given the vicissitudes of the rock record. Still, appeals to the absence of evidence for plate tectonics prior to the Neoproterozoic persist. I argue that appeals to absence of evidence as evidence of absence are still appropriate to scientific practice even in such "non-standard" cases, because such appeals help to advance the aims of scientific practice. In addition to the standard epistemic aims of forming true beliefs, scientists might gain a greater understanding of Earth's deep history by exploring the implications of alternative hypotheses and considering possible explanations for why certain key lines of evidence might be absent. By prompting such investigations, appeals to absence of evidence in the case of timing the onset of plate tectonics advance the aims of scientific practice, despite the fact that standard epistemic conditions for when such appeals should be warranted are not met.

The (beta) decay of effective realism

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The effective realism (ER) advocated by James Fraser and Porter Williams, following David Wallace, is a form of scientific realism designed to explain the extraordinary empirical success of the quantum field theories (QFTs) of the standard model of particle physics (SM). Their answer to the pessimistic meta-induction is a way to ` ` divide and conquer'' these theories into the elements crucial to their empirical success and idle theoretical wheels. It capitalizes on two widespread tools in the contemporary particle physicist's toolkit: the Wilsonian renormalization group (WRG) and the effective field theory framework (EFT).

In this paper, I raise an objection to ER, which relies on engaging with these physical theories and their mathematical structures from the ground. Take a particular EFT: Fermi's tremendously successful theory of beta decay, the best description of one of the most abundant kinds of radiation from the 1930s to the 1970s. Since the theory is ` ` non-renormalizable,'' using ER's recipe for extracting the belief-worthy contents of Fermi's EFT erases any trace of particle interactions! If ER were right, we should believe in a picture of the world without such radiation, which misses all the features that make Fermi's theory empirically successful. Fermi's theory is no antiquarian curiosity: using it is still a live option for physicists working with electroweak interactions, at least at some energy scales.

My objection also raises a more foundational question: Is renormalizability necessary (or at least a crucial ingredient) for predictability? One way to read my objection is as a call for a mild amendment to ER: non-renormalizable theories are also important for scientific practice and don't fit ER's realist commitments. However, I want to make a stronger claim: ER's criterion to identify and isolate the belief-worthy contents of an EFT would imply that non-renormalizable theories are (at least approximately) unfit for realist commitment altogether. Does this mean the aspirations for a realist view of QFT are doomed despite ER's laudable attempt?

Wallace's work on another non-renormalizable theory seems to throw a lifeline to ER. For ER, the sin of non-renormalizable theories is that their predictions depend on an energy scale that the WRG warrants in taking as idle theoretical wheels. In contrast, Wallace's realist take on QFT considers this energy scale to be physical, so non-renormalizable theories become trustworthy by realist standards, at least at some energy scales. I argue that the worry of ad hocness for locating realist commitments in renormalizable theories but not in non-renormalizable ones doesn't go away with Wallace's upgrade of ER.

To shed light on the feasibility of a realist view of QFT against the backdrop of these criticisms, I draw from the literature on scientific representation to understand EFTs as models in a way that lets us be ecumenical in taking both renormalizable and non-renormalizable theories on equal footing. Following the practicing physicists defending the EFT framework, the difference between these theories is of degree rather than kind, where what comes in degrees is the level of accuracy we want our predictions to have.

To the girl who cried pain they asked "How much"? On pain measurements in gynechological contexts

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The International Association for the Study of Pain (IASP) defines ‘pain’, as an “unpleasant sensory and emotional experience associated with actual or potential tissue damage” (IASP 1979). This definition reflects a commonly widespread view that pain is subjective, private, and the source of incorrigible knowledge (Aydede 2017). Such an understanding of pain however opens the door to issues related to its quantification and comparison between pain states at different times and among different patients. The attempt to quantify pain represents therefore an interesting epistemological challenge in the history of medical research, long ignored by philosophers.

