# SPSP 2022

## The 9$^{th}$ biennial conference of the Society for Philosophy of Science in Practice

2 – 4 July, 2022
Ghent University, Belgium
http://www.spsp2022.ugent.be/

# Contents

# About SPSP

Philosophy of science has traditionally focused on the relation between scientific theories and the world, at the risk of disregarding scientific practice. In social studies of science and technology, the predominant tendency has been to pay attention to scientific practice and its relation to theories, sometimes wilfully disregarding the world except as a product of social construction. Both approaches have their merits, but they each offer only a limited view, neglecting some essential aspects of science. We advocate a philosophy of scientific practice, based on an analytic framework that takes into consideration theory, practice and the world simultaneously.

The direction of philosophy of science we advocate is not entirely new: naturalistic philosophy of science, in concert with philosophical history of science, has often emphasized the need to study scientific practices; doctrines such as Hacking's "experimental realism" have viewed active intervention as the surest path to the knowledge of the world; pragmatists, operationalists and late-Wittgensteinians have attempted to ground truth and meaning in practices. Nonetheless, the concern with practice has always been somewhat outside the mainstream of English-language philosophy of science. We aim to change this situation, through a conscious and organized programme of detailed and systematic study of scientific practice that does not dispense with concerns about truth and rationality.

Practice consists of organized or regulated activities aimed at the achievement of certain goals. Therefore, the epistemology of practice must elucidate what kinds of activities are required in generating knowledge. Traditional debates in epistemology (concerning truth, fact, belief, certainty, observation, explanation, justification, evidence, etc.) may be re-framed with benefit in terms of activities. In a similar vein, practice-based treatments will also shed further light on questions about models, measurement, experimentation, etc., which have arisen with prominence in recent decades from considerations of actual scientific work.

There are some salient aspects of our general approach that are worth highlighting here.

1. We are concerned with not only the acquisition and validation of knowledge, but its use. Our concern is not only about how preexisting knowledge gets applied to practical ends, but also about how knowledge itself is fundamentally shaped by its intended use. We aim to build meaningful bridges between the philosophy of science and the newer fields of philosophy of technology and philosophy of medicine; we also hope to provide fresh perspectives for the latter fields.

2. We emphasize how human artifacts, such as conceptual models and laboratory instruments, mediate between theories and the world. We seek to elucidate the role that these artifacts play in the shaping of scientific practice.

3. Our view of scientific practice must not be distorted by lopsided attention to certain areas of science. The traditional focus on fundamental physics, as well as the more recent focus on certain areas of biology, will be supplemented by attention to other fields such as economics and other social/human sciences, the engineering sciences, and the medical sciences, as well as relatively neglected areas within biology, physics, and other physical sciences.

4. In our methodology, it is crucial to have a productive interaction between philosophical reasoning and a study of actual scientific practices, past and present. This provides a strong rationale for history-and-philosophy of science as an integrated discipline, and also for inviting the participation of practicing scientists, engineers and policymakers.

The SPSP board has voted in early 2022 to endorse the Barcelona Principles, aiming to give everybody equal weight irrespective of their background, accent, style, or fluency in English.

# Organising Committees

## SPSP steering committee

| | |
|---|---|
| Chiara Ambrosio | University College London |
| Justin Biddle | Georgia Institute of Technology |
| Julia Bursten | University of Kentucky |
| Till Grüne-Yanoff | Royal Institute of Technology (KTH), Stockholm |
| Catherine Kendig | Michigan State University |
| Sabina Leonelli | University of Exeter |
| Alan Love | University of Minnesota |
| Matthew Lund | Rowan University |
| Joseph Rous | Wesleyan University |
| Erik Weber | Ghent University |

## Local organising team UGent

Erik Weber (chair)
Pieter Beck
Thijs De Coninck
Leen De Vreese
Kristian Gonzalez Barman
Karina Makhnev
Julie Mennes
Pawel Pawlowski
Massimiliano Simons
Maarten Van Dyck
Qianru Wang
Ann Wyverkens
Karim Zahidi

## Acknowledgements

SPSP 2022 is financially supported by:

- Research Foundation – Flanders (FWO).
- the research fund of the Faculty of Arts and Philosophy of Ghent University.

The SPSP steering committee thanks Michel Durinx (webmaster) and Christine A. James (Twitter master).

# Philosophy and Philosophy of Science at Ghent University

The Centre for Logic and Philosophy of Science, which organises SPPS2022, is part of the Department of Philosophy and Moral Science. Our department offers a bachelor and master programme in philosophy, and a bachelor and master programme in moral science. Our philosophy program covers the traditional topics: history of philosophy from ancient to contemporary philosophy, epistemology, logic, philosophy of science, metaphysics, philosophical anthropology as well as theoretical and applied ethics. The aim is to give our students an advanced knowledge and grasp of theories, methods and skills in these fields. Our program in moral science has a different focus: it contains less logic, epistemology, philosophy of science and history of philosophy. Students in moral science are trained in empirical research methods, which allow them to study moral phenomena in a descriptive way (as opposed to the normative approach in philosophical ethics) and get a substantial background in the social sciences and psychology.

The Centre for Logic and Philosophy of Science was founded in 1993. Most of the research that is done at the centre fits into the following three research lines:

- Logical analysis of scientific reasoning processes
- Methodological and epistemological analysis of scientific reasoning processes
- Integrated history and philosophy of science

Examples of specific topics that fit into the first research line are: logical analyses of paraconsistent reasoning, reasoning under uncertainty, defeasible reasoning, abduction, causal reasoning, induction, analogical reasoning, belief revision, theory change, and conceptual change. Examples of specific topics that fit into the second research line are: methodological and epistemological analyses of causation and mechanisms, scientific discovery, experiments and thought experiments, scientific explanation, and evidence-based policy. The research in integrated history and philosophy of science includes work on scientists and philosophers such as Galileo, Stevin, Gassendi, Hooke, Euler, Van Musschenbroek, Lavoisier, etc.. and the history of philosophy of science in the twentieth century, with a focus on the tradition of historical epistemology (Koyré, Bachelard, Serres, Foucault, etc).

# Practical Information

## Coffee and lunch

Coffee, tea and lunch are included in the registration fee and will be served during the breaks. Vegetarian and vegan lunches will be served together with the regular lunch buffet so please pay attention to the sign at the buffet.

## Internet

If your home institution has eduroam, you will be connected automatically.
If not: make a wireless connection with `UGentGuest`. If you have set up to request an IP address automatically, you will receive an IP address starting with 193.190.8x.
Now you are connected, but not yet authenticated. You should start a webbrowser and you will be redirected to a logon screen. If not surf to `http://www.ugent.be`. Enter the username and password:
Login: guestSpsp20
Password: usnWBLEz
After correct authentication you can use the Internet connection. Your connection to this wireless LAN is not encrypted. To protect your personal data, please use encrypted connections like https, imaps, ssh etc. or a VPN client.
You are not allowed to pass on the login information to others.

## Detailed programme

Detailed up-to-date programme are available at the registration desk and on the website (`http://www.spsp2022.ugent.be/programme/`).
Twitter #SPSP2022 @SocPhilSciPract

# General Schedule

| | |
|---|---|
| **Friday 1 July** | |
| 10:00 – 17:15 | Pre-conference workshop |
| 18:00 – 19:30 | Registration & Pre-conference informal gathering (in pub Bluesette) |
| **Saturday 2 July** | |
| 08:15 – 09:00 | Registration |
| 08:50 – 09:00 | Opening session: Welcome |
| 09:00 – 10:15 | Plenary Lecture 1 |
| 10:15 – 10:45 | Coffee Break |
| 10:45 – 12:15 | Concurrent Sessions 1 |
| 12:00 – 13:30 | Lunch & Poster session |
| 13:30 – 15:30 | Concurrent Sessions 2 |
| 15:30 – 16:00 | Coffee Break |
| 16:00 – 17:30 | Concurrent Sessions 3 |
| 20:00 – 21:30 | Visit to GUM (Ghent University Museum) |
| **Sunday 3 July** | |
| 09:00 – 11:00 | Concurrent Sessions 4 |
| 11:00 – 11:30 | Coffee Break & Poster session |
| 11:30 – 13:00 | Concurrent Sessions 5 |
| 13:00 – 14:15 | Lunch |
| 14:15 – 16:15 | Concurrent Sessions 6 |
| 16:15 – 16:45 | Coffee Break & Poster session |
| 16:45 – 18:00 | Plenary Lecture 2 |
| 19:30 – 22:00 | Conference dinner |
| **Monday 3 July** | |
| 09:00 – 11:00 | Concurrent Sessions 7 |
| 11:00 – 11:30 | Coffee Break |
| 11:30 – 12:45 | Plenary Lecture 3 |
| 12:45 – 14:00 | Lunch & Newsletter meeting |
| 14:00 – 15:30 | Concurrent Sessions 8 |
| 15:45 – 16:30 | Closing session |

# Abstracts of Plenary Lectures

## (in order of appearance)

### Scientific Disciplines as Ecological Niches for Heuristic Method Choice

*Till Grüne-Yanoff*

*Royal Institute of Technology (KTH) Stockholm*

`gryne@kth.se`

Methodology concerns the rational choice of method for scientific purposes. If there is no universal logic that governs such choices, as many seem to agree, then what determines their rationality? In this talk, I argue that scientists choose between methods heuristically: they employ simple decision procedures that disregard available information. Drawing on cognitive science accounts, I show that such heuristic methodologies can be normatively assessed, and under certain conditions might be rational. One important condition for this normative assessment is its context-dependence: the same heuristic (employed for the same purpose) might be judged to be rational in one environment, but irrational in another. This context-dependence, I further argue, points to important functions that scientific disciplines perform in heuristic methodology. On the one hand, disciplines are often claimed to be distinguished by purpose and domain considerations. Such considerations might serve as justifiers for method choice, so that disciplines can be seen as embodying methodological heuristics. On the other hand, method choices often differ between discipline even if purpose and domain are similar. I explain this as methodological niche construction: scientists construct a disciplinary culture - e.g. incentives, tools and information formats - that favors certain method choices over others. Contrary to much of the literature on interdisciplinarity, I argue that such niche construction can serve epistemic goals, specifically the institutional stabilization of heuristic diversity. I conclude that a heuristic account of method choice provides a rationale for the disciplinary organization of science, offers a counterargu-

ment against institutional interdisciplinary integration, and identifies new modes of interdisciplinary collaboration.

---

**'Poetry is Not a Luxury': Quantum Physics – When Grammar Fail**

*Karen Barad*
*University of California at Santa Cruz*
kbarad@ucsc.edu

Niels Bohr once commented on the fact that the very structure of a grammar that entails nouns and verbs is a trap that assumes that there are independently existing entities that engage in activities, and that this makes it difficult to say what quantum physics entails. The first question that may arise is: why would a physicist have anything to say about grammar?

In this paper, I will summarize some key points of my agential realist relational ontology interpretation of quantum physics. This interpretation entails a further elaboration of Bohr's philosophy-physics that provides an account of quantum physics that goes beyond "piddling laboratory exercises", moving from Bohr's specifically epistemological focus to a more general ontological framework that implicates causality and spacetime more generally. Furthermore, moving from, what I argue is, Bohr's understanding of concepts as physical laboratory arrangements to a more general material-discursive articulation of the world in its iterative reworlding, agential realism is a framework articulated in conversation with and with implications for social and political thought. This brings to the fore a political analysis of quantum physics together with a quantum analysis of political thought.

Agential realism has at its heart the notion of intra-action – which challenges the noun-verb grammatical structure – rather than the common notion of interaction among things. This disruption is not mere word play but grounded in physics and material engagements within and as part of the world. Taking this a step further, into an exploration of quantum field theory (a more general account than the specific theory of quantum mechanics), I will offer some thoughts on an agential realist interpretation of quantum field theory, while further elaborating agential realism as a practice of engagement attuned to the inseparability of scientific and social and political thought. In particular, in this talk I will focus on the remarkable extent to which grammar falls apart in ways that make a rigorous scientific exposition necessarily one of poetics. As Audre Lorde puts it: "poetry is not a luxury. It is a vital necessity of our existence. It forms the quality of the light within which we predicate our hopes and dreams toward survival and change ....”

---

### Thinking about Laws in Political Science: Democratic Peace and Balance of Power

*Erik Weber*
*Ghent University*
erik.weber@ugent.be

There are several theses in political science that are usually explicitly called ‘laws’, e.g. Duverger’s Law (which states that an electoral system with plurality rule leads to a two party system) and the Iron Law of Oligarchy (which states that organisations after some time inevitably are ruled by a small elite group). In the last decades, there have been discussions among political scientists about whether such theses really should be called a law. The trigger of each debate is the habit of calling the thesis a law. Two positions are possible: one can endorse the habit and support this with epistemological arguments, or do the opposite.

A project for philosophers-of-science-practice emerges here: investigating which theses in political science really deserve the label ‘law of politics’. If we zoom in on international relations theory – as I will do in my talk – the Democratic Peace Proposition (which states that democracies almost never go to war with each other) is an obvious candidate for such a philosophical investigation. Another candidate is the Principle of Balance of Power (which states that near-hegemonic concentrations of power in a multistate system nearly always trigger a counterbalancing coalition of the other great powers).

My talk has five parts. First, I elaborate on the scientific practice from which I start. Second, I clarify the concept of ‘law of politics’ that I use. Then I apply this concept to democratic peace and to balance of power. Finally, I point at some conceptual confusion among political scientists (determinism versus lawhood).

---

# Abstracts of Symposia

## (alphabetical by last name of organizer)

### Protein Dynamics: Theory and Practice

#### Organizers & contributors: Andrew Bollhagen, William Bechtel, and Jacob Neal

Our session focuses on the interaction of theory and practice in studying protein dynamics. Our first speaker, Jacob Neal, discusses the interaction of theory and experimental practice that drove the development and eventual acceptance of the view that proteins are dynamic, rather than static, structures. On his account, the uptake of the dynamic view indeed involved the development of new technologies which aided in the discovery of phenomena anomalous from the "static" point of view but, he argues, it was the theoretical commitment to treating proteins as small thermodynamic systems that provided the impetus for the search for empirical anomalies. Our second speaker, Andrew Bollhagen, describes the practice of using what researchers can see—reconstituted molecular motor-driven movement of filamentous proteins and plastic beads—to measure what they cannot: the dynamical activity of motor proteins. He uses, as a case study, the efforts of the Spudich Lab at Stanford to reconstitute actomyosin-driven motility. Finally, William Bechtel discusses the practices involved in researchers using static images of proteins, derived from X-ray crystallography and Cryo-EM, to generate accounts of how proteins change their conformation in performing cellular activities. Together, we aim to provide a rich overview of the theoretical and practical aspects involved in studying the nanoscale dynamical activity of proteins.

---

# From Static to Dynamic: Investigating the Interplay between Theory and Experiment in the Emergence the Dynamic View of Proteins

*Jacob Neal*
*The Rotman Institute of Philosophy at Western University, Canada*
jacobpneal@gmail.com

Changes in scientific representations of proteins have recently undergone a dramatic shift from static to dynamic. The aim of this paper is to identify the causes of the shift by examining the interacting and often competing role of theory and experiment. I argue that the driving role of theoretical commitments held by early adopters of the dynamic view have been overlooked by historians and scientists who have sought to explain this shift.

For the first half of the twentieth century, the dominant view of protein structure held that proteins were rigid, compact, and largely static molecules. Emil Fisher's lock-and-key model of enzyme catalysis epitomized this static view, and protein x-ray crystallography beginning in the 1940s seemed to offer experimental confirmation for it. An alternative dynamic view of proteins arose in the 1970s and 1980s. It treated protein molecules as small thermodynamic systems and emphasized that proteins in solution would undergo constant structural fluctuations. Because of the Brownian-like motion of solvent molecules colliding with the protein, any individual protein molecule would be constantly interconverting among different structural states.

The rise of this dynamic view raises a historical puzzle about the quarter-century time lag between the origin of the view and its eventual acceptance. One plausible explanation suggests that the dynamic view of proteins in the 1970s and 1980s was a theoretical view awaiting experimental confirmation (Cui and Karplus 2008, Hilser et al. 2012, Morange 2020). Protein dynamics, on this account, would only be taken seriously after advances in experimental techniques, such as protein NMR, enabled researchers to visualize protein dynamics at high resolution. I contend that this explanation is partial at best. Although technological advances played a part, I argue that theoretical understanding of protein dynamics was a crucial driver behind the emergence and uptake of the dynamic view of proteins.

In the first place, the application of general thermodynamic principles to proteins led to the development of the dynamic view, which contested the static view with its x-ray crystallographic images of static proteins. Then, this shared commitment to treating proteins as thermodynamic systems led a small cadre of scientists to use the technologies available in the 1980s and 1990s to produce empirical evidence in support of the dynamic view. Motivated by their theoretical commitments, these early adopters slowly accumulated empirical evidence for the dynamic view. Their empirical findings, along with additional evidence for protein dynamics resulting from new technologies, eventually convinced the majority of protein scientists by the 2000s and 2010s to adopt the dynamic view. Explaining the uptake of the dynamic view, therefore, involves the discovery of anomalies aided by new technologies, but my work shows that it was the theoretical commitment to treating proteins as small thermodynamic systems that provided the impetus for the search for empirical anomalies.

---

## Reconstituting Molecular Motor-Driven Movement

*Andrew Bollhagen*
*University of California–San Diego, United States*
Abollhag@ucsd.edu

The dynamics of molecular motors—proteins that transform the chemical energy stored in ATP into mechanical movement—is a nanoscale phenomenon and, thus, not one that can be observed directly. In this talk, I discuss how researchers developed reconstituted systems to use what they could see—visualizable molecular motor-driven movement of filamentous proteins and plastic beads—to study what they could not, the mechanical dynamics of the motor proteins driving that movement.

Reconstituted systems are often glossed as means of vetting models of molecular mechanisms: "Conducting in vitro reconstitution experiments can confirm and refine molecular models of processes outside the complicated environment of cells" (Liu and Fletcher 2010). When used for this purpose, a key aspect of the practice involves drawing comparisons between the phenomenon reconstituted in vitro and its in vivo correlate. Noting failures of reconstituted systems to resemble the corresponding in vivo system prompts researchers to develop new iterations of the reconstituted system that more closely resemble the in vivo phenomenon in those respects in which it was lacking. Such comparisons thus drive a kind of "bootstrapping" procedure, the result of which is to develop support for analogical inferences drawn from the reconstituted system to the corresponding in vivo system.