I set out to highlight the main epistemological challenges in pain measurement practices, that – starting from a conceptualization of pain as a subjective phenomenon – attempt to quantify its intensity, establish pain scales and thresholds, and collect researchable data sets on pain experiences. To do so, I look at several subjective pain ratings developed over time, including numerical rating scales (NRS) (e.g., from 0, no pain, to 10, the worst pain imaginable), verbal rating scales (VRS) of category judgments (e.g., mild, distressing, excruciating), and visual analog scales (VAS), in which pain is indicated by marking a spot along a 10-cm continuum (Berger and Baria 2022); pain questionnaires (Melzack 1975); and dolorimeters (Giesecke et al. 2004). Additionally, I focus specifically on gynecological diseases involving chronic pain (such as endometriosis or vulvodynia), because of the tension between the general understanding of pain as subjective and private, which grants pain self-report the highest epistemic reliability, and societal gender biases that have traditionally dismissed women’s pain experience (Hoffmann and Tarzian 2001; Jones 2016). This case study therefore is at the crossroads of several topics recently explored within the philosophy of metrology, from definitional issues of the measurand, and its operationalization, through the construction of scales and validity, to the influence of values in measurement practices (Alexandrova and Haybron 2016; Rodriguez Duque, Tal, and Pamela Barbic 2024; Philippi 2021).

Overall, I suggest that a patient-centered approach, aiming at easing the daily suffering from chronic pain, needs to rely on the integration of several measurement procedures. A single pain measure cannot be taken to exhaust the empirical dimension of the phenomenon, nor its impact on women’s lives. This aligns with an increasing consensus on treating pain as a multidimensional phenomenon, calling for the integration of different pain measurement practices in pain management.

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The Many Faces of Parachute Science: Exploring Epistemic and Ethical Harms as Distributive Injustices

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Parachute science, the practice of going to low-income countries to conduct research without collaborating with locals has recently gained much attention in paleontology due to cases like the repatriation of Ubirajara jubatus and the contested legal exportation of Irritator challengeri. However, there are still no clear conceptions of what constitutes parachute science. Given this, I give a set of cluster criteria in an attempt to delineate the characteristics of parachute science that differentiate it from the bigger umbrella term, scientific colonialism. I argue that there will be some fixed, necessary criteria that must be present in every case of parachute science but some other criteria can be interchangeable or only used when applicable. The necessary criteria are that research must have been conducted outside of the researcher's affiliated institution location and that no meaningful collaboration occurred between local researchers and foreign researchers during the design, investigation, or analysis of the research project. The flexible criteria include but are not limited to the following: the objects of study including artifacts, fossils, and rocks get extracted from the land; data extracted from the objects is studied and analyzed in a country outside of where the data was acquired; the products of the data analysis are published or stored outside of the country where the data was collected; and physical objects from which the data as extracted during the research study are eventually displayed in museums outside of the country where the objects were collected.

After having a clear concept to work with, I turn to an exploration of the harms resulting from parachute science. I group the harms into three general categories: (1) non-reciprocal exploitation of local resources in the form of research input imbalances and lack of involvement with local stakeholders' interests, (2) loss of knowledge and heritage for local communities as a result of objects being taken away, and the lack of collaboration between the locals and the foreign researchers, and (3) increased likelihood of poor epistemic quality research and bad science as a result of lack of collaboration with locals and the foreign researcher's lack of knowledge of the country where research is being performed. I use the case studies of Ubirajara jubatus and Irritator challengeri to highlight the different harms included in the three categories of harms I present. I culminate the exploration of these harms by arguing that the main problem with parachute science is that it displays an instance of distributive epistemic injustice. I conclude the chapter by motivating the need for collaborative relationships in science and by exposing what I think are necessary values to have to produce ethical research.

Why Open Science Needs Philosophy

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This poster identifies epistemic challenges relating to current conceptualisations and implementations of Open Science (OS), and outlines a large research project devoted to exploring this domain and devising a philosophical framework for reliable, responsible and inclusive OS going forward.

OS, broadly understood as the commitment to collaborative research promoting the widespread distribution and reuse of results and methods, is transforming scientific practices and institutions around the globe. Principles like transparency and reproducibility are frequently invoked as clear ways forward, leading to the widespread adoption of practices like preregistration, preprints, and sharing code and data. Public engagement efforts, such as community science and visibility on social media, are increasingly viewed as integral to good research practice. Moreover, research institutions are under pressure to reform their evaluation criteria to recognize and reward scientists who devote time and resources to repeat experiments or make their data, methods, or other research materials and outputs available and accessible. The very notion of a research output is expanding to include data, models and methods, and there is wide consensus about the need to improve scrutiny of such outputs. Despite the impact of OS on all stages of research and across a wide variety of contexts, the philosophy underpinning OS has not been clearly articulated. A key reason for this gap is the difficulty in providing an overarching account of OS given the diversity of methods characterising scientific research, which makes it impossible to apply OS principles such as open dissemination, transparency and reproducibility uniformly across fields. This difficulty is exasperated by the variety of environments in which researchers operate around the world, and the unevenness with which OS infrastructures, procedures and digital tools fit everyday research work – leading to concerns that OS practices may actually increase epistemic injustice by widening existing divides between researchers working under different conditions.