In the practice of using reconstituted systems as instruments of measurement, however, the need for the reconstituted system to closely resemble any particular in vivo phenomenon becomes less of a priority. In such cases, what is important is that the outwardly observable features of the in vitro reconstituted system can serve as data that enables measurement of the underlying molecular processes. This is a distinctive desideratum. In my talk, I describe the "bootstrapping" procedure involved in meeting it, using as a case study the reconstitution of myosin-driven movement in the Spudich Lab at Stanford.

Myosin is a dynamic protein—a "molecular motor"—that drives a number of motility phenomena in cells, for instance muscle contraction and cytoplasmic streaming, through its interaction with the filamentous protein actin (hence, "actomyosin"). The Spudich Lab aimed to reconstitute not any one of these motility phenomena but, rather, to reconstitute motility in a form that could be used as a measure of particular features of the mechanical interaction between the proteins. I discuss various iterations of the reconstituted system, drawing attention to the criteria that were deployed in vetting these iterations and to how subsequent iterations attempted to overcome the shortcomings identified. Ultimately, these researchers succeeded in developing a system that enabled the measurement of the rate at which myosin slides over actin filaments and the "step size" of single myosin molecules. The case study illustrates the "bootstrapping" procedure at the core of this practice, and describes how researchers use reconstituted systems to make nanoscale measurements of the dynamic activity of molecular-motor proteins.

---

## Constructing Dynamical Models of Conformation Change in Proteins from Static Images

*William Bechtel*

*University of California–San Diego, United States*

wbechtel@ucsd.edu

How do researchers, limited to static images of proteins, generate accounts of how proteins change their conformation in performing cell activities? I will describe and analyze the practices through which researchers have developed models of conformation change in proteins using static images from X-ray crystallography or Cryo-EM. They integrate several practices in conducting such research. The first creates representations of the structure of the protein from the X-rays or EM images generated when the protein is frozen in conditions that are thought to correspond to stages in the cellular activity. The second compares these images to determine which parts of the protein move. Then, drawing upon what they know of the constraints provided by the structure, investigators piece together models of how forces could be transmitted from the hydrolysis of ATP to effect movement of different parts of the protein.

I focus on two examples: the myosin motor that generates muscle contraction and the KaiC complex that generates circadian rhythms in cyanobacteria. In both cases researchers knew that the process of conformation change was cyclic with the protein restored to its earlier conformation at the end of the process. They also knew the key energetic steps. In the case of myosin, various microscopic techniques revealed that it successively created crossbridges to actin, applied force to move along actin, and released. Biochemical studies revealed the steps in the process at which ATP was bound and hydrolyzed. In the case of KaiC, researchers knew that two sites in KaiC successively bound ATP and then, in the same order, are dephosphorylated. They also linked these processes to changes in how KaiC bound two other proteins, KaiA and KaiB.

To employ either X-ray crystallography or Cryo-EM, researchers needed to render the molecules static in states that could be expected to correspond to ones they assumed in vivo and generate images. These images had to be solved to determine the protein structure in each state. By comparing the identified structure in different states, they determined how the protein conformation changed between points in their cyclic activity and began to construct a model. In the case of myosin, they focused specifically on three components, the ATP binding site, the actin binding site, and the lever arm, and proposed how the force released at the ATP binding site could be transferred through the structure to the other sites. In the case of KaiC, the task was more complex as researchers knew that changes occurred at multiple parts of the Kai complex (KaiC is a large hexamer with two domains, each of which binds at various stages either KaiA or KaiB) but they have recently fit the pieces together into a coherent account. The successes that have been achieved in these cases resulted from coordinating different practices: constructing the needed static images, solving for the protein structure, identifying where the structures changed, and modeling how the forces were transmitted through the protein.

## P-Medicine: Reflections on the Practical Implementations of Health Personalisation

**Organizers: Stefano Canali and Yael Friedman**

**Contributors: Simon Lohse, Karin Christiansen, Stefano Canali, Yael Friedman, Sara Green**

In recent years several philosophers, sociologists, and historians of science and medicine have critically analysed p-medicine – an umbrella and constantly evolving term that encapsulates various proposals and movements that aim to increase the personalisation, precision, prevention, and person-centredness of medicine. Building on this literature, in this session we focus on a crucial aspect of the changes brought by p-medicine and discuss an overarching question: Which tensions arise between the propagated benefits and practical implementations of health personalisation? In this session, we present four contributions and a panel discussion that provide answers to this question from different epistemic, ethical, and methodological perspectives and are grounded on close analyses of biomedical practice at the research and clinical level – thus showcasing the merits of a philosophy of science in practice approach to these crucial issues. P-medicine is often mentioned as a way of bringing several benefits to health. Personal data collection based on the use of personal information technologies can enable constant self-monitoring, thus expanding disease prevention at the individual level. More precise medical interventions can improve clinical treatment and reduce side effects for specific patients. A more participative and active role for patients in clinical research can amplify disease classification and support patient empowerment. Yet, p-medicine is also a promised frontier, where various tensions are intertwined with benefits. The distribution of benefits from personal data collection is often misbalanced and concentrated in large corporations rather than individual users, which creates ethical dilemmas of control and burdens of privacy and security. In addition, new validation methods are required as precision tools create additional uncertainties and more active patient participation calls for a consideration of first-person perspectives in medical definitions. We need a multidimensional reflection on these tensions in order to adequately assess the potential and limitations of p-medicine.

As a contribution in this direction we present the following four talks, a commentary, and a panel discussion. Simon Lohse will open the session focusing on uncertainty, with a critical analysis of sources and relations between types of uncertainty, as a structural feature and paradox of p-medicine. Varying levels of uncertainty and plausibility will be discussed by Karin Christiansen, with an analysis of the ethical dilemmas arising from patient participation, self-monitoring, and new forms of control in p-medicine. Exploring technologies for self-monitoring and control, Stefano Canali will focus on wearable devices, framing them as p-medicine technology and identifying epistemic issues at the interface between wearable technology and p-medicine. Yael Friedman will discuss the broader issues of including patient perspectives in p-medicine, analysing conceptual discrepancies and epistemic injustices in the relation between patient and biomedical views of health. The commentary by Sara Green will reflect on the themes of the presentations with an outlook on further research and opening our panel discussion.

---

## Mapping Uncertainty in P-Medicine

*Simon Lohse*
*Universität zu Lübeck, Germany*
simon.lohse@uni-luebeck.de

P-medicine promises a more fine-grained understanding of the complexity of diseases and to develop innovative therapies that are precisely tailored to individual patient groups. To achieve this goal, it relies on the analysis and amalgamation of complex forms of evidence, using genomics, metabolomics and other omics technologies as well as e-health data sets and sophisticated IT infrastructure. However, there are deep epistemic concerns about P-medicine's ability to deliver on its promise. Many important concerns revolve around what has been described as the paradox of P-medicine, namely that uncertainty seems to be a key characteristic of P-medicine, in particular uncertainty regarding its evidential basis in relation to clinical decision-making (Kimmelman and Tannock 2018). This observation is paradoxical because it is in tension with the main rationale of P-medicine to increase certainty through a more exact understanding of diseases and individualised therapies.

In a much noted commentary in the New England Journal of Medicine, Hunter (2016) describes several aspects of uncertainty in P-medicine with an emphasis on uncertainty regarding testing procedures and the interpretation of ever more large, complex and probabilistic data sets in view of their therapeutical implications. Other sources for epistemic uncertainty that have been highlighted in the biomedical literature include unclear evidence thresholds for stratifying diseases into subtypes, opaque machine learning algorithms for predicting therapeutic outcome, and different ways to deal with genetic variants of unknown significance in clinical settings (e.g. Pollard et al. 2019). Uncertainty is also discussed as a feature of P-medicine in the philosophy of science and science studies literature. For example, the unclear causal status of genetic markers and the unreliability of genomic testing have been cited as contributing to uncertainty in P-medicine. Moreover, "uncertainty management" in clinical practice has been identified as one of the key emergent issues in this field of medicine (Kerr et al. 2019; Green at al. 2019).

In summary, uncertainty is highlighted as a structural feature of P-medicine in practice. While uncertainty is a typical feature of many new developments, in particular in biomedical research, technology development and medicine, in P-medicine more specific questions of uncertainty seem to become relevant too. In particular, there seem to be systematic links between uncertainty in P-medicine and other much-noticed aspects of this field – such as its complexity, its reliance on big data technologies, and its aim to reorganise disease taxonomies – that are in need of further exploration. However, there neither exists a comprehensive overview of factors that may contribute to uncertainty in P-medicine nor a description of its main characteristics (or forms).

This talk will take first steps to remedy this situation. Drawing on work in a large P-medicine consortium, I will provide a critical overview of sources of uncertainty in P-medicine on an ontological, conceptual, evidential, and technological level, thereby generating a high-resolution picture of the uncertainty paradox. Based on this picture, I will briefly discuss implications for the theoretical and practical assessment of translational research and therapy in the context of P-medicine.

---

## Ethical challenges of personalized and predictive medicine – reflecting on a future scenario

*Karin Christiansen*
*VIA University College, Denmark*
Kach@via.dk

P-medicine is branded as the new paradigm in health care. As Eric Topol puts it, "we are at a unique juncture in the history of medicine, with the convergence of genomics, biosensors, the electronic patient record and smartphone apps, all superimposed on a digital infrastructure, with artificial intelligence to make sense of the overwhelming amount of data created. This remarkably powerful set of information technologies provides the capacity to understand, from a medical standpoint, the uniqueness of each individual – and the promise to deliver healthcare on a far more rational, efficient and tailored basis" (The Topol review, 2019).

While these words provide a strong narrative of hope about the power of technique, data and human ingenuity to bring about reliable and secure health management, illness prediction, treatment and prevention now and in the future, P-medicine could also open up a Pandora's box of paradoxes, ethical dilemmas and potential ills for humans, which are hard to foresee.

Leading health experts have stressed the importance of understanding the basic principles of these novel technologies, in order to engage in critical discussion about their merits, scientific limitations and ethical dilemmas. Fundamental epistemic and ethical questions about what we need (and want) to know, what counts as knowledge and to what extent predictions about the future can and should be made on the basis of accumulated, assembled, measurable and computable data are currently being negotiated between clinicians, researchers, administrators, industry, patients and citizens. Our knowledge of the genome is far from complete and since we know that many of the current insights will be revised in the course of time, we are often faced with serious ethical dilemmas relating to its predictive power. It seems inevitable, however, that the development of risk-profiles, based on a multitude of assembled health data, will have the power to impact options for and choices made by ordinary people regarding partners, children, jobs, insurance, loans, death, etc.

In my talk I will address a series of ethical dilemmas relating to predictions based on health data, including genomic data. I will discuss a future scenario described by Prof. Michael Snyder (Snyder, 2016) in which genome sequences are determined before birth and epigenomes and a wealth of other information are commonly used to predict, diagnose and treat disease. In this narrative, a customized treatment plan is routinely developed for each person. The citizen or patient is expected to participate actively in self-monitoring activities and make proactive choices regarding their medical destiny and that of our children. Customized treatment is decided mainly by treatment algorithms, which can be administered outside the domain of the health system. To what extent this future scenario is plausible and might lead to new forms of behavioral and moral control will be the major theme of my talk.

**Wearables as P-Medicine Technology: Identifying Epistemic Relations and Issues**

*Stefano Canali*
*Politecnico di Milano, Italy*
stefano.canali@polimi.it

An increasing number of wearables devices, such as watches, patches, garments, are equipped with sensors that can track biomedical parameters, including heart rhythm, body temperature, respiratory rate. In this paper, I discuss wearables with the aim of identifying epistemic principles, assumptions, and issues involved in the use of this technology for health. I argue that wearables are a type of p-medicine technology, with shared principles, goals, and methods between p-medicine epistemology and the design and employment of wearables. While the use of wearables in this direction is promising, I discuss epistemic issues that are particularly and mutually significant for debates on p-medicine and wearables.

Relying on the philosophical literature on p-medicine, I start my analysis by introducing the main principles, goals, and methods of p-medicine and discuss their relation with wearables. The wearable context is permeated with ideas of precision, for instance as a response to poor quality standards in e.g. disease monitoring. More importantly though, current uses of wearable technology share principles, goals, and methods of p-medicine, which I show by highlighting the connection with specific functions that wearable technology is currently used to serve for health. For instance, disease monitoring and prediction based on wearables clearly goes in the methodological direction of p-medicine, while this type of research is traditionally based on population level data. In the clinical context, wearables have been applied to intervene on e.g. physical activity and fitness, acting on individual traits and tailoring intervention to specific patients – thus serving the main epistemic goals of p-medicine.

I frame wearables as an emerging p-medicine technology, which is not without issues. First, I argue that the wearable context is particularly interesting to look at since several issues discussed in the critical literature on p-medicine gain specific and different meanings. I discuss overestimation as an example in this direction: epistemic and ethical risks of overestimation in disease screening and prediction are frequently discussed in p-medicine, but are particularly salient for wearables, which can deliver consequential and actionable information on very personal aspects of individual health. Second, some issues affect wearables but have only partially been discussed for p-medicine. For instance, data quality is increasingly difficult and problematic to assess for wearables because of the variability of these devices, lack of access to contextual information on data practices, and issues of representativity. While these are significant issues for wearables, their presence and severity signal the need for more discussion on data quality as a crucial issue for p-medicine too.

I conclude by framing the paper as a contribution for the philosophical debate on p-medicine, where discussions on the role of wearable technology have been scarce, and the philosophy and ethics of technology, where identifying epistemic issues can improve our ability of harnessing benefits in the use of (health) technology.

---

## Patient Participation: A Conceptual Challenge for P-Medicine

*Yael Friedman*
*University of Oslo, Norway*
yaelf@uio.no

One of the prominent P's included under the elusive umbrella term 'P-medicine' stands for patient Participation. Patient participation means increasing patient engagement in decision-making and their responsibility for their own health. In addition, increasing participation can also help achieve three other goals: personalized treatment, personalized prevention, and patient-centered care. These goals require collecting and analysing a vast amount of data in all aspects of the patient's life, which often leads to the description of p-medicine as presenting a more holistic model of medicine. However, taking the patient role seriously and centering medicine and care around the patient requires more than the interpolation of personal data; it should include reconsideration of the epistemic role of the first-person perspective in basic medical terminology, which is used for communication and designs the medical decision making. In this talk, I will suggest that in order to create a more holistic view on the patient, p-medicine should deal with the conceptual challenges that follow the inclusion of the patient perspective.

One of the conceptual challenges is a possible gap between the patient's view and the biomedical view. I will focus on two basic medical terms to demonstrate this gap: diagnosis and recovery— both are key concepts of medical practice. At one end, diagnosis bears the logical weight of medical science and serves as the epistemic hinge for decision-making. At the other end, recovery marks the finish line of a successful process, where one is no longer considered a patient. However, taking from two different perspectives, the biomedical and the phenomenological, the entry and exit points in and out the medical process, could be understood very differently. Both terms currently lack important nuances to distinguish between those views; in the p-medicine context, they create more conceptual discrepancies regarding individuals' conditions.

Conceptual discrepancies between the biomedical and the phenomenological view can raise the question whether someone should be considered a patient or not; in other words, whether they require medical treatment and are eligible for health benefits. Take for example the case of a gap between self-diagnosis (Hannah feels ill and suffers from extreme muscles pain) and biomedical no diagnosis (the doctors say there is nothing biomedically wrong with Hannah); and between biomedical recovery (Alma's cancer tumours were removed and she is now cancer-free) and phenomenological recovery (years of fighting with cancer left Alma completely troubled, she still sees herself as a patient in need for support).

When the biomedical and phenomenological perspectives do not go hand in hand, like in the cases of Hannah and Alma, it can lead to epistemic injustice. Epistemic injustice causes us to privilege some agents' perspectives, like on diagnosis and recovery, while rejecting those of others as epistemically relevant, although they are in principle no less pertinent. Epistemic injustice can worsen people's conditions. Our current terminology glosses over the epistemic role of the patient. Therefore, genuine patient participation for personalized medicine and care would require a terminological change that will acknowledge these perspectives differences and their epistemic value.

---

**Comments**

*Sara Green*
*University of Copenhagen, Denmark*
sara.green@ind.ku.dk

---

## The Philosophy of History in Practice

### Organizers & contributors: Adrian Currie, Kirsten Walsh and Karoliina Pulkkinen

"Professional historians might object, too, to the emphasis on narrative historiography. Professional history, a historian might say... does not exclude the construction of narrative accounts, but that is a literary skill quite independent of professional skill in actual research." – Louis Mink, Narrative Form as Cognitive Instrument.

Philosophers of Science in Practice complain that the philosophy of science has paid far too much attention to the products of scientific work: theories, evidence and explanations, to the detriment of scientific processes: theorizing, generating data, performing and designing experiments, and so forth. This symposium suggests a similar complaint could and should be made about much of the philosophy of history and explores the consequences of taking historical practice philosophically seriously.

From the early 1970s onwards, philosophers in the so-called 'linguistic turn' in the philosophy of history, such as White (in Metahistory, 1973) and Mink (above) emphasized discontinuities between the narrative products of history and historiography on the one hand, and historical research on the other. This involved emphasizing the similarities between historical and literary understanding and downplaying the role of the historical record in determining what histories are told. Although there are important lessons in this tradition—most especially in capturing the crucial roles of ideology and value in the construction of history—splitting historical narratives from other historical practices can only provide an impoverished philosophy of history.