The PHIL_OS project was started in September 2021 to address these concerns. It combines a philosophical analysis of research conditions with empirical research on how different environments – particularly within low-resourced and marginalised settings – enact and conceptualise OS. The empirical research centres on a number of case studies from the life sciences broadly construed, including: OS implementation efforts at agricultural field stations in Ghana and Greece, aiming to make information about crops widely available and comparable across locations, and distributed research networks such as those surrounding the study of pest-plant interactions in Europe; plant biology investigations in NASA's GeneLab, an open science program for experimental outputsgenerated on the International Space Station; OS practices in ecology and conservation biology such as citizen science biodiversity projects and efforts to open up and reuse animal tracking data; and the role of research software in enacting and channelling conflicting approaches to the sharing of genomic data. These case studies support an analysis of the opportunities, challenges and risks involved in implementing OS across a variety of research settings, with a specific focus on an inclusive understanding of openness as a path towards cultivating both equitable and pluralistic standards for 'best practice'.

Examining Part–Part Interactions toward Improving Explanations in Cell Biology

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Mechanistic explanations are a mainstay of causal accounts in cell biology. Such explanations are underpinned in large part by a network of part–part interactions, e.g. protein–protein or protein–nucleic acid interactions. These interactions have traditionally either been discovered in a focused, experiment-by-experiment manner or via so-called 'hypothesis-free' large-scale 'interactome' studies, which require subsequent verifications of the individual interactions of interest. In all such studies, regardless of the scale and mode of experimentation, there is a tacit assumption that an 'interaction' is constituted simply by the proximity between and/or enzymatic changes imparted on the two parts (of note, multipart interactions can still be thought of as being composed of a number of two-part interactions). However, very few substantive theoretical accounts of what may actually constitute an 'interaction' between molecular parts in the context of the biological cell have been put forth, thus hindering the goal of properly modelling and explaining realistic part–part interactions. Starting with the example of a mechanistic explanation of an important cellular phenomenon (the mitochondrial respiratory chain), I develop a two-part account of protein–protein interactions, with implications for other types of cellular part–part interactions. First, I map out four aspects relevant to the sequence of events taking place in protein–protein interactions, and, second, propose (i) interaction-enabling properties of proteins and (ii) interaction-enabling properties of the proteins' environment as elements that could be explained by relevant lawlike generalizations. These generalization-based explanations could answer contrastive why-this-and-not-that types of questions pertaining to different aspects of a protein–protein interaction of interest in a mechanistic explanation.

Technical and epistemic objects in cell-type classification: A study of single-cell sequencing platforms

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There are approximately 37 trillion cells in the adult human body. One of the challenges for modern biomedical science involves organizing those cells into an atlas, periodic table, or some other suitable structure, so as to ensure reproducible and measurable progress, and leave behind the "wild west" of cell-type classification (1). Our aim here is not to propose a new normative method for categorizing cell types. Instead, we investigate some of the conditions that make such categorizations possible. Although Anglophone philosophers of science have traditionally occupied themselves with structures of scientific theories, there are also rich philosophical traditions that take seriously the social and historical epistemologies of experimental practices (2). For philosophers coming from Continental traditions, this beckons back to Bachelard and his concept of the phenomenotechnique, a concept used to theorize a deep relationship between the contingency of experimental methods and the contingent (but non-arbitrary) knowledge acquired with those methods. Much in the same spirit, Rheinberger has argued via case studies that experimental systems generally involve "mutual instruction" between scientific objects under study (epistemic objects) and instruments and technologies used in the investigation (technical objects) (3). From his Toward a History of Epistemic Things to his most recent book Split & Splice, Rheinberger has endeavored to provide us with a theory of experimental systems, one that captures both their temporal stability but also their capacity to surprise. In this presentation, we apply Rheinberger's philosophy of experimentation to the problem of cell-type classification. We focus on single-cell RNA sequencing (scRNA-seq). scRNA-seq has emerged over the last fifteen years to become the predominant platform used for "unbiased" classification of cell types in organs and tissues and it works by computationally sorting cells based on their individual mRNA transcription profiles. Overall, we argue that Rheinberger's framework helps make sense of the proliferation of cell types that emerge, stabilize, and/or vanish into obsolescence following widespread adoption and standardization of scRNA-seq platforms by diverse disciplines across biomedical research facilities. In support of our arguments, we rely on textual analysis of the scientific literature, in addition to engagement with staff and scientists at a university core facility specialized in scRNA-seq. Along the way we supplement our work with comparisons to other philosophy-of-science-in-practice studies of omics-type research, particularly those of other biomedical platforms, as well as histories of other earlier tools for measuring gene expression, including micro-arrays.