A crucial historical practice is digging into and exploring primary texts in archival contexts. Historians do not simply weave chronologies into explanatory narratives, they also both evidence and construct those products via the discovery and interpretation of those texts. Recognising the role of archival research has at least two important upshots for philosophers of history. First, as Currie argues in his paper, there is plausibly a two-directional, iterative relationship between the construction of narratives and archival research. Second, as Walsh makes clear, this shows that archival work itself—how the historical record comes to be preserved—plays a much greater role in shaping the narratives historians tell than philosophers of history have appreciated. Just as political and social contexts shape how archives are constructed, they also shape how history is told. Walsh emphasizes this in how the Royal Society shaped their archives, and Pulkkinen explores this relationship through the example of B.M. Kedrov, the Soviet historian and philosopher of science who especially focused on chemistry. Kedrov's detailed work on the edited volumes of Russian chemist Dmitrii Mendeleev's early papers illustrates how Soviet historians avoided conforming to ideological demands by focussing on publishing collections of primary source materials. Generally, Soviet historians' focus on creating collections of primary source material over detailed narratives of events puts pressure on the 'linguistic' turn on philosophy of history. Moreover, it suggests that there needs to be greater emphasis on the broader political and social context in which historians operate.

Overall, then, recognising that historiographical narratives are intimately tied to other historical practices provides a richer and more accurate conception of the philosophical issues arising concerning history. That is, it opens the door to a philosophy of history in practice.

---

## Historical Events, Historical Records & Winston the Platypus

*Adrian Currie*
*University of Exeter, United Kingdom*
a.currie@exeter.ac.uk

The most influential work in the philosophy of history is focused on the construction of narratives—of the explanations historians give—at the expense of other important aspects of historical practice such as digging into archives, analysing primary texts, and chasing up their validity. This, I think, is a mistake: I'll suggest that to understand the nature of history, we should understand the relationship between those latter practices and the construction of narratives. I'll make my argument via a discussion of Mink's broadly anti-realist account of historical narratives, in particular his view that historical events are only such in light of the narrative in which they are embedded. I'll suggest that an examination of historical practice shows that this is a mistake: 'historical events' emerge via a combination of narrativising and evidence from the historical record. Crucially, this shows that narratives are far more sensitive to evidence than Mink and others allow for.

I'll also consider one of Mink's metaphysical arguments: he argues that narrative structures cannot be part of the world (that historians do not discover historical narratives) because, if they were, we should be able to integrate narratives—and we cannot. Due to the holism of narrative structures, combining narratives simply creates new narratives. I'll argue that this argument fails as it excludes a further option: historical events are complex and are parts of multiple narratives simultaneously. This promiscuity about narrative events is consistent with them not aggregating. Throughout I'll refer to Natalie Lawrence's (2011) argument that the fate of Winston the Platypus—the subject of a failed attempt to export a monotreme from Australia to the UK during the second world war—needs to be understood in the context of both wartime relationships between Australia and the United Kingdom, the role of zoos in colonial wars and notions of 'gift-giving' in international relations. This is in contrast with more traditional stories that emphasize Churchill's idiosyncracy. Although both the traditional story and Lawrence's agree on the chronologies, they are quite different narratives, and Lawrence brings to bear multiple streams of primary evidence in the forms of various communications, telegrams and newspaper articles to defend her view. For Mink, the case should be understood as two different narrative 'cognitive devices' which cannot be contrasted on evidential grounds. I'll argue that the case is better understood in terms of an iterative relationship between the archives Lawrence examines and the narrative she constructs. As such, a narrative history is not drawn from a pre-established chronology, but chronology and history emerge together. An upshot of this is that narratives are highly sensitive to the historical record, albeit in complex ways.

---

## Philosophy and History of Science in the Soviet Union: the case of Bonifatii Mikhailovich Kedrov

*Karoliina Pulkkinen*

*University of Helsinki, Finland*

karoliina.pulkkinen@helsinki.fi

The values in science literature brought the spotlight to the fact that science is not separate from society. Social values influence scientific research in myriad ways. Unsurprisingly, also humanities are influenced by the broader social and political context, and philosophy of science is no exception (Reisch 2005). Here, I'll investigate how political circumstances influenced history and philosophy of science (HPS) by providing a close examination of the Soviet philosopher and historian Bonifatii Mikhailovich Kedrov (1903-1985). Kedrov, who had an interest especially in philosophy of chemistry, was born to a Bolshevik family and became a controversial and established academic figure in the Soviet Union. In the late 1940s, he founded the journal Voprosy Filosofii (Questions of Philosophy) and eventually became a Member of the Soviet Academy of Sciences and briefly the director of the Institute of Philosophy of the Academy of Science.

I'll illustrate how political circumstances of 1940s-1950s influenced Kedrov's work on Russian chemist Dmitrii Ivanovich Mendeleev. After the Second World War, studies on the scientific legacy of Mendeleev were commissioned on patriotic and nationalistic grounds (Antonova 2011). Such motivations for an interest in Mendeleev had an impact on Kedrov's approach, as they both closed and opened avenues of research. Initially, the publication of Kedrov's book World Science and Mendeleev (1949) was blocked on the grounds of its cosmopolitan focus on international collaborations between chemists and physicists in Russia, Great Britain, and the USA. For this reason, it is striking that Kedrov's next major publication on Mendeleev had minimal narrative, as it was a collection of edited volumes of Mendeleev's early papers on the periodic system (Kedrov 1958, 1960).

After a close study of both works, I explore the question of whether Kedrov's edited volumes of Mendeleev's writings are one example of the broader trend of how Soviet historians averted conforming to the ideological expectations by focussing their efforts on publishing collections of primary source materials (Waldron 2020). More generally, the Soviet historians' increasing focus on creating collections of primary source material over detailed narratives put pressure on the "narrativist" turn on philosophy of history and suggest that greater emphasis should be put on the broader context in which historians operate.

---

## Archival Practices of the Early Royal Society

*Kirsten Walsh*
*University of Exeter, United Kingdom*
K.Walsh2@exeter.ac.uk

From its very inception, the Royal Society of London exhibited a profound awareness of its own history. The first official history of the Society, written by Thomas Sprat, was published in 1667, just five years after Charles II had bestowed upon it his Royal Charter. Restoration England was conservative, and anyone pursuing new forms of knowledge was vulnerable to the stigma of liberalism, materialism, and even atheism. By giving the experimental philosophy a respectable history and defending it against such criticism, Sprat's History of the Royal Society attempted to manage public perception of the Society by taking control of the historical narrative.

Early members of the Society wished also to ensure its legacy. To this end, plans for a Society archive were underway as early as 1659. The Royal Society was founded on the vision of Francis Bacon, who proposed a new philosophy based on observation and experiment on a grand scale. Record-keeping would pay a central role in this venture. The Society certainly didn't invent the concept of an archive, but they were probably the first to implement it on such a massive scale. Over the first four or five decades of its existence, the Society encountered various challenges vis-à-vis the quantity, reliability and accessibility of records. These challenges, I argue, shaped the archive and, in turn, the historical narrative of the Society.

I focus on two aspects of this case. First, I explore how the early archiving practices of the Royal Society both shaped and were shaped by its ideology. The archive was intended to serve as a 'foundation for philosophy', but also to keep track of what had already been done and to help figure out what was left to be done. To this end, in addition to recording Society activities, they needed to generate a record of relevant past studies and build a library of natural history and philosophy. The enormity of this undertaking meant that, in these early days, standards were continually re-negotiated, and strategies revised.

Second, I explore how the archive was used in the early decades of its existence. In addition to storing records of minutes and registers relating to meetings of the Society, the archive was a repository for new scientific ideas and discoveries, as well as a resource for its members. However, the extent to which individuals were willing to entrust their work to the Society archives depended on the reliability and trustworthiness of the secretaries responsible for creating and maintaining records. Indeed, due to 'teething problems' in the early decades, many important Society documents ended up in private collections.

In short, the institutionalisation of the Royal Society was concomitant with its archive. Its ambitious plans, as well as its desire to maintain a monopoly on both the products and processes of its new philosophy, shaped the archiving practices and, in doing so gave the Society control of its own historical narrative.

## Opacity and Explainability in Data-Centric Science

**Organizers: Hajo Greif, Alessandro Facchini and Alberto Termine**

**Contributors: Emily Sullivan, Florian Boge, Hajo Greif, Alessandro Facchini and Alberto Termine**

Fuelled by progress in computer power and the large availability of data, models generated by machine learning (ML) algorithms are gaining wide currency in scientific research. In some fields, their performance revolutionises the traditional approach to scientific inquiry based on theories, models, and experiments and promotes a transition towards a data-centric science. Unfortunately, ML models suffer from the problem of being epistemically opaque, which roughly means that their format, structure, and complexity prevent human users from understanding their functioning and behaviour on various levels, and therefore from relying on them in critical situations. In the context of scientific applications, these opacity problems are likely to affect the possibility of understanding phenomena through the models that are being employed. Only in the last few years, however, philosophers and STS scholars have started to reflect on the consequences of the intensive use of opaque ML models in scientific research in particular. These consequences are practical, in terms of how research is conducted under the conditions of using opaque models, as well as epistemic, in terms of their implications for the fundamental purposes of scientific inquiry, such as explanation, prediction, understanding, and objectivity.

In our symposium, we will reconstruct the specific, jointly practical and epistemic, issues with using opaque ML models in science, and how they are distinct both from opacity problems in applied and engineering contexts and from the issues that more traditional computer modelling approaches in the sciences are confronted with. We will also explore whether and how the research programme of eXplainable Artificial Intelligence (XAI), in its quest for methods and tools of making ML models more humanly understandable, may contribute to resolving or otherwise countering ML-related opacity problems in scientific practice. Questions that we will address in our talks include: How are forms of epistemic opacity of the internal workings of ML models and of their ability to represent, explain and understand phenomena related? What are the specific needs and dispositions of the epistemic agents involved? How and to what extent may various types of XAI methods help scientists to counter either set of opacity problems? How and to what extent may ML methods and related XAI approaches be integrated with scientific practice? What implications does this have for the conceptions of scientific understanding in modern science? More generally, we are looking for a path to understanding the effects of data-intensive science and ML methods on science that navigates between epistemologically motivated defences of the traditional scientific method and the embrace of 'the end of theory' suggested by proponents of data-intensive approaches.

This symposium comprises four contributions, which to present and discuss in sufficient detail will require a 120 minute time slot. The first paper, "Representation, Understanding, and Machine Learning", explores the conditions under which ML models may represent phenomena and foster scientific understanding despite being epistemically opaque. The second paper is more critical of this possibility, outlining a specific and hitherto underappreciated way in which ML models are opaque in "Two Dimensions of Opacity and the Deep Learning Predicament". The third paper, "Analogue Models and Universal Machines", also distinguishes between various opacity problems, locating the most fundamental of them in the very method of computer modelling, as distinguished from the use of analogue models in science. The fourth paper in our symposium, "Beyond Hypotheses-Driven and Data-Driven Biology Through Explainable AI", returns to a more positive assessment of the scientific potential of ML methods, exploring the role that XAI approaches can play in establishing a hybrid approach between ML-based prediction and more traditional hypothesis-based inquiries.

---

## Representation, Understanding, and Machine Learning

*Emily Sullivan*
*Eindhoven University of Technology and Eindhoven Artificial Intelligence Systems Institute, Netherlands*
e.e.sullivan@tue.nl

One way machine learning (ML) modelling is different from more traditional modelling methods is that they are data-driven, instead of what Knüsel and Baumberger (2020) call process driven. Moreover, ML models suffer from a higher degree of model opacity compared to more traditional modelling methods. Despite these differences, modellers and philosophers (e.g. Sullivan 2020, Meskhidze 2021) have claimed that ML models can still provide understanding of phenomena. However, before the epistemic consequences of opacity become salient, there is an underexplored prior question of representation. If ML models do not represent their targets in any meaningful sense, how can ML models provide understanding?

The problem is that it does in fact seem as though ML models do not represent their targets in any meaningful sense. For example, the similarity view of representation seems to exclude the possibility that ML models can represent phenomena. ML models use methods of finding feature relationships that are highly divorced from their target systems, such as relying on decision-rules and loose correlations instead of causal relationships. Moreover, the data that models are trained on can be manipulated by modellers in a way that reduces similarity. For example, the well-known melanoma detection ML model (Esteva et al. 2017) augments the RGB spectrum of dermatologist images (Tamir and Shech 2022). Thus, if the similarity view is right, then even if model opacity qua opacity does not get in the way of understanding, ML models may still fail to enable understanding of phenomena because they fail to represent phenomena.

Contrary to the similarity view, I argue that ML models are in fact able to represent phenomena, under specific conditions. Drawing on the literature of how highly idealised models represent their targets, and the interpretative view of representation (Nguyen 2020), a strong case can be made that ML models can accurately represent their targets. Even though ML models seem to be the opposite of highly idealised simple models, there are a number of representational similarities between them. Thus, if we accept that highly idealised models can represent phenomena, then so can ML models.

---

### Two Dimensions of Opacity and the Deep Learning Predicament

*Florian Boge*
*University of Wuppertal, Germany*
`fjboge@gmail.com`

Deep neural networks (DNNs) have become increasingly successful in applications from biology to cosmology to social science. For instance, they allow the identification of rare astrophysical objects, or even the prediction of three-dimensional protein structures from amino acid sequence data; a problem that had been unsolved for decades. Trained DNNs thus correspond to models that allow predictions, ideally even of novel phenomena, while their 'training' corresponds to an automated updating of certain free parameters of the model. Based on these and a few further observations, however, I will argue for two core theses in my talk: (a) that these models are instrumental in a sense that makes them non-explanatory; and (b) that their automated generation is opaque in a unique way.

The instrumentality I have in mind is different from that of (most) traditional scientific models, where it means the employment of unrealistic assumptions. In contrast, DNNs are instrumental in that they, as models, do not have enough content themselves to deliver understanding-promoting explanations. Furthermore, the unique, additional opacity of DNNs concerns not their functioning per se, as has been the case with traditional notions of opacity employed in the debate on computer simulations. It rather concerns the opacity of what it is that the DNN 'learns' during training—information that is highly relevant for understanding.

As I shall further argue, this combination of instrumentality and opacity thus implies the possibility of an unprecedented gap between discovery and explanation. In particular, when unsupervised models (i.e., models that are trained without labelled data) are successfully used in exploratory scientific contexts, i.e., contexts that are not, or not primarily, guided by existing theories and may even require the definition of new concepts, scientists will likely face a whole new challenge in forming the concepts required for understanding the mechanisms and processes that underlie the discovered phenomena.

---

## Analogue Models and Universal Machines. Paradigms of Epistemic Transparency in Artificial Intelligence

*Hajo Greif*
*Warsaw University of Technology, Poland*
hans-joachim.greif@pw.edu.pl

This paper is an attempt to resolve some of the ambiguity in the notion of epistemic opacity in the context of AI-based scientific modelling. It takes issue with the dichotomy between the view that epistemic opacity is chiefly a matter of complex or otherwise intractable algorithms that can be resolved on the algorithmic level and the claim that opacity is an 'essential' characteristic of AI models. Against this putative dichotomy, I argue that epistemic transparency comes in degrees and differentially applies to various levels of a model. It is a function of the degrees of an epistemic agent's perceptual or conceptual grasp of a model on the one hand, and of the elements and relations embodied in that model on the other. This condition, which is discussed here as 'model intelligibility', in analogy to de Regt's (2017) criterion of intelligibility of theories, is not primarily affected by the complexity or tractability of algorithms. Instead, it is chiefly affected by an element of underdetermination that is proprietary to computational methods.

In order to elucidate this claim, I first contrast computer models and their claims to algorithm-based universality with cybernetics-style analogue models and their claims to structural isomorphism between elements of model and target system (Black 1962). While analogue models aim at perceptually or conceptually accessible modelling relations, computer models are underdetermined in that they may establish any kind of modelling relation that lies within the mathematically delimited domain of computable problems, given appropriate time and computing resources. Accordingly, computer models are prima facie free of the constraints that make analogue models meaningful in the first place. Computer models need not and do not rely on isomorphism relations between concrete elements and relations in model and target system, nor are they subject to the requirement that human observers may conceptually and perceptually grasp these relations.

Against the background of this analysis, I undertake a comparison between two contemporary AI approaches that, although related, distinctly align with the above modelling paradigms and represent distinct strategies towards model intelligibility: Deep Neural Networks (DNN) and Predictive Processing (PP). Despite building on a fundamentally similar set of basic connectionist principles, DNNs do while PP does not exploit the potential complexity of their neural network architecture. While PP models present a specific analogy between prediction error minimisation in computer engineering and strategies of neuronal information processing, DNNs are very generally geared towards effective problem-solving, without respect to the demands of empirically adequate modelling, mechanistic explanation or scientific understanding. Where PP models are supposed to work as intelligible models of a specific and concrete target system, DNNs remain at least prima facie indifferent towards the criterion of model intelligibility. Even a better understanding of their internal operations will not change this condition. I conclude that the respective degrees of epistemic transparency of DNNs and PP primarily depend on the underlying purposes of modelling, not on their computational properties.

---

## Beyond Hypotheses-Driven and Data-Driven Biology Through Explainable AI: a Proposal

*Alessandro Facchini*[a] *and Alberto Termine*[b]
[a]*Molle Institute for Artificial Intelligence (IDSIA) USI-SUPSI, Switzerland;*
[b]*University of Milan, Italy*
[a]`alessandro.facchini@idsia.ch`; [b]`alberto.termine.ph@gmail.com`

Contemporary biological sciences are characterised by a lively and wide debate between supporters of a hypothesis-driven methodology and supporters of a data-driven (or data-centric) one. The former represents the traditional approach to the study of biological phenomena that have characterised experimental biology in the last century after the rise of modern synthesis. Focusing on the search of the mechanisms that produce the occurrence of biological phenomena, this methodology is based on the formulation of theoretical hypotheses regarding the entities and activities that make up these mechanisms and their subsequent evaluation through the use of specific experimental manipulation techniques. The data-driven methodology, on the contrary, represents a novel approach that follows the widespread diffusion of big data and machine learning techniques. Instead of focusing on mechanisms, this approach relies on learning powerful predictive models of the phenomena of interest through statistical analysis, supported by the latest available machine learning algorithms, of large amounts of observational data.

Both approaches have their weaknesses and strengths. The hypothesis-driven methodology allows scientists to grasp the mechanisms beyond observable phenomena and thus explain why these occur and how to intervene to modify their occurrences. It nevertheless struggles with the study and the accurate prediction of highly complex phenomena (e.g., gene regulation in human cells or embryonic development) resulting from the interaction between many different stochastic mechanisms that include hundreds of entities and activities. The data-driven methodology, instead, can produce very accurate predictive models even for highly complex phenomena. Its drawback is that the produced models do not provide any information about the mechanisms responsible for the occurrence of the predicted phenomena. Their explanatory power is therefore practically nil. Furthermore, these models are notoriously opaque. It is in fact very difficult for scientists to grasp their inner workings and understand the reasons for their predictions.