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The Epistemic Precautionary Principle for Research in Controversial Domains

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Certain kinds of research are controversial due to their potential to cause harm. For instance, research on sex/gender/race-based differences in IQ or the genetic causes of autism risk harming certain social groups. Likewise, many dual-use research that can be readily used for beneficial or harmful purposes (e.g., genetic alteration of viruses) involve risks to public health and safety, plants, animals, or the environment. Should such research be abandoned?

While some advocate abandoning or restricting such research on moral grounds (Kitcher 2001; Kourany 2020), others contend it would be detrimental to scientific progress (Ceci and Williams 2009; Flynn 2009). Such debates can often be distilled to a fundamental disagreement over which set of values—i.e., epistemic, moral, or social—to prioritize if scientific inquiry is to be undertaken responsibly.

Although undertaking scientific inquiry responsibly requires balancing various sets of values, prioritization is neither appropriate nor helpful in doing so. This paper proposes an alternative approach based on the "epistemic precautionary principle" (EPP), which can facilitate epistemically, morally, and socially responsible scientific practices in controversial research domains. I argue that responsible inquiries need not compromise scientific integrity because abandoning certain lines of inquiry can be both epistemically and morally virtuous under certain conditions.

I first distinguish selective ignorance (i.e., absence of knowledge resulting from selectivity in various stages of research) from the other kinds of ignorance in science, like falsehoods (i.e., false beliefs resulting from errors). I argue that, unlike the latter, selective ignorance is not inherently an epistemically detrimental state. While selective ignorance can be epistemically detrimental in some contexts, for instance, when it results from an inefficient distribution of epistemic resources and efforts (i.e., it entails an epistemic opportunity cost) or when the benefits and burdens of that ignorance are distributed unfairly (e.g., hermeneutical injustice), it can also be epistemically virtuous in other contexts. I then argue that selective ignorance can be epistemically virtuous if it results from adopting the EPP.

The EPP, an epistemic norm for facilitating collective and individual decision-making, places the onus on the proponents of controversial inquiries to show that if the inquiry must be undertaken, it can be conducted with a degree of epistemic precaution proportionate to the risk involved. It has two key components. First, when the benefits of pursuing an inquiry are not unequivocally evident, it shifts the burden of proof from those calling for constraints on the inquiry to those proposing it. In doing so, it calls for greater participation of the scientific community, the relevant stakeholders, and the public in risk assessment and decision-making. Second, when inquiries involve significant non-epistemic risks, it encourages careful consideration of the concomitant epistemic risks (e.g., opportunity cost, epistemic injustice) and urges extensive exploration of alternative options and mitigation strategies to minimize the harm. Therefore, it seeks to ameliorate social harms by mitigating the epistemic risks. In addition to improving scientific practices this way, the EPP promotes several epistemic virtues among the scientific community. Lastly, I discuss the application of this principle in various research domains.

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Dealing with Reproducibility Crisis in Biochemistry: Constructing Guidelines for a More Trustworthy Experimental Report on Enzyme Data

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Recent discussions on the reproducibility crisis have mainly centered around psychology and biomedicine, but biochemistry remains relatively overlooked by scholars in philosophy of science and science studies. If there is no general solution for the reproducibility crisis (Peterson 2021), we need more field-specific discussions. In this poster, I will discuss the meaning of reproducibility in the current efforts to deal with reproducibility crisis in biochemistry.