In recent years, scientists have begun to ask for a hybrid approach combining both the predictive power of the data-driven approach and the explanatory power of the traditional one. In a nutshell, this hybrid approach should enable us to justify in mechanistic terms the predictions made by machine-learning models through the use of traditional experimental manipulation techniques. Unfortunately, struggling with the implementation of such an approach, a growing number of scientists are starting to believe that, when studying biological phenomena, we will always be forced to choose between explanation and predictive power. In our paper, we argue that the main reason for the difficulty in implementing a hybrid approach is due to the variety of epistemic opacities exhibited by machine-learning models. Starting from this observation, we then show that employing tools and methods developed under the programme of explainable AI to counter the effects of the use of opaque machine-learning models may be the key to combining the hypothesis-driven and the data-driven methodologies.

References

Black, M. (1962): Models and Metaphors. Ithaca: Cornell University Press.

de Regt H. W. (2017): Understanding Scientific Understanding. Oxford: Oxford University Press.

Esteva, A.; Kuprel, B.; Novoa, R. A.; Ko, J.; Swetter, S. M.; Blau, H. M.; Thrun, Seb. (2017): Dermatologist-level classification of skin cancer with deep neural networks. Nature 542 (7639), 115–118.

Knüsel, B., and Baumberger, C. (2020): Understanding climate phenomena with data-driven models. Studies in History and Philosophy of Science Part A, 84, 46-56.

Meskhidze, H. (2021). Can Machine Learning Provide Understanding? How Cosmologists Use Machine Learning to Understand Observations of the Universe. Erkenntnis, 1-15.

Nguyen, J. (2020). It's not a game: Accurate representation with toy models. The British Journal for the Philosophy of Science, 71(3), 1013-

1041.

Sullivan, E. (2020): Understanding from Machine Learning Models. The British Journal for the Philosophy of Science.

Tamir M., and Shech E., (2022) "Understanding from Deep Learning Models in Context" in Scientific Understanding and Representation, ed. Insa Lawer, Kareem Khalifa, and Elay Shech, Routledge.

## Philosophy of Economics Roundtable I: Values in Economic Policy-Making

**Organizers: Conrad Heilmann and Julian Reiss**

**Contributors: Antoinette Baujard, Alexander Linsbichler, Constanze Binder, Alex Voorhoeve, Guilhem Lecouteux, James Grayot**

The most fundamental questions of economics are often philosophical in nature, and philosophers have, since the very beginning of Western philosophy, asked many questions that current observers would identify as economic. As John Maynard Keynes reminded us, a good economist 'must be mathematician, historian, statesman, philosopher—in some degree. He must understand symbols and speak in words'. Neoclassical economics has often been accused of exaggerating the importance of numbers over words. But the truth is that the philosophy of economics is now a vibrant and intellectually stimulating field of research in which scholars from many disciplines, including neoclassical economics, partake. The goal of the session is to bring the most exciting and philosophically significant developments in this area to the session attendees' attention, and to explore the relevance of these ideas to their own work.

Part I of the double session on the philosophy of economics focuses on economic science in the context of policy making, that is, on questions concerning the values and value judgements underlying policy decisions, policy evaluation, and rationality in the context of policy. It addresses questions such as: How are — and should — value judgements be treated in welfare economics? How does the Austrian School of Economics deal with value judgements? Do markets promote or stifle freedom? How should economic policies be evaluated in situations of severe uncertainty? Do apparent violations of norms of rational choice justify paternalistic interventions? What are the implications of recent findings in neuroeconomics for our understanding of economic agency?

## Values in Welfare Economics

*Antoinette Baujard*
*Université Jean Monnet, France*
antoinette.baujard@univ-st-etienne.fr

This talk focuses on the inner rationale and consequences of four different archetypal positions regarding how ethical and political values are tackled in welfare economics. Welfare economics is standardly associated with the welfarist framework, for which social welfare is only based on individual utility. Beyond this, we distinguish (i) the value-neutrality claim, for which ethical values should be and are out of the scope of welfare economics; (ii) the value confinement ideal, for which ethical values are acceptable if they are minimal and consensual; (iii) the transparency requirement, for which any ethical values may be acceptable in the welfare economics framework if explicit and formalized; and (iv) the entanglement claim, which challenges the very possibility of demarcation between facts and values.

---

## Philosophy of Austrian Economics

*Alexander Linsbichler*
*University of Vienna, Austria*
alexander.linsbichler@univie.ac.at

Carl Menger's Principles of Economics published in 1871 is usually regarded as the founding document of the Austrian School of economics. Many of the School's prominent representatives, including Friedrich Wieser, Eugen Böhm-Bawerk, Ludwig Mises, Hans Mayer, Friedrich August Hayek, Fritz Machlup, Oskar Morgenstern, and Gottfried Haberler, as well as Israel Kirzner, Ludwig Lachmann, Murray Rothbard, and Don Lavoie, advanced and modified Menger's research program in sometimes conflicting ways. Yet, some characteristics of the Austrian School remain (nearly) consensual from its foundation through to contemporary neo-Austrian economists. In eight sections, we will briefly discuss some of the philosophical and methodological characteristics of Austrian economics: Austrian action theory and interpretative understanding, a relatively thoroughgoing subjectivism, methodological individualism, ontological individualism, apriorism, essentialism, an often overstated rejection of formal methods, and alertness to economic semantics.

---

## Freedom and Markets

*Constanze Binder*
*Erasmus University Rotterdam, Netherlands*
binder@esphil.eur.nl

Markets have been prominently defended and criticized in the name of freedom throughout history. Existing contributions usually focus on different conceptions of freedom in order to defend or criticize the market in terms of the particular understanding of freedom adopted. Libertarians like Nozick, for instance, usually employ a notion of freedom as noninterference in one's justly acquired property rights and then defend the market as the most freedom-promoting societal system. Friedman adopts a notion of freedom as choice and defends markets for their positive effects on it. Socialists such as Cohen, on the other hand, criticize markets for the unfreedom they create for the proletariat as a collective to move up the social hierarchy. Yet others argue for specific limitations of markets on the basis of a republican notion of freedom or to safeguard the socioeconomic preconditions for freedom and autonomy. The drawback of this practice of starting out from one specific conception of freedom is that one's assessment of markets will be limited: it risks inhibiting one's understanding of how markets can possibly both promote and limit the freedom of different actors in different circumstances. However, precisely such nuanced insights are necessary for both policy assessment and a constructive debate about the merits and possible limitations of markets. The objective of this chapter is to shed a more nuanced light on markets and their effects on freedom. We do so by first discussing some of the most prominent arguments for and against markets in the name of freedom. In a second step, we shall then discuss the different conceptions of freedom employed and their different roles in market assessments by drawing on a more general concept of freedom pioneered by MacCallum. The chapter concludes by discussing insights such that a more general concept of freedom can provide for the use of freedom in contemporary welfare economics, as well as its role in the assessment of policies of redistribution.

## Policy Evaluation Under Severe Uncertainty: A Cautious, Egalitarian Approach

*Alex Voorhoeve*
*London School of Economics and Political Science, United Kingdom*
a.e.voorhoeve@lse.ac.uk

In some severely uncertain situations, as exemplified by climate change and novel pandemics, policymakers lack a reasoned basis for assigning probabilities to the possible outcomes of the policies they must choose between. I outline and defend an uncertainty-averse, egalitarian approach to policy evaluation in these contexts. The upshot is a theory of distributive justice that offers especially strong reasons to guard against individual and collective misfortune.

---

## Behavioral Welfare Economics and Consumer Sovereignty

*Guilhem Lecouteux*
*Université Côte d'Azur, France*
guilhem.lecouteux@univ-cotedazur.fr

The aim of this chapter is to critically assess the argument advanced in behavioral welfare economics (BWE) that preference inconsistency and violations of rational choice theory are the result of errors and to offer a direct justification for paternalistic regulations. I argue that (i) this position relies on a psychologically and philosophically problematic account of agency, (ii) the normative argument in favor of coherence is considerably weaker than what is usually considered, and (iii) BWE fails to justify why agents ought to be coherent by neoclassical standards. I conclude by discussing how BWE could still justify paternalistic regulations by endorsing a more institutionalist perspective.

---

**Economic Agency and the Subpersonal Turn in Economics**

*James Grayot*
*University of Groningen, Netherlands*
james.grayot@gmail.com

A recurring theme in the history of economic thought is the idea that individuals are sometimes better viewed as collections of subpersonal agents, each with its own interests or goals. The modeling of persons as collections of agents has proved to be a useful heuristic for investigating aberrant choice-behaviors, such as weakness of will, procrastination, addiction, and other decision anomalies that indicate internal or motivational conflict. Yet, the concepts and methods used to study subpersonal agents give rise to a frenzied and sometimes confusing picture about who or what economic agents are, if not individual persons. In an attempt to clarify this picture, this chapter investigates how the concept of the economic agent has changed following the subpersonal turn in behavioral economics and neuroeconomics.

---

## Philosophy of Economics Roundtable II: Values in Economic Policy-Making

### Organizers: Conrad Heilmann and Julian Reiss

### Contributors: Philippe Verreault-Julien, Aki Lehtinen, Aris Spanos, Robert Northcott, Melissa Vergara Fernández

The second roundtable is concerned with methodological and epistemological questions raised by economic science. The debate in economic methodology has shifted markedly over the past 30 or so years. In early 1990s, it was, to a large extent, engaged in discussions about the appropriateness of this or that "-ism" to characterise economics: realism, instrumentalism, positivism, operationalism, falsificationism, rhetoric (not an -ism but nevertheless a far-reaching methodological point of view), or pragmatism. Deirdre McCloskey once coined the apt term "big-M methodology" for work of this kind. As the label suggests, big-M methodology is concerned with the big "philosophical" questions about the nature and aims of economics as a science. Today, philosophers of economics are more concerned with what McCloskey calls "small-m methodology": methodological problems posed by day-to-day economic practice. All contributions to this roundtable exemplify this kind of practice-based methodological inquiry.

Sample questions include: How can we make causal inferences more reliable? Which of a number of concepts of causality is most appropriate for economics? What precise role do theoretical models play in economic reasoning? Are computer simulations a viable alternative to theoretical models on the one hand and laboratory experiments on the other? Can false models explain? Is p-hacking a problem and, if so, to what extent? What is the best strategy for econometric testing?

---

### Explanation in Economics

*Philippe Verreault-Julien*
*Eindhoven University of Technology, Netherlands*
p.verreault-julien@tue.nl

Discussions in the literature on economic methodology often do not explicitly concern explanation. The goal of this chapter is to show that, often implicitly, some key discussions are best understood as reflecting debates about explanation in economics. Disputes about, for instance, causal inference, idealizations, or microfoundations are debates about whether and how economics (should) explain. Examination of these issues through the prism of explanation sheds light on what is actually at stake and may help us progress on the route to solving them.

---

### Computer Simulations in Economics

*Aki Lehtinen*
*Nankai University, China*
aki.lehtinen@helsinki.fi

Although economics is becoming increasing computational, economists are still ill at ease with simulation methods. We review central questions in the philosophy of simulation: what distinguishes simulations from other models and experiments? In what sense can simulations produce novel results? What implications does the epistemic opacity of simulations have for their explanatory value? We then explore these questions in the context of the Monte Carlo methods in econometrics, dynamic stochastic general equilibrium models, and the emerging field of agent-based macroeconomics, and we argue that the methodological peculiarities present in all of these cases provide us with interesting lessons about the way in which the role of theory is conceived in economics.

---

## Philosophy of Econometrics

*Aris Spanos*
*Virginia Technological University, United States*
aris@vt.edu

The preceding quotation from Einstein's reply to Robert Thornton, a young philosopher of science who began teaching physics at the university level in 1944, encapsulates succinctly the importance of examining the methodology, history, and philosophical foundations of different scientific fields to avoid missing the forest for the trees. The field of interest in the discussion that follows is modern econometrics, whose roots can be traced back to the early 20th century. The problem of induction, in the sense of justifying an inference from particular instances to realizations yet to be observed, has been bedeviling the philosophy of science since Hume's discourse on the problem. Modern statistical inference, as a form of induction, is based on data that exhibit inherent chance regularity patterns. Model-based statistical induction differs from other forms of induction, such as induction by enumeration, in three crucial respects.

---

## Economic Theory and Empirical Science

*Robert Northcott*
*Birkbeck College, United Kingdom*
r.northcott@bbk.ac.uk

Economics over-invests in orthodox rational choice models. The problem with these models are not that they are idealized but that they lack empirical success, and when empirical success is achieved their contribution to it is typically only heuristic. As a result, many of the alleged benefits of orthodox models do not hold up: they do not explain, they do not provide understanding in terms of agent rational choices, and they do not generalize across cases. Their presumed advantage over heterodox models and methods melts away. A pressing issue then becomes the discipline-wide balance of work across different methods. The recent empirical turn in economics is an example of re-balancing, and is to be welcomed.

---

**Finance and Financial Economics: A Philosophy of Science Perspective**

*Melissa Vergara Fernández*
*Erasmus University Rotterdam, Netherlands*
vergarafernandez@esphil.eur.nl

In this talk, we introduce topics in finance and financial economics that should be of interest to philosophers of science and philosophers of economics, in particular. The chapter is divided in two parts. In the first part, we briefly discuss key elements of modern finance: the joint hypothesis problem as a problem of underdetermination and event studies as a method to cope with it; the relation between science and ideology; the performativity of financial models; and the role of models in finance as benchmarks or normative guidelines. In the second part, we delve deeper into the practice-oriented role of models. We focus on the influential model by Franco Modigliani and Merton Miller on the cost of capital to suggest that values held by modelers can often be the driving force of their model building and of the models' potential relevance. Thus, values, we suggest, should be part of the philosophical appraisal of models, as opposed to the much narrower attention to their explanatoriness.

---

---

## Racial Biases in Police Science

**Organizers & contributors: Saana Jukola, Manuela Fernández Pinto, Anna Leuschner and Abigail Nieves Delgado**

Recent years have seen an increase in the discussions concerning racial biases in police and judicial systems. The deaths of George Floyd in the US, Oury Jalloh in Germany and numerous others speak of a world-wide discrimination problem fed mainly by racial biases. These cases in combination with numerous studies reporting racist attitudes among police officers (e.g., Abdul-Rahman et al. 2020; Fryer 2019) have started an intense debate about discrimination in the context of justice systems. This symposium contributes to such discussions by providing a philosophical analysis of racial biases in police science. The aim is to analyse the nature of these biases in research that is used for informing law enforcement practices and examine how they influence policing and judicial systems.

The talks in the symposium address three case studies: research related to facial recognition technologies, research on shooting bias, and forensic research on the so-called Excited Delirium Syndrome. By analyzing these cases, the symposium provides answers to the following questions: What are 'biases' in police research, i.e., how should 'bias' be conceptualized in this context? How can we better understand what biases do within these fields and to what degree they influence law enforcement practices? For example, how do biases in police research influence who is incarcerated or not? How is such research methodologically problematic or affected due to particular interests, and under what circumstances does it become not only socially, but also epistemically problematic?

This symposium addresses questions of high social relevance by exploring a field of research so far understudied by philosophers of science. The field of police research provides results that are used for guiding practices in police agencies, for example in issues related to officer training and the use of body cams (Brown 2020). Different subdivisions of forensics, in turn, focus on evidence that can be used for investigation of crimes (Timmermanns 2006). Consequently, philosophical analyses of research in these fields have the potential to shed light on some mechanisms through which problematic practices in law enforcement emerge and are justified. Moreover, the discussion of the three case studies contributes to more general theorizing about biases in science.

References

Abdul-Rahman, L., Espín Grau, H., Klaus, L., & Singelnstein, T. (2020). Rassismus und Diskriminierungserfahrungen im Kontext polizeilicher Gewaltausübung: Zweiter Zwischenbericht zum Forschungsprojekt" Körperverletzung im Amt durch Polizeibeamt* innen"(KviAPol).

Brown, M. (2020, June 20). Police Research — An Important Tool for Police, Often Underutilized. `https://nij.ojp.gov/`

Fryer, R. G. Jr. (2019). An Empirical Analysis of Racial Differences in Police Use of Force. Journal of Political Economy 127 (3), 1210–1261.

Timmermans, S. (2006). Postmortem. How Medical Examiners Explain Suspicious Deaths. Chicago: University of Chicago Press.

**Historical biases in facial recognition technologies**

*Abigail Nieves Delgado*
*Utrecht University, Netherlands*
a.nievesdelgado@uu.nl

It is known that facial recognition technologies are faulty systems that can reproduce and augment human biases contributing to societal discrimination (e.g., Najibi 2020). Racial biases in these technologies are usually explained as the result of a lack of phenotypical diversity in training databases. As a consequence, to solve racial biases it is often recommended to increase phenotypic diversity in those databases. Such an approach has been supported by prominent voices in the field who have shown that with increased diversity racial biases can be solved (Buolamwini & Gebru 2018).

Despite this apparent success, I argue that this approach has two big problems that are left unattended and that are impossible to solve unless we offer a different conceptualization of what biases really are. The first problem is that understanding biases as a matter of phenotypic diversity further naturalizes race. This understanding motivates researchers to theorize phenotypical differences in terms of racial categories without critically evaluating what is been considered as "phenotypical diversity" and how this diversity is constructed. Second, it promotes conceptualizing biases as something that can be solved by constructing bigger, more diverse databases. This can motivate illegal or at least unethical practices such as internet scraping (e.g., Matsakis 2020) as it implies that we can have unbiased algorithms once we get enough data.

In contrast to that, in this paper, I argue that racial biases should be understood as "historical biases". Historical biases result from past decisions about what needs to be fine-tuned (and what does not) in an algorithm (Koene 2017). In short, what is considered relevant is fine-tuned, and what is irrelevant is not. These later unattended issues can become historical biases over time. To exemplify the concept of historical biases in the context of facial recognition, I focus on the competition FERET (1993-1996) organized by the Department of Defense of the United States to promote the standardization and improvement of facial recognition. FERET is unique because it initiated a series of challenges that until now (currently hosted by the National Institute of Standards and Technology) defines the state of the art in the field. Within FERET I analyze two technologies in detail: (1) SCFacerec developed by the team at University of South California and (2) Eigenfaces developed by the MIT. Through these cases, I exemplify how historical biases enter and remain in these algorithms, with important consequences for today's facial recognition technologies. This approach helps us to overcome the dichotomy neutral vs biased by acknowledging that algorithms are always biased as a result of their history.