The STRENDA (The Standards for Reporting Enzyme Data) project, set up in 2003 and supported by the Beilstein Institute, aims for enhancing the trustworthiness of biochemical experiments, especially enzyme activities and their kinetic properties. While enzyme activities are dependent on the assay conditions under which they are determined (e.g., temperature, pH, ionic strength, etc.), the expression of enzyme activities is often ill-defined or arbitrary units are used, making it difficult to find any meaningful statistical estimation of the errors of all reported enzyme parameters (Tipton et al. 2014). As the commission claims, STRENDA aims to develop standards for reporting enzyme data to ensure comprehensive descriptions of the conditions under which they were obtained, recommending the guidelines to more than fifty international journals.

Following Nelson (2021), I reconsider reasons for challenges in conducting reproducible experiments, going beyond the common external factors such as publication incentives. One difficulty peculiar to the STRENDA project is balancing between "sufficiency" and "practicability." The guidelines aim to provide enough information for readers to comprehend and evaluate interpretation and conclusion in the experiments. Yet, they must also be practical, ensuring widespread use without being overly burdensome, especially as the guidelines are only recommended to the practitioners. This pragmatic consideration goes beyond the idea of reproducibility, which implies that if we can specify every parameter of a dataset and its experimental context, then in principle, we can always get the same experimental results. Then, how can we reconceptualize the meaning of "reproducibility" that is workable in this context?

Another challenge I will address is the "variability" inherent in experimental systems. Enzymes, conceived as parts of living things that manifest specificity and individuality, may complicate applying standardized protocols to the system of interest. While STRENDA strives for standards independent of the organism being studied and intended application of the data, Tipton (2014) suggests that, in fact, this may be an unachievable ideal. Then the project meets a fundamental question in biochemistry, asking how it is possible to justify in vitro experiments for understanding in vivo systems (Strand 1999).

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Philosophy for Science: The Case of Origins of Life Research

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Philosophy of science aims to articulate how science works and what our scientific theories refer to. In other words, philosophers help elucidate the epistemic and metaphysical foundations of science. Recently, philosophy in science (PinS) emerged as a purported alternative to philosophy of science. Whereas the latter does philosophy 'on' (or about) science, the former does philosophy 'in' science, that is, using concepts and ideas from philosophy to contribute to science by formulating 'scientifically useful proposals' (Pradeu et al., 2021). I will introduce a new proposal, philosophy for science. Philosophy for science encourages the development of new conceptual tools and frameworks to help find novel ways to understand and investigate existing problems in science. Philosophy for science is another way to practice philosophy of science. So, instead of seeing it as a rival alternative, philosophy for science should complement and work in tandem with philosophy in science. The central difference between my proposal and PinS is that whereas PinS focuses on making a new scientific proposal with existing philosophical tools, my proposal focuses on developing new philosophical tools to enhance understanding of existing scientific problems. I will show the utility of philosophy for science in relation to origins of life research. Specifically, I will develop conceptual tools to break down the different questions faced by and explanations available for an account of the emergence of life.

First, I will sketch an account of emergence I term methodological transformational emergence. This account considers the origins of life as a transformation of individuals in a domain D to individuals in a domain D*. The framework outlines the possible relations between the theories of D and the theories of D*. The options aren't restricted to reduction/antireduction, in which the theories of D* are either reduced or not reduced to the theories of D. Instead, there are a plethora of different potential relations between them. When applied to origins of life, the framework of methodological transformational emergence helps understand the options for an explanation of the emergence of life. The philosophical self-awareness the framework enables can help determine which accounts have unwarranted assumptions, as well as which experiments should be carried out to corroborate each account.

Second, I will outline five substrate independent properties of living systems that warrant an explanation when developing an account of the origins of life. Existing approaches may focus on explaining the origins of one or two of these while leaving others out. It is not clear whether the explanations they provide suffice to explain the origins of the remaining properties. If they do not, then the problem of origins has not been fully resolved. I will suggest that an explanation of these five distinctive properties should work as a desiderata for an account of the emergence of life. By bringing these properties to the fore, the problem of origins (i) can be understood in full detail and (ii) in a substrate independent way. The properties I propose are chemically agnostic, helping understand the origins of life anywhere in the universe.