References

Boulamwinim, J. & Gebru T. (2018) Gender Shades: Intersectional Accuracy Disparities in Commercial Gender Classification. Proceedings of Machine Learning Research 81:1-15.

Koene, A. (2017) Algorithmic Bias. Addressing Growing Concerns. IEEE Technology and Society Magazine.

Matsakis, L. (2020) Scraping the Web is a Powerful Tool. Clearview AI Abused It. Wired.
https://www.wired.com/story/clearview-ai-scraping-web/

Najibi, A. (2020) Racial Discrimination in Face Recognition Technology. Blog Science Policy, Special Edition: Science Policy and Social Justice.

---

## Research on shooting bias: social and epistemic problems

*Manuela Fernández Pinto*[a] *and Anna Leuschner*[b]
[a]*Universidad de los Andes, Colombia;* [b]*University of Wuppertal, Germany*
[a]m.fernandezp@uniandes.edu.co; [b]leuschner@uni-wuppertal.de

As many philosophers of science have argued, diversity within scientific communities is epistemically valuable as it leads to a broader range of criticism. However, there are limitations to this epistemic benefit. In particular, the denial of well-established scientific findings, such as anthropogenic climate change or gender bias, can come with social and epistemic costs when it meets epistemic and political asymmetries in society and contributes to a social atmosphere that is hostile to science (Biddle & Leuschner 2015; Leuschner & Fernández Pinto 2021). Thus, it seems justified in some cases to exclude certain voices from the exchange of opinions.

In this paper, we'll explore this problem further in light of a new case study: research on shooting bias. The shooting bias hypothesis aims to explain the disproportionate number of minorities killed by police. We'll first present the mounting evidence that supports the existence of a shooting bias among police officers, especially but not exclusively in the US.

Then we'll focus on two studies by James et al. (2016) and Fryer (2016) who have claimed that—although they corroborate widespread racism in non-lethal police use of force—they cannot confirm a shooting bias. While we grant the authors good intentions, we consider the studies highly problematic: The authors have made questionable generalizations and presented the studies in a way that made it easy for right-wing groups and media to misuse them. Not surprisingly, the studies have been embraced and disseminated by powerful right-wing media outlets, such as Breitbart and Fox News, and white-supremacist websites, such as stormfront.org. Consequently, the studies have been both epistemically and socially detrimental as they have contributed to a social atmosphere in which anti-racist campaigns and scientists working on relevant topics have been attacked.

However, in contrast to studies that bluntly deny well-confirmed scientific findings, such as the existence of anthropogenic climate change or gender bias, the situation seems more complex here. We'll argue that the shooting bias studies could have been socially and epistemically useful if the findings concerning the existence of shooting bias were more carefully interpreted and communicated. We'll undergird our argument by drawing upon Kitcher's "Millian Argument against the Freedom of Research2 (Kitcher 2001, ch. 8) as well as recent debates on epistemically detrimental dissent.

References

Biddle, Justin and Anna Leuschner (2015). Climate Skepticism and the Manufacture of Doubt: Can Dissent in Science Be Epistemically Detrimental? European Journal for Philosophy of Science 5 (3), 261–278.

Fryer, R. (2016). An Empirical Analysis of Racial Differences in Police Use of Force. Working Paper 22399. National Bureau of Economic Research.

James, L., Stephen M. J. & B. J. Vila (2016). The Reverse Racism Effect: Are Cops More Hesitant to Shoot Black than White Suspects? Criminology and Public Policy 15 (2), 457–479.

Kitcher, P. (2001). Science, Truth, and Democracy. Oxford University Press.

Leuschner, A & Fernández Pinto, M (2021). How Dissent on Gender Bias in Academia Affects Science and Society. Philosophy of Science, online first.

## Bodies of evidence – Excited Delirium Syndrome and biases in forensic medicine

*Saana Jukola*
*Ruhr-University Bochum, Germany*
saanajukola@gmail.com

Excited Delirium Syndrome (ExDS) is a controversial diagnosis described as a potentially fatal condition characterised by a number of features, such as aggression, "superhuman" strength, and disregarding the commands from police officers (Strömmer et al. 2020). The typical presentation of the condition is in a black male in his thirties, who is obese, and who has a history of drug use or psychiatric illness (Gonin et al. 2017). What makes ExDS particularly contentious is that it is used as a diagnosis in cases where an individual dies while being restrained by police (Strömmer et al. 2020). Many claim that the deaths assigned to ExDS can be explained by police use of force. This talk analyses how ExDS was established as a diagnosis and how it is used as a cause of death today. I argue that in these processes, several questionable assumptions have influenced the interpretation of available evidence.

I begin the talk by outlining how underdetermination understood as an epistemological problem concerning the relation between data and hypotheses (Longino 1990) is salient in the practice of forensic medicine. Determining the cause of a death can become a controversial matter even in circumstances where no foul play is suspected (e.g., Amoretti & Lalumera 2021). Findings from an autopsy have to be interpreted in the light of pathophysiological knowledge, toxicology, applied physics etc. (Meilia et al. 2018). Contextual factors, such as witness and police reports influence the interpretation of the physical evidence. Determining the cause of death thus involves numerous judgments, which are partially influenced by the context where the pathologist operates.

In the second part of the talk, I focus on evidential reasoning in cases where ExDS is used as a cause of death. In particular, I examine the interpretation of evidence that has been used for arguing for the existence of the condition since the 1980s. By focusing on research that has tried to refute the so-called 'asphyxia hypothesis', I argue that the background assumptions enabling using ExDS as a cause of death on the basis of existing evidence are questionable. ExDS is a case of harmful medicalization.

References

Amoretti, M. C., & Lalumera, E. (2021). COVID-19 as the underlying cause of death: disentangling facts and values. History and philosophy of the life sciences, 43(1), 1-4.

Gonin, P. et al. (2018). Excited delirium: a systematic review. Academic Emergency Medicine, 25(5), 552-565.

Longino, H. (1990). Science as Social Knowledge. Princeton: Princeton University Press.

Meilia, P. D. et al. (2018). A review of the diversity in taxonomy, definitions, scope, and roles in forensic medicine: implications for evidence-based practice. Forensic Science, Medicine and Pathology, 14(4), 460-468.

Strömmer, E. M., et al. (2020). The role of restraint in fatal excited delirium: a research synthesis and pooled analysis. Forensic Science, Medicine and Pathology, 1-13.

---

## Science communication, public opinion, and the epistemology of ignorance

**Organizers & contributors: Quill Kukla, Corey Dethier, Ilvie Otto and Lukas Steinbrink**

The public presentation of scientific results usually requires scientists to substantially simplify complex information. When publicizing their research, therefore, scientists must make decisions about which elements to emphasize and which details can be passed over. This is, in effect, a decision about which instances of ignorance to correct, and while these decisions are sometimes quite simple—you should provide information on what happened to the treatment group as a whole, not what happened to each individual, for instance—they are also sometimes much harder.

In this talk, I focus on a particularly salient hard case, namely the presentation of probabilistic climate science results by the IPCC. The IPCC's current approach is best understood as prioritizing accurate presentation of existing uncertainties. In more detail, the IPCC presents probabilistic results in complex two-tiered language that communicates both how much support the available evidence offers to a given hypothesis and something like how trustworthy or reliable the available evidence is thought to be. And a number of philosophers have endorsed either this practice or similar approaches on the grounds that simpler alternatives would misrepresent the degree of certainty that is actually warranted by the available evidence. In particular, they've argued that simpler approaches cannot handle different sources or kinds of uncertainty and are therefore untenable.

There's a clear cost to this choice, however: the two-tiered system that the IPCC uses makes it harder—arguably substantially harder—for both the public and other scientists to understand the results. As just indicated, both scientists and others often face similar choices about whether and where to sacrifice accuracy in the name of an understandable presentation; it's not the case that scientists always ought to prioritize accuracy no matter what. As such, I argue that it is at least an open question whether the IPCC should prioritize the accurate presentation of uncertainty in these cases. Contrary to what's been argued by the prior literature, it may be permissible for the IPCC to allow for—and indeed, even encourage—certain types of ignorance about sources of uncertainty insofar as that ignorance serves the end of better overall knowledge of climate science.

While this particular example is interesting, it is also an instance of a larger question: what ignorance can we encourage in service of a given educational or communicative goal? I'll end the talk by arguing that this question is ubiquitous and unavoidable: as educators, we're basically always in situations where we need to encourage or at least permit some ignorance, because communicating all of what we know is infeasible. In my view, recognizing that these choices are inevitable is a crucial first step; we can't answer the specific question about the IPCC's mode of presenting probabilistic results without recognizing the inevitable tradeoffs involved.

---

## Making your audience ignorant: Simplification and accuracy in the presentation of scientific results

*Cory Dethier*

*Leibniz Universität Hannover, Germany*

corey.dethier@gmail.com

The public presentation of scientific results usually requires scientists to substantially simplify complex information. When publicizing their research, therefore, scientists must make decisions about which elements to emphasize and which details can be passed over. This is, in effect, a decision about which instances of ignorance to correct, and while these decisions are sometimes quite simple—you should provide information on what happened to the treatment group as a whole, not what happened to each individual, for instance—they are also sometimes much harder.

In this talk, I focus on a particularly salient hard case, namely the presentation of probabilistic climate science results by the IPCC. The IPCC's current approach is best understood as prioritizing accurate presentation of existing uncertainties. In more detail, the IPCC presents probabilistic results in complex two-tiered language that communicates both how much support the available evidence offers to a given hypothesis and something like how trustworthy or reliable the available evidence is thought to be. And a number of philosophers have endorsed either this practice or similar approaches on the grounds that simpler alternatives would misrepresent the degree of certainty that is actually warranted by the available evidence. In particular, they've argued that simpler approaches cannot handle different sources or kinds of uncertainty and are therefore untenable.

There's a clear cost to this choice, however: the two-tiered system that the IPCC uses makes it harder—arguably substantially harder—for both the public and other scientists to understand the results. As just indicated, both scientists and others often face similar choices about whether and where to sacrifice accuracy in the name of an understandable presentation; it's not the case that scientists always ought to prioritize accuracy no matter what. As such, I argue that it is at least an open question whether the IPCC should prioritize the accurate presentation of uncertainty in these cases. Contrary to what's been argued by the prior literature, it may be permissible for the IPCC to allow for—and indeed, even encourage—certain types of ignorance about sources of uncertainty insofar as that ignorance serves the end of better overall knowledge of climate science.

While this particular example is interesting, it is also an instance of a larger question: what ignorance can we encourage in service of a given educational or communicative goal? I'll end the talk by arguing that this question is ubiquitous and unavoidable: as educators, we're basically always in situations where we need to encourage or at least permit some ignorance, because communicating all of what we know is infeasible. In my view, recognizing that these choices are inevitable is a crucial first step; we can't answer the specific question about the IPCC's mode of presenting probabilistic results without recognizing the inevitable tradeoffs involved.

---

## The interaction of active and passive ignorance in the science communication on COVID-19

*Ilvie Otto*

*Leibniz Universität Hannover, Germany*

ilvie.otto@philos.uni-hannover.de

"Every 20 days, the numbers double. We are in exponential growth." This statement is all too familiar from media reports on the spread of the coronavirus. Interestingly, this statement has been made on the publications dealing with the virus. The COVID-19 pandemic is accompanied by a perhaps historically unparalleled expenditure of scientific resources and research effort in a very short period of time. The aim was (and still is due to new variants) to gain as much knowledge as quickly as possible about the threat situation and possible countermeasures. Leading scientists presented new findings to decision-makers and the public through a variety of media channels. Daily news quickly picked up and disseminated new information. Despite this information offensive, characterized by speed and some dimensions of transparency, the corona crisis demonstrated a phenomenon that is already familiar from debates on climate change or the danger of tobacco: doubts on scientific findings in parts of the public.

I argue that this is on the one hand due to the active manufacturing of doubt, especially by seeming intellectuals who acted as epistemic trespassers. On the other hand, there is perhaps an even more interesting active and passive consolidation of ignorance. This ignorance is a backfiring by-product of the efforts of scientists and scientific institutions, especially those responsible for the publication processes, to promote productivity and transparency.

In many features, the internal and external information sharing of science in the pandemic is consistent with the open science approach. Although its goal is to generate as much reliable, accessible knowledge as possible, the publication practices associated with this approach (preprints, expedited publication procedures) tend to lead to ignorance under time pressure. Thousands of articles on COVID-19 are submitted and published every month which makes keeping track of the flood of publications nearly impossible. The result is the publication of papers containing serious errors and false conclusions. The strategy of publishing (almost) every potential finding as a valuable contribution to understanding the pandemic overlooks the effect that errors can have. The rapid dissemination in the media continues to form the impression of dissent in the scientific conclusions and the subsequent retraction of these studies is water on the mills of those who say that current studies and scientific work on COVID-19 do not yet represent reliable knowledge.

The established knowledge with broad consensus in the scientific community at the beginning of the pandemic was certainly much lower than in often-cited examples of public ignorance on scientific knowledge, such as climate change. I argue that the way findings were reported contributed to the impression that there was not enough knowledge to adopt mitigation measures. A pressing question is how to break through a mutual reinforcement of active and passive contributions to ignorance, when scientists and their control and publication bodies are working under enormous time pressure and uncertainty.

**Varieties of ignorance and the problem of public trust in science**

*Quill Kukla*
*Leibniz Universität Hannover, Germany*
rkukla@gmail.com

There is no denying that science has a PR problem. Scientists often reach consensus on issues of pressing social importance, but science communication fails to convince large swaths of the public to believe and to act in light of the scientific consensus. Climate change and vaccine efficacy are the two most obvious and socially disastrous examples of this kind of gap between scientific messaging and public opinion. Epistemological explorations of this gap tend to focus on how we can better frame and communicate scientific results so as to convince people of their legitimacy. The model is that, roughly, scientists have good information, and they need to find a way of transferring that information successfully into laypeople's heads.

In this presentation I will argue that we will not solve the problem of lack of trust in science if we treat it as just an information transfer problem. I argue this via a detour through the growing literature on the epistemology of ignorance. The epistemology of ignorance is concerned with how ignorance is produced, maintained, and propagated by our epistemic practices. The overwhelming focus in the epistemology of ignorance literature has been on what we can call propositional ignorance, which is ignorance that a fact is true. Indeed, many accounts of ignorance in philosophy of science and epistemology restrict the notion to propositional ignorance by definition. But just as propositional knowledge is not the only kind of knowledge we can have, likewise propositional ignorance is not the only kind of ignorance we can have. Moreover, other kinds of ignorance are epistemologically and practically important. For example, in addition to propositional ignorance, there is practical ignorance, or the lack of a skill or inability to do something. I am ignorant of how to ride a unicycle, but my ignorance is not a matter of my not knowing true facts. There is also perceptual ignorance, or the inability to perceive or be sensitive to some type of input. My ineptness at listening to and parsing jazz is a kind of perceptual ignorance. We also have phenomenological ignorance, which is when we don't know what it's like to experience something. Frank Jackson's famous Mary the scientist, who doesn't know what colors are like, is phenomenologically but not factually ignorant.

Social epistemologists and philosophers of science need to pay more attention to the fact that scientific knowledge is not just propositional knowledge, but rather consists also of a body of skills, perceptual capacities, and experience. Many people are profoundly scientifically ignorant, but the problem is not just that they don't read or believe scientific facts. Rather, they have little or no meaningful practical, lived scientific education. Most people, even if they are well-read, are deeply ignorant of the practices of science. The lab is a completely foreign environment to them, and scientific concepts are abstract for them. But skill and experience are part of scientific knowledge, so even good science reporting is imparting only partial scientific knowledge. In light of this, it is not surprising, I argue, that people are unsure when to trust scientists or scientific claims, or how to act in light of scientific information. Practicing scientists trust their own results on the basis of robust multimodal knowledge, which cannot be passed on through testimony. We are trying to address ignorance by imparting only a specific sort of decontextualized knowledge, based on a philosophically narrow conception of what knowledge is. Perhaps, I suggest, we need more hands-on advanced science education and participation if we want more public trust in science.

---

### Political ignorance in public opinion research

*Lukas Steinbrink*
*Leibniz Universität Hannover, Germany*
lukas.steinbrink@philos.uni-hannover.de

Science does not only communicate its results to the general public (and potential policy-makers); sometimes this general public itself is becomes the object of scientific investigation. Notably, this is happening in public opinion research, because information about public opinion is thought to be valuable in the context of a liberal democracy. "Public opinion" functions as a descriptive and as a normative term in both philosophical and social scientific contexts. On the one hand it describes widely-held beliefs or attitudes at a given time on issues such as crime, social and environmental policies and business regulation. On the other hand there is long and honorable tradition in the theory of democracy that ascribes a special normative burden to the concept and its relatives. In a democracy political legitimacy depends on consensus and the democratic responsiveness to the "will of the people". However, a quite robust empirical result of decades of research in political science is that most citizens in Western democracies are ignorant about fundamental facts of the political system they live in. The basic skeptical argument goes like this: Demanding conceptions of democracy legitimacy and political participation seem to require the existence of an informed (and politically active) citizenship. But empirical research seems to suggest that such a citizenship does not exist. This is neither due to a lack of available information – information is now more available than ever before – nor does it change with rising general levels of education. Therefore, evidence of widespread political ignorance may be a potential threat to demanding conceptions of democratic participation. This then prompts the question: Is this kind of political ignorance a problem for the legitimizing function of public opinion in a democratic context?