Science and Tradition: Towards a Pluralistic Approach

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IBE is based on choosing the best explanation among the available options, claiming it to be at least approximately true. In his significant critique of IBE, known as the "argument from the bad lot" (Psillos, 1996), Bas Van Fraassen argues that IBE relies on a prior belief favoring the truth being more likely to reside within the available set of explanations than not (van Fraassen, 1989: 143). However, the veracity of this prior belief remains uncertain, leaving open the possibility of selecting the best explanation from a bad lot, thereby casting doubt on the reliability of IBE. Moreover, Kyle Stanford, in invoking the Problem of Unconceived Alternatives and drawing upon historical evidence, argues that we are not good at exhausting the available set of explanations or the "space of scientific alternative possibilities" (Stanford, 2006: 36), so the likelihood of bringing about 'bad lots,' is considerable. These criticisms raise profound concerns for scientific inquiry, highlighting our inability to exhaust our set of explanations and the consequent risk of bringing about bad lots and overlooking better alternatives yet unconceived.

One way to address this challenge is to enrich the set of explanations by incorporating more probable theories, thereby augmenting the likelihood of identifying correct or better theories. Scientific theories are deeply rooted in presuppositions and values underpinning the foundation of scientific inquiry and permeating scientific research. The origins of these foundational

elements can be traced back to the rich tapestry of traditions. Diverse traditions serve as cognitive and normative frameworks and offer rich sources of various presuppositions and values, presenting scientific inquiry with a spectrum of theoretical possibilities encompassing concepts, methods, problems, and worldviews. Exploring alternative opportunities rooted in the theoretical landscapes of different traditions can enrich available explanations. Therefore, attending to diverse traditions emerges as a viable strategy for enriching the set of explanations and mitigating the challenge to scientific inquiry.
Contemporary scientific inquiry, rooted in the Western intellectual tradition, rightfully holds a prominent position as the primary source of scientific advancement. However, it has often been reluctant to embrace the theoretical perspectives offered by non-Western traditions, thereby limiting their potential for significant influence within mainstream science. This reluctance stems from the dominance of Western tradition over its non-Western counterparts, resulting in a lack of recognition for alternative modes of scientific inquiry within Western scientific society (Kawagley and Norris-Tull, 1995). Some conservative and monopolistic tendencies persist within contemporary scientific society, hindering the acceptance of novel and distinct frameworks and theories outside the Western tradition. This stance diminishes the opportunity for other traditions to contribute meaningfully to mainstream science. Consequently, the potential of alternative traditions to serve as platforms for enriching the set of explanations remains largely untapped.

In conclusion, the overarching aim of this article is to advocate for the promotion of pluralism in scientific inquiry, particularly concerning the utilization of diverse theoretical possibilities from various traditions. Embracing a more inclusive approach, the scientific community can harness the insights offered by a more comprehensive array of traditions, thus providing the pool of explanations with more viable theories and less bad lots. Kafaee and Taqavi, in their work "The Value of Traditionality: The Epistemological and Ethical Significance of Non-Western Alternatives in Science," advocate for the recognition of Traditionality as a core value within scientific discourse (Kafaee and Taqavi, 2021). By integrating Traditionality as a foundational value, science can capitalize on diverse theoretical possibilities inherent in different traditions, thereby expanding scientific inquiry horizons. Embracing the richness of diverse traditions enhances the depth and breadth of scientific understanding and perspective on the complexities of the natural world.

Critical Making to Solve Wicked Problems: Changes in Student Knowledge and Attitudes about Problem Solving in a Philosophy of Science Classroom

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In this poster, I present preliminary results of an IRB-approved study (N=150 across six sections of the course) about changes in student knowledge and attitudes about problem solving in a philosophy of science class designed for science students.

Context
I teach an upper division philosophy of science and technology course for undergraduate STEM majors at a large public university. In order to motivate students to engage philosophically (and

historically and sociologically) with matters that are closely tied to their intended professional identities, I have structured the course around student-selected “wicked problems,” those issues that resist straightforward resolution because they involve multiple stakeholders with differing values and aims, there is disagreement about what a solution would entail, and even whether there is a problem in the first place. A distinctive feature of the course is the incorporation of “critical making” – a combination of critical reflective work common in philosophy classes with a constructivist making practice, in this case building low fidelity material prototypes of responses to the wicked problem, or more plainly, building cardboard models using kindergarten-level arts and crafts practices. We investigated whether students would learn new approaches to problem solving, transfer those practices outside of the classroom, and whether building models would have positive effects on understanding the problem, sophistication of the response to the problem, teamwork, or attitudes about problem solving.