In this contribution I want to focus on the different ways in which the phenomenon of public opinion is conceptualized in the disciplines that take interest in it. The focus will be on the different conceptions available in public opinion research its dominant methodological strain, i.e. survey methodology. I will argue that the most common understanding of political ignorance relies on an aggregation model of public opinion, for which survey methods are indeed the most suitable approach. Having said this, other conceptions are available, which, as I will argue, will give us a more exhaustive and nuanced picture of the various ways in which knowledge of particular facts and other kinds of knowledge interact to produce (or impede) democratic competence. The focus on individual ignorance of particular politically relevant facts is reductive. An examination of the different ways in which public opinion is conceptualized in public opinion research is able to point out other dimensions of democratic competence. We should, therefore, broaden our conception of political ignorance in order to achieve a more accurate assessment of the threat that it poses.

---

## Reproducibility in Computational Practice

**Organizer: Johannes Lenhard**

**Contributors: Frédéric Wieber and Alexandre Hocquet, Hans Hasse and Johannes Lenhard, Gabriele Gramelsberger and Andreas Kaminski**

Reproducibility underwent a sudden transition from something that is taken for granted into an urgent problem. Of course, making scientific results reproducible is no trivial undertaking; philosophical, historical, and sociological studies have shown how delicate it is to reproduce experiments and to decide whether a result has actually been produced. However, reproducibility counts as a fundamental characteristic of scientific knowledge. In the discourse of most of the practitioners, if it is not reproducible, it is not scientific. In this sense we mean that reproducibility is taken for granted, or rather: has been taken for granted. Yet, a number of critical inquiries, mainly into psychological experiments and medical research, has produced spectacular failures that caused a "reproducibility crisis".

This symposium does not focus on the usual suspects, but on those disciplines that supposedly are the least affected, namely those that work with formal, computational methods. If anything is reproducible, it is the run of program on a digital computer. This expectation does not survive the practice test. Very recently, several concerns have been raised that target the non-reproducibility of computational results. These concerns indicate a growing awareness among practitioners that computational science is not on the safe side regarding reproducibility. Any measures that solve the problem arguably must be based on a coherent and comprehensive diagnosis. Such diagnosis, however, is still missing. This symposium aims at contributing to a diagnosis of (non)reproducibility in computational practice.

We will discuss and compare three case studies that focus on different aspects of the reproducibility problem. The contribution by Wieber and Hocquet examines the entanglement of model building and the use of software, i.e., of simulation epistemology and coding practices. The field of molecular modeling, a simulation method with wide uptake in physics, chemistry, and pharmaceutical research, highlights this entanglement. Wieber and Hocquet use practitioners' debates for analyzing pertinent controversies around reproducibility. Lenhard and Hasse stay with the field of molecular modeling, but approach the problem of reproducibility from a different angle. They discuss an experiment conducted in process engineering that documents to what extent different groups could reproduce the results of an (allegedly) identical model. They identify factors inherent in simulation modeling that contribute to the problem. The third contribution, by Gramelsberger and Kaminski, tackles the field of climate modeling that combines experimental and theoretical methods with an arsenal of computational techniques. They discuss the reasons for why a rerun of the same model with the same starting conditions and parameters can result in different outcomes and thus the need for finding a new framework for what counts as reproducibility.

By analyzing the differences and the similarities between the three case studies, the symposium will make inroads toward a diagnosis of the reproducibility problem in computational practice.

## Computational reproducibility and scientific software: beyond code transparency

*Frédéric Wieber*[a] *and Alexandre Hocquet*[b]
[a,b]*Université de Lorraine, France*
[a]frederic.wieber@univ-lorraine.fr;
[b]alexandre.hocquet@univ-lorraine.fr

Discussions about reproducibility most of the time put an emphasis on data-related scientific practices. Yet, computational reproducibility (i.e. issues of reproducibility stemming from the computer as a scientific tool) possesses its own dynamics and narratives of crisis. What is at stake is not only to be able to reproduce data, but also to focus on the disclosure and sustainability of computational methods used to produce data. Computational reproducibility is not that easy to achieve, indeed. Whether they are writing, using or reviewing scientific software, actors express their dismay about how to achieve reproducibility in practice.

Computational reproducibility is often expressed in terms of mere transparency of code. In our talk, we argue that the epistemic issues at stake in actual practices are best unveiled by focusing on software as a pivotal concept, one that is surprisingly often overlooked in accounts of reproducibility issues. Software is not only about designing and coding but also about maintaining, supporting, distributing, licensing, and governance; it is not only about developers but also about users. Such an emphasis on software allows us to depict a more complex epistemic landscape, beyond a mere requirement for code transparency. In addition to transparency, we offer a categorization of three more epistemic characteristics (namely consistency, sustainability and inclusivity) as key to the role of software in computational reproducibility. We also argue that the complexity of computational reproducibility is to be understood within the entanglements of the different layers of what we call a software millefeuille, beyond its reduction to "code". We thus aim to expand on the multifaceted nature of software to better apprehend the complexity of computational reproducibility issues, as a simplistic moral imperative for more transparency may be not enough.

To shed light on this multifaceted nature, we use the field of computational chemistry as a case study. This field is an interesting computational science, with peculiarities concerning software. Because of its particular historical context of development, because of its proximity to the pharmaceutical industry, computational chemistry has had to deal with academic norms as well as business norms. Software packages, in this context, are devised to produce novel scientific results. It is also important to acknowledge that they are distributed, maintained and licensed under these sometimes conflicting norms. Computational chemistry is thus an interesting field to engage in, so as to articulate multiple dimensions of software within the problem of computational reproducibility.

---

### Reproducibility and the Identity Crisis of Models

*Hans Hasse*[a] *and Johannes Lenhard*[b]
[a,b]*Technische Universität Kaiserslautern, Germany*
[a]hans.hasse@mv.uni-kl.de; [b]johannes.lenhard@mv.uni-kl.de

In many fields of science and engineering, simulation modeling starts from a theoretical mathematical model. The latter is then said to be evaluated by simulation experiments. These experiments "live" on simulation models. Many practitioners assume that these simulation models give an accurate picture of their theoretical starting point (in the limits of controlled approximation and statistics). We conceive this as a question directed at scientific practice. The guiding question of our contribution is to what extent simulation experiments provide reproducible results when the same theoretical models are simulated at different locations by different groups using different implementations on different computers. We tackle this question via a case study from thermodynamical engineering in which molecular simulations were used. The empirical material comes from a round robin study that found reproducibility problems in the setting of our guiding question above (Schappals et al. 2017). Our main claim is that the reported problems indicate an identity crisis of simulation models. In a nutshell, we argue that the transformation process from the well-defined mathematical model to the result of a simulation contains many steps. In these steps, technological, epistemological, and social aspects are intertwined. This leads to a merely vaguely defined and partially opaque simulation model. And two runs in such hazy circumstances will in general not produce the same results. Our analysis proceeds in three steps. Firstly, we briefly introduce Molecular Dynamics (MD), which is a simulation technique that investigates properties of materials by a two-step recipe. First model the interaction of particles via classical mechanics, then run simulations to extract properties of interest from these models. MD simulations numerically solve the Newtonian equations of motion simultaneously for all particles. The scope and precision of predictions made MD a popular tool in science and engineering. Secondly, we present a round robin study that assigned the task of simulating one and the same model to different expert groups, working at different locations and with their own implementations (Schappals et al. 2017). This study reports problems with reproducibility that were not anticipated by the practitioners and that pose a serious challenge. Thirdly, we analyze the factors that contribute to this problem. The main suspect is an over-simplified picture of the modeling process in between the mathematical and the simulation model. Only through analyzing all modeling steps that lead from the theoretical model to the concrete implementation, can one find out the reasons for the reproducibility limits as well as their delineation.

References

Schappals, M.; Mecklenfeld, A.; Kröger, L.; Botan, V.; Köster, A.; Stephan, S.; García, E.; Rutkai, G.; Raabe, G.; Klein, P.; Leonhard, K.; Glass, C.; Lenhard, J.; Vrabec, J.; Hasse, H. (2017). Round Robin Study: Molecular Simulation of Thermodynamic Properties from Models with Internal Degrees of Freedom. Journal of Chemical Theory and Computation 13, 4270-4280.

---

## Limits of Reproducibility in Climate Science

*Gabriele Gramelsberger*[a] *and Andreas Kaminski*[b]

[a]*Rheinisch-Westfälische Technische Hochschule Aachen, Germany;* [b]*University of Stuttgart, Germany*

[a]gramelsberger@humtec.rwth-aachen.de; [b]hpcakami@hlrs.de

Computing based on mathematical models seemed to be perfect role models for reproducible science. However, several workshops and publications in computational science have shown that this is not the case. The reasons used to explain why the computer-based sciences, of all disciplines, are confronted with reproducibility problems were seen primarily in their scientific culture: Documentation was insufficient; there was a lack of information on parameters, data, etc. Models, algorithms were not or only insufficiently published. Therefore, a change in the scientific culture is expected to ensure reproducibility to a large extent (LeVeque et al. 2012; Müller et al. 2014; Janssen 2017; Peng 2011).

The Association for Computing Machinery (ACM), for example, developed a classification to distinguish between the different ways in which a study or results can be duplicated (repeatability, reproducibility, replicability) and introduced an artifact and badging system. This should also serve precisely to change the culture of science in such a way that the conditions for reproducibility are met to a greater extent.

In the meantime, however, various studies have been undertaken which show that the problems are more profound (Donkin 2018; Mesnard & Barba

2017; Diethelm 2011; Nazaré et al. 2020; Merali 2010). Using the example of a climate simulation, we will show how reproducibility problems arise from computational techniques and the mathematical properties of the model. Thus, a change of culture will not overcome some limits of reproducibility in computer-intensive research. By presenting the case of climate science we will ask for an advanced framework for what counts as reproducibility in computational science using a mix of methods based on computational strategies of simplification, approximation, estimation, data fitting, etc. With this we want to broaden the view about the reasons for the occurrence of reproducibility problems.

References

ACM Badging v. 1.0. 2016. Artifact Review and Badging – Version 1.0 (not current).

Diethelm K. 2011. The Limits of Reproducibility in Numerical Simulation. Computing in Science Engineering 14, 64-72.

Donkin E., Dennis P., Ustalakov A. et al. 2017. Replicating complex agent-based models, a formidable task. Environmental Modelling & Software 92, 142-151.

Janssen M.A. 2017: The Practice of Archiving Model Code of Agent-Based Models. Journal of Artificial Societies and Social Simulation 20/1, 10.18564/jasss.3317.

LeVeque R.J., Mitchell I.M. & V. Stodden. 2012. Reproducible Research for Scientific Computing: Tools and Strategies for Changing the Culture. Computing in Science & Engineering 14/4, 13-17.

Merali Z. 2010. Computational science: ...Error. Nature 467, 775-777.

Mesnard O. & L.A. Barba. 2017. Reproducible and Replicable Computational Fluid Dynamics: It's Harder Than You Think. Computing in Science & Engineering 19/4, 44-55.

Müller, B.; Balbi, S., Buchmann, C.M. et al. 2014. Standardised and transparent model descriptions for agent-based models: Current status and prospects. Environmental Modelling & Software 55, 156-163.

Nazaré T., Nepomuceno E., Martins A. & D. Butusov. 2020. A Note on the Reproducibility of Chaos Simulation. Entropy. 22(9): 953.

Peng, R.D. 2011. Reproducible Research in Computational Science. Science 334, 1226-1227.

## Unpacking Openness: Sharing Practices in Contemporary Biology

**Organizer: Sabina Leonelli**

**Contributors: Paola Castaño, Rose Trappes, Sabina Leonelli, Rachel Ankeny**

In a scientific world ever more strongly focused on the significance of sharing outputs and methods, this session examines what this emphasis on Open Science means for research practices and knowledge production within the life sciences, and whether this calls for a different philosophical interpretation of the role of openness in research (see also `http://www.opensciencestudies.eu`). Biology offers a rich space to ask such a question, given its widely documented pluralism and the diversity of traditions and ethos underpinning contemporary work across its numerous subfields. Existing scholarship on openness in biology has emphasized the epistemic advantages of sharing "omics" data within molecular biology, and used that as a platform to argue for the usefulness of sharing practices in enabling exchange – and ultimately discovery – across different research communities (Ratti 2015, Stevens 2016, Strasser 2019). In contrast to this focus on genetic data sharing, we consider the areas of space biology, behavioural ecology and crop science, and evaluate what "sharing" has come to mean in these fields and with which implications for the knowledge therein created. Paola Castaño starts us off with a discussion of sharing in space biology, and the significance of governance structures and community ethos in shaping key initiatives such as GeneLab, which are then taken as exemplars for 'good scientific practice' around the world. Rose Trappes then considers whether and how sharing data about animal movement affects the characteristics of ecological models built on such data, and how this in turn speaks to openness policies and guidelines in this domain. Sabina Leonelli considers lessons learnt from sharing initiatives in crop science, where long-standing inequity among participants – and lack of inclusion of low-resourced researchers and relevant experts in data collection efforts - has damaging effects on how researchers re-use data to understand plant development and plan agricultural interventions. Finally, Rachel Ankeny builds on her expertise on the history of genetic data sharing to comment on these three different attempts at biological openness and the ways in which they challenge, each in their own way, existing understandings of biological "best practice".

## Data Processing as Sharing Practice: Making Space Plant Biology at NASA GeneLab

*Paola Castaño*
*University of Exeter, United Kingdom*
p.a.castano-rodriguez@exeter.ac.uk

This paper interrogates the extent to which data processing practices, including both methods and governance procedures used to prepare data for analysis, can and should be informed by the experimental conventions and goals underpinning particular forms of investigation. I show how data processing is unavoidably framed by a normative vision for what constitutes good scientific practice, the aims of sharing data, and what the wider societal goals of that practice should be. This is well exemplified by Open Science and Open Data stances which make such commitments explicit and connect them to the technical choices made by researchers handling data on an everyday basis. To illustrate this argument and analyse its epistemic consequences, I consider the domain of space plant biology, and particularly the emblematic GeneLab, a recently implemented open science platform from the Space Biology program at NASA. GeneLab is a database, specimen repository, and collaboration space that hosts omics data generated in investigations on the International Space Station, other spaceflight platforms, ground-based simulations, and centrifugation experiments (Stotzky et al. 2018). Conceived as part of NASA's Open Data Plan, GeneLab was designed in 2014 and began operations in 2018. Currently, it is part of the agency-level Transform to Open Science (TOPS) that from 2022 until 2027 aims to "accelerate the engagement of the scientific community in open science practices," "lower barriers to entry for historically excluded communities," and increase "opportunities for collaboration while promoting scientific innovation, transparency, and reproducibility" (NASA 2022). I focus on an early stop in the GeneLab data journey (Leonelli and Tempini 2020): the collective work of processing raw data. For this task, the platform established Analysis Working Groups (AWG) with voluntary participation from members of the research community to scrutinize subsets of data before they are added to the repository. One of their tasks is to develop canonical analysis pipelines to process the data and "speed the harmonization of results across space biology experiments" (GeneLab Analysis Working Groups Charter 2022). First, I show how, with the AWGs, GeneLab already makes explicit in its mode of operation the labour-intensive nature of data mobility and interoperability (Leonelli 2016, 2020). Second, I examine the variability in the ways in which the materiality and eventuality of the experimentation process (Rheinberger 2011) – which is of is great value for this community (Kiss 2015) – gets inscribed in the data. Given the fact that the AWGs are heterogeneous collectives that negotiate the terms for the data's future use, I make a broader argument about processing tasks as, in themselves, data sharing practices.

References

GeneLab Analysis Working Groups Charter. 2022. `https://genelab.nasa.gov/awg/charter`

Kiss, John. 2015. Conducting Plant Experiments in Space, in Plant Gravitropism. Methods and Protocols, edited by Elison Blancaflor, 255-283. New York, Springer, 2015.

Leonelli, Sabina. 2016. Data-centric Biology: A Philosophical Study. Chicago, University of Chicago Press.

Leonelli, Sabina. 2021. Learning from Data Journeys, in: Data Journeys in the Sciences, edited by Sabina Leonelli and Niccolò Tempini. Springer Open: 1-23

NASA, 2022. Transform to Open Science. `https://science.nasa.gov/open-science/transform-to-open-science`

Rheinberger, Hans-Jörg. 2011. Infra-Experimentality: From Traces to Data, From Data to Patterning Facts, in: History of Science 49 (164): 337–348.

Stotzky, et al. 2018. A Researcher's Guide to the International Space Station: GeneLab. NASA ISS Program Office.

---

## Sharing Movement Data to Answer Big Questions: Are there Trade-Offs in Opening Ecological Observation?

*Rose Trappes*

*University of Exeter, United Kingdom*

`r.g.trappes@exeter.ac.uk`

In ecology, general knowledge tends to come at the expense of developing detailed realistic accounts or quantitatively precise predictions. Much has been written about such trade-offs in ecological modelling (Odenbaugh 2003; Matthewson 2011; Elliott-Graves 2016) and experimentation (Inkpen 2016). In this talk I ask whether there is a similar trade-off for field observations, focusing on data sharing as a practice for developing general descriptive knowledge in ecology. Field observations are typically extremely local, but researchers do bring observations from multiple studies together to produce more general descriptions of ecological systems and processes. This is important for answering some of the bigger questions in ecology, and for generating transferrable knowledge for conservation. As a case study, I consider animal telemetry, the remote tracking of animal movement using GPS, radio, or similar technologies. Animal telemetry delivers huge amounts of data. Much of this data is published on Movebank (movebank.org), where researchers can find multiple datasets on a species, locality or phenomenon of interest. Movement data are extremely heterogenous (Kays et al. 2021). A standardisation procedure has therefore been proposed to facilitate data reuse (Sequeira et al. 2021). It involves, amongst other things, eliminating case-specific details and factors and using 2D geographic representation (not always the preferred format for studying movement). Standardisation would seem to facilitate generality only by sacrificing realism and precision. Yet it can also enable connecting different kinds of data. For instance, standardised movement data can be more easily linked with environmental, meteorological, fisheries, and transport data. This can contribute to a more detailed, rich description, thereby enhancing realism and precision. Exploring potential trade-offs in animal telemetry offers a novel case study of the use of open data to produce scientific knowledge, highlighting the complex negotiations of the variety and contextuality involved in data sharing and reuse. This complements other studies of "data journeys" in fields such as plant biology, biomedicine, and molecular biology (Leonelli 2016; Leonelli and Tempini 2020).

References

Elliott-Graves, Alkistis. 2016. "The Problem of Prediction in Invasion Biology." Biology & Philosophy 31 (3): 373–93.