Methods
Students completed pre- and post-course surveys with a mix of Likert scales and open responses. Under my guidance, a team of undergraduate researchers used grounded theory to code the open responses and then conducted follow-up interviews with a smaller subset of students to validate and extend their results.

Results
We found evidence that students gained comfort in sitting with uncertainty after completing the course. We also found improvements in the number and quality of problem-solving approaches students named. For example, in the pre-course survey, the most common answer was, “break the problem into manageable chunks.” In the post-course survey, the most common answer was, “research multiple perspectives,” followed closely by “research past failures.” While students clearly enjoy the model-building aspect of the class and there are some signs that it contributes positively to effective or satisfactory teamwork, there are mixed results as to whether this practice leads to learning gains regarding the problem or sophistication of the response.

Evidential Weight: Towards Resolving IBE’s Failure to Match Scientific Practice

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Inference to the best explanation (IBE) is often described as the method that scientists actually use to evaluate hypotheses. IBE is most commonly construed as using the theoretical virtues as a basis for hypothesis evaluation. Several different philosophers of science have catalogued the most important virtues, typically listing consistency, conservatism, simplicity, avoiding ad hocness, predictive power, causal adequacy, and explanatory scope. However, none of these lists make any mention of evidential weight: the fact that some pieces of evidence are more powerful than others. The virtue that seems to come closest to this concept would be explanatory scope/consilience. But this is quite insufficient. After all, explanatory scope is defined in only two dimensions: A hypothesis has more explanatory scope if it explains more facts, or if it explains a greater variety of different kinds of facts. That is, we can count tokens of

facts and types of facts that support a hypothesis. But no consideration is given as to how weighty those facts are. This oversight indicates a serious discrepancy between IBE and how claims are evaluated in real life. For in many historical cases the key factor in choosing among competing hypotheses has been just one or two pieces of very powerful evidence. For example, Kirk Bloodsworth was actually on death row for the murder of a nine-year old girl. This was because of several tokens and types of evidence that indicated his guilt—footprints that matched his shoe size, a witness who reportedly saw him in the vicinity of the crime scene, Bloodsworth's making comments about a "terrible thing" he did, etc. Despite these various tokens and types of evidence, he was released from prison in 1993 simply because new DNA evidence forcefully demonstrated that he was not involved in the girl's death. Likewise, in the 1980s Barry Marshall and Robin Warren proposed that the bacterium Helicobacter pylori was a primary cause of common stomach ulcers. After their original paper was published there were scientists who disagreed by pointing to several facts that did not fit with the H. pylori hypothesis. Marshall and Warren countered with more evidence and arguments. But according to gastroenterologists' testimony from the time, it was not a long list of facts that convinced most of them. Rather, it was a study in 1988 (not conducted by Marshall or Warren) that had great statistical significance and clearly confirmed the H. pylori hypothesis. In sum, accounts of legal and scientific reasoning frequently fail to match what we find in common listings of the theoretical virtues. It is often the case that evidential weight (not just tokens or types of evidence) is the key factor in warranting belief. This does not necessarily indicate that IBE was not being used. It really may be the case that considerations of explanatory quality were at play in these epistemic situations. But if these episodes are going to fit within an IBE (virtue-based) framework, a virtue that takes cognizance of evidential weight must be included.

Individual Based Research - A New Scientific Practice?

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In ecology, behavioral biology, and evolutionary biology a common scientific practice is to do research on a group-, a population-, and even on a species-level. This is done by collecting data of those groupings and calculating averages, standard deviations, growth rates, and other traits. However, a new trend seems to be taking root as scientists are becoming more and more interested in the individual differences and how these influence the groups, populations, and species (Bouchard and Huneman, 2013; Trappes, 2021, p. 5f; Uriarte and Menge, 2018). For example, if you want to know whether there are morphological size differences between two sexes of a population, you have to measure a number of individuals of both sexes and then group the results. By the end you have two groups of data based on individual measurements. You can now calculate the average sizes for both groups which may or may not reveal a significant size difference. With IBR, however, the information that can be gained, provides more insights into the individual and the intra-specific variation without losing any information about the group, population, or species.