Inkpen, S. Andrew. 2016. "Like Hercules and the Hydra: Trade-Offs and Strategies in Ecological Model-Building and Experimental Design." Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences 57 (June): 34–43.

Kays, Roland, Sarah C. Davidson, Matthias Berger, Gil Bohrer, Wolfgang Fiedler, Andrea Flack, Julian Hirt, et al. 2021. "The Movebank System for Studying Global Animal Movement and Demography." Methods in Ecology and Evolution, December, 2041-210X.13767.

Leonelli, Sabina. 2016. Data-Centric Biology: A Philosophical Study. Chicago: University of Chicago Press.

Leonelli, Sabina, and Niccolò Tempini, eds. 2020. Data Journeys in the Sciences. Cham: Springer International Publishing.

Matthewson, John. 2011. "Trade-Offs in Model-Building: A More Target-Oriented Approach." Studies in History and Philosophy of Science Part A 42 (2): 324–33.

Odenbaugh, Jay. 2003. "Complex Systems, Trade-Offs, and Theoretical Population Biology: Richard Levin's 'Strategy of Model Building in

Population Biology' Revisited." Philosophy of Science 70 (5): 1496–1507.

Sequeira, Ana M. M., Malcolm O'Toole, Theresa R. Keates, Laura H. McDonnell, Camrin D. Braun, Xavier Hoenner, Fabrice R. A. Jaine, et al. 2021. "A Standardisation Framework for Bio-logging Data to Advance Ecological Research and Conservation." Methods in Ecology and Evolution 12 (6): 996–1007.

---

## Openness as Disruption: Epistemic Injustice in Crop Data Linkage

*Sabina Leonelli*

*University of Exeter, United Kingdom*

s.leonelli@exeter.ac.uk

The Open Science movement aims to ensure that research outputs, research components and methods are widely disseminated, scrutinized and re-used for the good of science and of society. This movement has gathered tremendous momentum over last two decades as a response to the broad transformations in research brought about by the digitalization, globalization and increasing commodification of scientific communications and research processes. Particularly within the molecular life sciences, openness is often interpreted as making data available to all who may wish to access and re-use the data, thus speaking to long-held ideals around sharing genetic information (Leonelli and Ankeny 2012, Maxon-Jones 2018). This interpretation of openness as sharing, however, clashes with significant obstacles to the mobilization of data in areas such as crop science and clinical research, where there are significant concerns around the scientific and social implications of widespread sharing - including challenges around evaluating data quality, recontextualizing findings in ways that do not betray their provenance, and taking account of sensitivities around the commercialization and exploitation of data (Beaulieu and Leonelli 2021). In this paper I focus on questions of epistemic injustice arising from the implementation of Open Data within crop science, particularly the twin threats of bioprospecting (that is, the extraction of data from local communities to benefit large corporations based in the Global North) and knowledge erasure (that is, the systematic privileging of Western concepts and tools for crop data processing and interpretation over other knowledge systems). Through this example, and drawing inspiration from Karl Popper's Open Society (1945), I highlight the epistemic and social advantages of adopting a different conceptualization of openness as disruption, rather than sharing.

References

Beaulieu, A. and Leonelli, S. (2021) Data and Society: A Critical Introduction. London, UK: SAGE.

Maxson Jones, K., Ankeny, R.A. & Cook-Deegan, R. The Bermuda Triangle: The Pragmatics, Policies, and Principles for Data Sharing in the History of the Human Genome Project. J Hist Biol 51, 693–805 (2018)

Leonelli, S. and Ankeny, R.A. (2012) Re-Thinking Organisms: The Epistemic Impact of Databases on Model Organism Biology. Studies in the History and Philosophy of the Biological and Biomedical Sciences 43(1): 29-36.

Popper, K. (1945[2002]) The Open Society and Its Enemies. Routledge.

---

**Comments**

*Rachel Ankeny*

*University of Adelaide, Australia*

`rachel.ankeny@adelaide.edu.au`

---

## Alignment, Narrative, and Exploration: Strategies and consequences of integration in the life sciences

### Organizer: Robert Meunier

### Contributors: Pierre-Luc Germain and Fridolin Gross, Robert Meunier, Saliha Bayır

Integration is a central concern of contemporary sciences, as it counteracts the tendency of specialization that has characterized research since the mid-nineteenth century. As such, integration has also become an issue in philosophy of science. Earlier work on the topic focused on disciplinary relations in terms of reduction or non-reductive collaboration, mainly on the level of theories and explanations. Recent scholarship, inspired by the practice turn in philosophy of science, has drawn a more complex picture, where next to disciplinary specialties and their theories and explanations also methods, data, standards, and approaches can be subject to integration (see the special issue on "Integration in biology" in Studies in History and Philosophy of Biological and Biomedical Sciences 44, 2013, edited by Ingo Brigandt). This perspective emphasizes that while integration often involves interdisciplinary constellations, it can also occur within one field of research.

This symposium addresses and expands on these themes. Focusing on the life sciences, talks will discuss the integration of models, data, and techniques, as well as associated strategies, such as alignment and narrative. They will also examine underlying motivations, such as expanding exploratory reach and, finally, the consequences of integration for disciplinary dynamics. The presentation by Pierre-Luc Germain and Fridolin Gross focuses primarily on integration at the level of models, drawing on cases from bioinformatics and the use of animal models. By pointing out parallels between measurement, experiment, and modelling, they elucidate the alignment of spaces of representation as a strategy for integration. In contrast with this rather formal approach, Robert Meunier proposes to understand the use of narrative as a strategy for integration. Looking at precision medicine as an interdisciplinary research field which is closely intertwined with clinical practice, he shows how this strategy is used to make sense of heterogeneous data associated with diseases. Both contributions are thus concerned with recent science and highly interdisciplinary research contexts. Saliha Bayır, on the other hand, presents a historical case from the 1980s and shows how the integration of molecular techniques, which was motivated by the aim to expand the exploratory potential of environmental microbiology, facilitated the formation of molecular microbial ecology as a new specialty. Together, the talks highlight similarities and differences as well as interferences of integration at various levels (models, data, techniques).

With its broad spectrum of cases, clearly circumscribed thematic focus, and its novel emphasis on alignment, narrative, and exploration as important aspects of integration, this symposium is expected to advance the philosophical understanding of integration as a process and its role in shaping knowledge and disciplinary landscapes.

## Integration as alignment: from measurement to experimentation and models

*Pierre-Luc Germain*[a] *and Fridolin Gross*[b]
[a]*ETH Zürich and Universität Zürich, Switzerland;* [b]*CNRS and University of Bordeaux, France*
[a]pierre-luc.germain@hest.ethz.ch; [b]fridolin.gross@u-bordeaux.fr;

Commenting on Rheinberger's work, Richard Burian wrote that "[w]hat must be explored is how one might justify the claim that scientists working with different experimental systems, and thus different epistemic things, might nonetheless have good grounds to hold that they had gotten hold of different parts of aspects of the same elephant." (Burian 1995, p.130) This points to the central problem of integration in contemporary science, especially in the life sciences, where studies typically rely on a composite of diverse methods, experimental systems, and often disciplines. We think that at the root of this problem lies the following question: how, given that their operational meanings are restricted to their contexts of production, can different pieces of data be brought to bear on one another? While it has been suggested that, in many sciences, theoretical interpretation can achieve this goal, we show that biologists most often employ more pragmatic approaches. We argue that, across a variety of contexts (ranging from simple integration of measurement methods to the joint use of complementary experimental systems), many of the strategies through which scientists achieve integration can be characterized as practices of aligning different representational spaces. Representational spaces can be concrete and material, such as the varying height of a mercury column in a thermometer, but they may also be abstract, such as the quantity of a variable in a mathematical model. In the case of simple measurements (length, temperature) the representational space consists of only one dimension, but alignments can also be made between multidimensional spaces, which corresponds to more complex scenarios found in modeling or big data contexts. Alignment, which implies the transformation or projection of different spaces onto a common system, is especially achieved through practices of "anchoring", that is, the identification of sets of points that preserves some structural relationships.

Using selected examples from multi-omic bioinformatic analysis and experimental biology, we show how operations of alignment are carried out in practice. We next turn to modeling practices, which are often construed as entirely different from other scientific practices such as measurement or experimentation. Against this, we argue that they can be conceptually assimilated to each other and exemplify similar strategies of alignment and anchoring. For example, we argue that much work on behavioral mouse models, rather than instantiating surrogative reasoning, is best understood as the alignment between respective spaces of causes (e.g., drugs or mutations) and effects across systems. We thus propose a new perspective on both modeling and integration, which not only reveals important parallels between practices that have traditionally been characterized separately, but also brings to light the rich recourse that modelers make to both technical objects and epistemic things beyond the model-target dyad. It thus provides a natural starting point to capture more complex practices of integration of models and data of different kinds that constitute the complex epistemic landscape of the life sciences today.

References

Burian, R. M. (1995). Comments on Rheinberger's "From Experimental Systems to Cultures of Experimentation". In G. Wolters and J. G. Lennox (Eds.), Concepts, Theories, and Rationality in the Biological Sciences, pp. 123–136. University of Pittsburgh Press.

---

## The role of narrative in integrating heterogeneous data in precision medicine – notes from the field

*Robert Meunier*

*University of Lübeck, Germany*

robert.meunier@uni-kassel.de

Today most life science fields are characterized by the production of large data sets. Hence interdisciplinary knowledge production often concerns the integration of diverse types of data. Precision medicine is a case in point, as it puts the individual patient at the center and promises to determine the causes, expression, and possible treatments of a disease on various levels from the genome to the microbiome to lifestyle. This requires interdisciplinary interaction regarding both translational research and clinical work.

However, in practice, such an interdisciplinary agenda meets many challenges. On the one hand, researchers too often work in parallel on various biological aspects of a disease. What remains elusive is the physiological-environmental system as an epistemic object in itself. Doctors, on the other hand, are presented with an individual patient's profile as a mosaic of markers, while treatment affects the patient as a whole. Among the obstacles for a holistic, integrated view are the different epistemic cultures of the various biomedical and clinical fields and the respective different practices, languages, and epistemic and non-epistemic values. As precision medicine is a data-intensive field, these problems crystalize in the heterogeneity of the data produced and used in various research projects and by clinicians.

I work as embedded philosopher in the context of a large research consortium involving translational research as well as specialized centers for individual patient care concerned with chronic inflammatory diseases. Among the data produced are genomics, epigenetics, metagenomics (microbiome), and metabolomics data. I take a situated intervention approach to empirical and applied philosophy of science in practice. While participant observation provides the empirical basis for identifying epistemological problems, the aim is to suggest ways to improve interdisciplinary interaction and data integration in research and clinical practice.

Here I present work on the role of narrative in the integration of heterogeneous data in the above-mentioned precision medicine context. Philosophers of science have recently directed attention to the various roles of narrative in the sciences beyond historical narrative explanations (see the special issue on "Narrative in Science" in Studies in History and Philosophy of Science 62, 2017, edited by Mary S. Morgan and M. Norton Wise). The use of narrative is widespread and has various epistemic functions. It cannot be dismissed as defective or preliminary and there are many situations where it remains the appropriate way to proceed. In particular, narratives are the preferred epistemic strategy for bringing together heterogeneous elements that are connected through a heterogenous set of relations (causal, classificatory, value etc.). In this way, narratives can function as proto-models or proto-explanations, but this often amounts to a reduction of the information contained in narratives to a set of causal relations. However, such narratives can also function as representations in which holistically conceived entities appear as epistemic objects. In this function, they are not preliminary but have a constant and evolving role in integrating information and exploring possibilities. I will show how narratives are used to integrate heterogeneous data, guide data management and data-based interventions, and suggest links between research and clinical practice.

**Integration of Molecular Techniques in Environmental Microbiology as Exploratory Practice**

*Saliha Bayır*
*University of Kassel, Germany*
saliha.bayir@uni-kassel.de

Experimental practices were not an object of analysis for the classical history and philosophy of science. Following the practice turn, they received more attention. One thread of this literature is focused on the exploratory characteristic of experimentation. Friedrich Steinle emphasizes the search for regularities and characterization of phenomena resulting in a revision of existing concepts or development of new ones (2002). Richard Burian argues that experimental practices are not necessarily theory-driven and constitute a rich ground for philosophical inquiry besides their hypothesis-testing role (1997). Kevin Elliott (2007) characterizes four types of exploratory practice: i. seeking regularities or identification of specific phenomena, ii. development of new techniques and instruments, iii. collection of big data and its analysis through a new experimental procedure, iv. field formation around a specific set of exploratory practices.

Taking Elliott's broad definition of exploratory practice as a lead and focusing on types ii., iii., and iv., I will present the integration of a set of experimental techniques from one field into another research setting as a form of exploratory research. More specifically, I show that transfer of sequencing and data analysis methods from molecular biology into environmental microbiology research facilitated the formation of microbial ecology as an emerging field. Using the insight of Carl Woese that rRNA subunits can be employed for phylogenetic characterization, Norman Pace began to adjust this approach as a technique for studying the biodiversity of naturally occurring microbial communities. Isolating (16S) and (5S) rRNA subunits from their environmental samples, his group explored the composition of microbial communities (Pace, et al., 1986). The establishment of molecular techniques offered a way to study natural microbial communities independently of culturing methods. This early work on phylogenetic analyses constituted the beginning of the metagenomics approach in microbial ecology.

The case study demonstrates how integration of molecular lab techniques into the field practices and problem agendas of environmental microbiology takes the form of exploratory experimentation, aiming both at developing new approaches (ii, see above) and gathering new kinds of data (iii). Thereby, it facilitates the formation of microbial ecology as a new field (iv) characterized by metagenomic approaches and an exploratory agenda for studying diversity and ecological function in naturally occurring microbial communities. The talk will thus contribute to the understanding of integration at the level of research techniques and its consequences for the integration of research fields as well as to the growing literature on exploratory strategies in science.

References

Burian R. (1997) "Exploratory Experimentation and the Role of Histochemical Techniques in the Work of Jean Brachet, 1938-1952", History and Philosophy of the Life Sciences, 19: 27-45.

Elliott, K.C. (2007) "Varieties of Exploratory Experimentation in Nanotoxicology", History and Philosophy of the Life Sciences, Vol.29, No.3, pp. 313-336.

Pace, N.R., et al. (1986). The Analysis of Natural Microbial Populations by Ribosomal RNA Sequences. In K.C. Marshall (Ed.), Advances in Microbial Ecology (Vol.9, pp.1-55).

Steinle F., (2002) "Experiments in History and Philosophy of Science", Perspectives on Science, 10: 408-432.

## Where is technology in the philosophy of science in practice?

**Organizers: Federica Russo and Emma Tobin**

**Contributors: Federica Russo, Elena Falco, Jaspreet Jagdev, Emma Tobin, Christian Hennig, Ines Hipolito, Emanuele Ratti, Juan M. Durán**

The philosophy of science in practice (PSP) has helped philosophy of science to pay due attention to the practice of science, current and past. A lot has been done over the years to broaden and diversify the scope and object of classic philosophy of science (Phil Sci) questions. It is undeniable that PSP, as a community, has achieved a better science-informed philosophy of science. While PSP created a fertile ground for new Phil Sci questions to grow, one seed has not made it into a flourished plant: discussion about technology. Technology, however, is a topos in STS, social studies of science, and philosophy of technology, but these fields do not address phil sci-type of questions. For instance, there has been much discussion of highly technologized areas in science, such as data intensive approaches and, more recently, on AI. But a distinct philosophical reflection on technology in these areas as well as in other scientific fields has not followed.

In this panel-oriented symposium session, we aim to put technology under the spotlight of PSP. A diverse panel of scholars will engage in an active and lively discussion with the audience in order to motivate a dedicated discussion of technology in Phil Sci and in PSP; detail and exemplify the kind of questions that are worth and urgent to ask; present and showcase episodes, case studies, examples of techno-science that hold the potential to drive change into Phil Sci / PSP debates; and potentially expand SPSP to include technology: SPSTP.

The panel will make use of various 'unconferencing' techniques in order to set up an active and lively dialogue with the audience. It will be structured in three parts. The first part will consist of lightning talks from the panellists about the status of technology in PSP and Phil Sci. The lightning talks are followed by fishbowl discussion to get the audience actively engaged with the topic of the session. The second part will consist of lightning talks presenting interesting cases in the practice of the sciences that require proper thinking of technology. This second round of lightning talks is followed by a Q&A session with the audience. In the third part, we will discuss whether technology needs to be more visible in SPSP and in other institutional forms where Phil Sci is present, engaging in a 'dot-voting' activity.

---

**The gap between Phil Sci and Phil Tech, and the role of PSP**

*Federica Russo*
*University of Amsterdam, Netherlands*
federica.russo@gmail.com

Russo will introduce the discussion by briefly reconstructing the divide between Philosophy of Science and Philosophy of Technology. She will explain how Philosophy of Science in Practice has contributed to bridge this gap, but also motivate the need for further and more dedicated discussion of technology in the practice of the sciences. By giving due importance to technologies in the practice of the sciences, we can provide the kind of deep and science-informed philosophical discussion that we are missing at the moment. This means, in many cases, to radically re-think our classic Phil-Sci questions (e.g., what is knowledge? What is causality? What are entities? ...). At the same time, due attention to technology in the practice of the sciences also enriches Phil-Tech questions, by considering technologies and experimental equipment beyond their mere materiality and for their fundamental role in the production of knowledge in scientific contexts.

---

### How should we think about instruments?

*Elena Falco*
*University College London, United Kingdom*
elena.falco.18@ucl.ac.uk

Instruments have something to do with the production of scientific knowledge. That much is relatively uncontroversial. But what, exactly, is their role? Within STS and philosophy of technology, some attention has been given to this question, but there is ample space for development. Bruno Latour conceives instruments as repositories of power; Davis Baird, of knowledge. Ronald Giere sees instruments as an integral part of our perspective on reality; authors within postphenomenology emphasise how instruments mediate the relationship between the scientist and their object of study, as well as between scientists themselves. Building on this work, I will argue that instruments also embed assumptions about the world. By doing this, and using fMRI as a focus for my analysis, I will highlight the – so far under appreciated – role of designers and early adopters of scientific instruments.