Our research group is part of a larger collaborative research center (CRC) that includes biologists from ecology, behavioral, and evolutionary biology as well as two groups of

philosophers of science. Within this CRC we have observed a shift from focusing on groups to focusing on individuals. To support our observations, we created a questionnaire, conducted interviews, and held a discussion round. Most of the participants claimed they were doing individual based research and described their methods and processes in detail. However, when asked to explain what IBR is, most answered with “doing research on individuals” in one variation or another. This explanation is rather unsatisfying as it would allow almost anything to pass as individual based research including the example given earlier. Also, this explanation clashes with the methods and processes the biologists described. We found that IBR has at least three main characteristics: identifiability, multiple measurements, and individual-based analysis. All individuals should be identifiable. This can be done by spatial separation (placing individual fruit flies into test boxes), phenotypic traits (individual markings of fire salamanders), tagging (ringing birds), or tracking (GPS trackers on sea lion, e.g.: Schwarz et al., n.d.). Multiple measurements means that there has to be more than one piece of information. This can be done with an ongoing test that takes place over a longer period of time, tests that are repeated in a certain interval, or measurements of multiple traits without repetition. Lastly, the analysis of the data should be individual based. While the data can be grouped to calculate averages or variation within the groups, it is also analyzed in a way that allows a clear depiction of the individual. This makes the final results traceable and individually distinguishable.
This list of features is not exhaustive further analysis of IBR is taking place with the focus being on the epistemic consequences of conducting IBR.

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Mathematical SETIbacks: Open Texture in Mathematics as a new challenge for Messaging Extra-Terrestrial Intelligence

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The question of alien conceptual structures did not become a practical one until the first radio telescopes of the 20th century made it possible to send and receive messages from the stars. In this paper, I will present a new argument against the feasibility of making our signals understood by aliens and show that it is different from the more typical Wittgensteinian arguments for and against alien contact. The new problem I present, the Problem from Open Texture in Mathematics, is a practical problem that falls out of the history of mathematics and the implementation of real METI projects – specifically, the semiprime number self-decryption schema of the Drake Pictures message strategy. I argue that the version of the problem I present would remain a problem even under ideal circumstances, and thus that it is the strongest version of the argument.

Great ape iPS cells as a model system for human Evo-Devo

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This presentation examines the philosophical underpinnings of research into induced pluripotent stem cells (iPS cells) of non-human great apes (i.e., chimpanzees, bonobos, gorillas, and orangutans). iPS cells are pluripotent stem cells generated from somatic cells through artificially induced dedifferentiation. While much scientific and philosophical attention has been paid to the potential of human iPS cells in biomedical research, this is not the only use of iPS cells. Scientists generate and study iPS cells of various non-human animals for clinical, developmental, physiological, and evolutionary research. In particular, iPS cells of great apes have recently been actively studied for the purpose of elucidating evolution of human-specific traits—-large brain, characteristic craniofacial morphology, increased small intestine to colon ratio, decreased hair thickness and increased eccrine gland density, etc (Pollen et al., 2023).

In this presentation, I analyze reasoning practices associated with great ape iPS cells by employing conceptual resources from the philosophical literature on model organisms and experimental organisms (e.g., Ankeny & Leonelli, 2020). My discussion focuses on some characteristic features of great ape iPS cells: (1) Unlike model organisms, such as the yeast, fruit fly, and Arabidopsis thaliana, great ape iPS cells are not studied to produce results that are widely generalizable to many different taxa. They are studied to better understand evolution of a particular species: Homo sapiens. At the same time, unlike typical experimental organisms, which are studied to elucidate a particular biological phenomenon, great ape iPS cells are not targeted towards a single biological phenomenon. Rather, they are expected to contribute to solving a problem of human evolution, which consists of evolutionary changes in a wide range of developmental processes. (2) In the context of human Evo-Devo, great ape iPS cells are not studied as substitutes of the target system (humans). Rather, their role is to suggest ancestral states of certain characters or mechanisms, which are to be contrasted with those of humans (as discussed by Love & Yoshida, 2019). (3) Although great ape iPS cells are derived from cells of living organisms, they are extracted and separated from the organismal context and undergo significant experimental modifications. Although this is analogous to model organisms (which are separated from their natural environments and modified through breeding and genetic engineering), some of the modifications are peculiar to iPS cell culture and hence require distinct analyses.

My characterization articulates the representational role of great ape iPS cells as well as revealing conceptual and methodological issues with this newly emerging model system.

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