---

### Are we focussing on 'today' enough?

*Jaspreet Jagdev*
*University College London, United Kingdom*
jaspreet.jagdev.15@ucl.ac.uk

Technologies are becoming increasingly sophisticated, and are being used in increasingly complex ways. As it stands, much of the focus on technology has looked at its influence on methodologies in science and the epistemology of our knowledge making practises on the one hand and on the ethical consequences of technology in society on the other, what is missing is considering the effects that technology has on us as actors and users. This means that there is room for significant benefit, and significant distress. In looking at this, are we focussing too much on how technologies are developing, and less on what they are doing to us now? Consider AI - AI technologies are being used to make decisions about our ability to claim insurance, what adverts we see, and what news articles we interact with. Yet, when we think of AI, we think of self-driving cars and similar technologies. Should we scale back and look at works similar to Safiya Noble and Virginia Eubanks to see how technologies of today are harming some, and protecting others? This paper will examine the interaction between humans and emerging technologies allowing us to think about what risks they pose for us as well as their epistemic benefits.

---

### Technology in classificatory practices

*Emma Tobin*
*University College London, United Kingdom*
e.tobin@ucl.ac.uk

Recently in the philosophy of classification, there has been an epistemological turn – theorists have sought to elaborate epistemological criteria for natural kindhood and membership and then construct a metaphysics that fits the epistemology best. Some of the epistemology first approaches have made very clear how crucial technology is in classification. Despite this, traditionally, philosophers have been sceptical about technological kinds often termed artifact kinds. Most of the metaphysical accounts ignore most of the kinds that scientists actually use and this is particularly the case for kinds that are mediated via technology. Tobin will motivate the view that artifact kinds ought not to be understood as what makes up the furniture of the world, but as the kinds of things that humans refer to in reasoning and scientific investigation. It is to be hoped that this way of looking at classificatory practices is much closely aligned to how scientists think about classification. This epistemological approach to classification serves as a useful example from SPSP to show how important it is to ask questions about technology. Ignoring the role of technology in scientific practice in this case obscures and misleads both the epistemology and metaphysics of classification. Providing a proper philosophy of technologically mediated classification serves to ammeliorate our theoretical accounts of classification in practice.

---

## Statistical classification methodology as technology

*Christian Hennig*
*University of Bologna, Italy*
Christian.hennig@unibo.it

"Cluster analysis" or "unsupervised classification" refers to methodology for automatically finding classes in data. These methodologies are applied in various fields such as image segmentation or biological species delimitation. Cluster analysis is often based on models that assume the existence of a uniquely true classification. Using an exemplary application, Hennig will problematize the status of this assumption, and suggest that the classifications found by these methods always depend on definitions and specifications provided by the user that may vary between situations.

---

## Technology in cognitive science

*Ines Hipolito*
*Humboldt University Berlin, Germany*
inesh@uowmail.edu.au

Today most of our social interaction is made in digital and/or smart environments: we use digital things as a way of interacting with the environment. What is the form of the cognitive relation with technological artefacts and what are the implications for wellbeing of the smart worlds we are creating? Are artefacts mediators between us and the natural world? Or are technological artefacts an integral part of the worlds we are creating? Hipolito will propose that the relation between technological artefacts should be seen as a form of niche construction. Beavers build dams, humans build (technological) tools. In doing so, humans alter their own and another species' local environment as well as the human-nature couplings. This will allow us then to ask how we can work with the technology to construct environments that are health-driven, i.e. that enhance and improve mental health and wellbeing.

---

### Technology in Biology and Bioinformatics

*Emanuele Ratti*
*Johannes Kepler University Linz, Austria*
emanuele.ratti@jku.at

In the past few years, HPS and STS scholars have investigated the nature of bioinformatics. These works have thoroughly documented how bioinformatics tools have become essential to manage and organize the recent data explosion that has overwhelmed biology. However, this emphasis on data management is also a limit of this literature, because it reduces bioinformatics to a set of computational tools that provides mere support to wet-lab biologists (e.g. databases). But bioinformatics is not just data management; it is also a set of computational tools to do (in-silico) experiments. What does it mean for bioinformatics to do biological experiments? How do these experiments differ from traditional, material experiments? What is the epistemic role played by technology in material and bioinformatics experiments in the context of molecular biology?

---

### The philosophical novelty of technology

*Juan M. Durán*
*Technische Universität Delft, Netherlands*
j.m.duran@tudelft.nl

In 2009, a short-lived debate emerged over whether computer simulations required a new — or a non-trivially revised — philosophy of science. Two positions surfaced: one claiming that any philosophical issue that came up in connection with this technology was not specific to simulations but variants of problems that are discussed in more familiar philosophical contexts; the other stated the opposite by presenting concepts that cannot be capture by familiar philosophy of science (e.g., epistemic opacity and the distinction "in principle/in practice") Durán revisits this debate with two purposes in mind. First, to show how failing to appreciate the philosophical merits of technology can impede philosophical analysis. Second, to show how looking into current practice and development of computer simulations (e.g., via software engineering) can shed new — or non-trivially novel — light on old philosophical problems. To achieve these ends, Durán will discuss the notion of the "simulation model" and its implications for the philosophical interpretation of scientific representation and explanation.

---

## Causation and Evidence in Political Science

**Organizers & contributors: Yafeng Shan, Jon Williamson and Rosa Runhardt**

Causal inferences and explanations abound in political science. Recently, there have been many discussions on the methodology and understanding of causal inferences and explanations among political scientists and philosophers of science. That said, there is still no consensus about central issues. For example, the question of how to establish a causal claim in political science is still very much under debate. Some maintain that a causal inference can be legitimately made based on a particular type of evidence, while others argue that causal claims can only be established when there is a variety of evidence. How to understand causal analyses in political science is also controversial. Do different methods of causal analysis imply that there are different concepts of causality? In sum, there are two central questions:

1) What should political scientists do in order to make a causal claim?

2) What is the best way to characterise the causal practice of political scientists?

Although these questions are of interest to both political scientists and philosophers, their work iso some extent parallel to each other. In particular, political scientists pay rather little attention to philosophical work on causation. This symposium aims to examine the two central questions by engaging both the methodological reflections by political scientists and contemporary philosophical analysis of causation. The talks in the symposium will shed new light on methodological and philosophical issues relating to causal analysis in political science and integrate the work of political scientists with that of philosophers. Runhardt will examine the application of an interventionist theory of causality to political science. Shan will focus on the methodology of single-case causal analysis and reject the view that evidence of mechanisms alone is sufficient to establish single-case causal claims. Williamson will argue for a new approach to evidence-based policy, EBP+, that handles evidential diversity in policy evaluation.

---

**From Philosophy of Causation to Political Science Practice: Studying Type-Level Claims with the Interventionist Theory of Causation**

*Rosa Runhardt*

*Radboud University, Netherlands*

rosa.runhardt@ru.nl

Philosophy of causation has received insufficient attention by political scientists. Many political scientists are unsure about the kind of fundamental causal relation (e.g. probabilistic, mechanistic, or process) their preferred method corroborates (Rohlfing and Zuber 2021, 1638). Similarly, they are often unsure of the empirical conditions under which the causal claims they make are corroborated (ibid). However, corroborating causal claims is at the heart of both political science practice and any subsequent policy-making. Therefore, there is a need for further engagement between philosophers of science and political science practitioners and methodologists.

The discrepancy between philosophy and practice is most obvious in the study of type-level causal claims. Such claims are defined by philosophers as a relation between event types or properties. As examples, political scientists hail such claims as the democratic peace theory, which claims that democracies rarely, if ever go to war against one another. Political scientists study type-level claims with various methods, including: process-tracing analyses within multiple cases, which are then generalized to a type-level theory; large-N quantitative analyses which find the average treatment effect of democratic status on the (probability of) war onset for a large data set of countries; and mixed-method designs which combine the aforementioned two approaches.

Problematically, conceptual pluralists believe that each political science method speaks towards radically different concepts of causation: "moving from method to method we would in fact change the hypothesis to be tested" (Reiss 2009, 28). For the conceptual pluralist, the claims investigated by different methods may be incommensurable, making mixed-method designs infeasible. In this paper, I argue that 'conceptual pluralism' for methods aimed at type-level claims is mistaken and that mixed-methods approaches are in fact possible. However, I also argue that mixed methods require a solid grounding in philosophy of causation which, given Rohlfing and Zuber's results, is not yet available. I then provide this grounding.

In the first half of the paper, I explore whether an interventionist philosophy of causation can ground both quantitative analysis and process-tracing, which would mean that the type-level causal claims made by these methods are commensurable. I extend an intuition in Rohlfing and Zuber (2021) that quantitative research is mainly based on interventionist theories of causation, and combine it with earlier work which argued that process-tracing can also be founded on such theories (Runhardt 2015). In the second part of the paper, I consider the objection that process-tracing is inherently token-level and that an interventionist framework is therefore unsuitable for grounding such work. I reject this by applying the connections Woodward has drawn between his type-level theory and token-level causation (Woodward 2003 chapter 3).

---

**Is evidence of mechanisms sufficient for political scientists to make within-case causal claims?**

*Yafeng Shan*
*University of Kent, United Kingdom*
y.shan@kent.ac.uk

Political scientists are interested in studying causes of rare events. What are the causes of World War I? What are the causes of the terrorist attack on the World Trade Center on 11 September 2001? What are the causal factors of the weak American welfare state? A standard method used to investigate these problems is process-tracing, which is typically defined as a method to unpack causal mechanisms (Beach and Pedersen 2013; Crasnow 2017). Many political scientists contend that it is sufficient to establish a causal claim by identifying an underlying mechanism. However, such a view is incompatible with Evidential Pluralism, which maintains that in order to establish a causal claim, one normally needs both evidence of correlation and evidence of mechanisms (Russo and Williamson 2007; Shan and Williamson 2021). This paper defends the application of Evidential Pluralism in the context of political science by arguing that it is not sufficient to make a within-case causal claim with evidence of mechanism alone.

I begin addressing two arguments for the view that evidence of correlation is not necessary for making within-case causal claims in political science. The first argument stems from a concern that evidence of correlation is difficult to obtain in the cases of rare events. The second argument is from the observation that qualitative political scientists are not concerned with quantitative methods. I argue that neither of the arguments is compelling by showing that both arguments assume some misunderstanding of evidence of correlation. I illustrate the argument with a case study of Weinstein's work on violence in civil war (2007).

---

**Evidential Pluralism and evidence-based policy: EBP+**

*Jon Williamson*
*University of Kent, United Kingdom*
j.williamson@kent.ac.uk

Evidential Pluralism holds (i) that in order to establish a causal claim one normally needs to establish that the putative cause and effect are appropriately correlated and there is an appropriate mechanism that can account for the correlation, so (ii) in order to evaluate a causal claim one needs to assess both association studies and mechanistic studies where available. Evidential Pluralism has been applied to medicine in the form of the EBM+ programme, which recommends assessing mechanistic studies alongside the association studies that are the focus of standard evidence-based medicine. This paper seeks to apply Evidential Pluralism to policy evaluation, leading to EBP+, an analogue of EBM+.

After providing an introduction to Evidential Pluralism and EBM+, I set out the key principles of EBP+ evaluation and provide some tools for those seeking to carry out such an evaluation. Then I explain the connection between EBP+ and a variety of other approaches to evaluation that also have a role for evidence of mechanisms. These include realist evaluation, effectiveness-implementation hybrid designs, the common elements approach, contribution analysis, causal mediation analysis, theory of change, logic models, process tracing, process evaluation, and certain mixed methods approaches.

References

Beach, Derek, and Rasmus Brun Pedersen. 2013. Process-Tracing Methods. Ann Arbor, MI: The University of Michigan Press.

Crasnow, Sharon. 2017. "Process Tracing in Political Science: What's the Story?" Studies in History and Philosophy of Science 62 (1): 6–13.

Reiss, Julian. 2009. "Causation in the Social Sciences: Evidence, Inference, and Purposes." Philosophy of the Social Sciences 39 (1): 20–40.

Rohlfing, I., and C. I. Zuber. 2021. "Check Your Truth Conditions! Clarifying the Relationship between Theories of Causation and Social Science Methods for Causal Inference." Sociological Methods and Research 50 (4): 1623–1659.

Runhardt, Rosa W. 2015. "Evidence for Causal Mechanisms in Social Science: Recommendations from Woodward's Manipulability Theory of Causation." Philosophy of Science 82 (5): 1296–1307.

Russo, Federica, and Jon Williamson. 2007. "Interpreting Causality in the Health Sciences." International Studies in the Philosophy of Science 21 (2): 157–70.

Shan, Yafeng, and Jon Williamson. 2021. "Applying Evidential Pluralism to the Social Sciences." European Journal for Philosophy of Science 11 (4).

Weinstein, Jeremy M. 2007. Inside Rebellion: The Politics of Insurgent Violence. Cambridge: Cambridge University Press.

Woodward, James. 2003. Making Things Happen: A Theory of Causal Explanation. Oxford: Oxford University Press.

---

## The Dynamics of Diagnoses

**Organizers: Karin Tybjerg and Sarah Yvonnet**

**Contributors: Julia Tinland, Ariane Hanemaayer and Lara Keuck, Helene Scott-Fordsmand, Sarah Yvonnet and Karin Tybjerg**

Diagnoses are emerging as an important – if philosophically somewhat understudied – element of medical knowledge and practice. They offer a possibility for investigating connections between knowing and doing in medicine, i.e. between knowledge of disease categories and practices investigating/treating patients or between clinic and research laboratory.

Much philosophical literature concentrates on either knowing or doing. A focus on the categorizing or labelling aspect of diagnostics thus examines the logical, epistemological and ontological criteria required to attribute such a label, while a focus on the process has resulted in work on decision theory that was investigated the use of AI, fuzzy logic and differential diagnostics (Reiss and Ankeny, 2022).

However, the participants of this symposium explore Annemarie Jutel's suggestion that the nature of diagnoses is dual: diagnoses are both labels and processes (Jutel, 2009). Concerns with this dynamism of diagnoses have led to new research, for instance, how diagnostic categories trades precision off against flexibility so they can act as "epistemic hubs" for different groups (Kutschenko 2011), how diagnoses evolve in an iterative process (Chang 2017), and how taxonomic work in research is plastic (Green et al 2021). As such, diagnoses are both shaped by and influencing medical activity, they serve patients, doctors and researchers, and they have important temporal and social aspects. They are thus interdisciplinary objects/processes and are as such of particular interest for the philosophy of science in practice.

With this symposium, we propose to gather philosophers and researchers from adjacent disciplines to discuss different dynamics that underlie the establishment, development and application of diagnoses and to focus on continuities between changing categories.

First, we will try to understand how diagnostic categories move and evolve across time. Julia Tinland will propose an analysis of the oft-conflated concepts of medicalization, de-medicalization, over-medicalization, and under-medicalization and how they relate to diagnosis. Second, Ariane Hanemaayer will look into how diagnostic categories maintain continuity while undergoing change with new methods of diagnosing. Third, Helene Scott-Fordsman will examine whether clinical diagnostic practices can serve to enlighten us on the relation between abstractions (taxonomies) and concrete reality (patient bodies). Finally, Karin Tybjerg and Sarah Yvonnet will try to investigate how patients' tissues, data and diagnoses are integrated into a research center to understand how the diagnoses of the patients are translated into research categories and questioned.

Sharing a common methodology drawing on ethnographic or historic methods and closely interacting with the different stakeholders of the field of biomedicine (researchers, medical doctors and patients), these papers offer a collection of new perspectives on the different dynamics underlying diagnoses and disease categories.

References

Chang, H (2017) "Epistemic Iterations and Natural Kinds: Realism and Pluralism in Psychiatry" in K. S. Kendler and J Parnas, Philosophical Issues in Psychiatry IV: Classification of Psychiatric Illness. Oxford: Oxford University Press.

Green S, Carusi A, Hoeyer K. (2019) Plastic diagnostics: The remaking of disease and evidence in personalized medicine. Social Science & Medicine 18:112318.

Kutschenko, L. K. (2011). How to Make Sense of Broadly Applied Medical Classification Systems: Introducing Epistemic Hubs. History and Philosophy of the Life Sciences, 33(4), 583–601.

Reiss, J and Ankeny, R A. (2022) "Philosophy of Medicine", The Stanford Encyclopedia of Philosophy, Edward N. Zalta (ed.), forthcoming `https://plato.stanford.edu/archives/spr2022/entries/medicine/`.

**Medicalisation, demedicalisation, overmedicalisation and undermedicalisation**

*Julia Tinland*
*Sorbonne University, France*
julia.tinland@sorbonne-universite.fr

The aim of this paper is to study in depth how several conditions and phenomena came to be under the purview of medicine (medicalisation) or, on the contrary, be removed from it (demedicalisation).

The term "medicalization" was historically associated with a strong criticism of medicine's perceived overreach into areas of human life that ought to have remained free from it (Conrad, 1992), though it now seems important to distinguish medicalisation as a more neutral phenomenon from its critical and normative counterpart: "over-medicalisation" (Rose, 2007). The process of medicalisation can impact diagnostic classifications, as it tends to widen them (Moynihan, 2011), but also the ways in which previously non-medical problems are addressed.

Many different factors have been said to drive such processes, including the depoliticisation of politico-social concerns (Zola, 1972), the pharmacological industry (Szasz, 1990) or even state-driven forms of biopower (Foucault, 1976). A more careful analysis of specific cases of medicalisation might shed some light on the role of other relevant stakeholders, and in particular of patients and citizens themselves.

This might also draw attention to another, though less commonly formulated, criticism of "under-medicalization" regarding a purported failure to pay medical attention to conditions or phenomena that should nevertheless be addressed and treated medically. Such criticisms have notably emanated from feminist advocates and people suffering from medically unexplained symptoms (Nettleton, 2006).

Examples of "demedicalisation", meaning the process through which conditions and phenomena cease to belong to the medical realm, are less numerous. This might explain why it has not yet been studied with the same attention as medicalisation, though it does open a fascinating window into complex dynamics at the juncture of the development of personalised (or precision) medicine and current economic, social and political concerns.

This paper aims to clarify closely-related but distinctive concepts: the conflation of medicalisation, demedicalisation, over-medicalisation and under-medicalisation is a significant problem when one aims to hone in on and refine normative discourses regarding ethical issues of over-medicalisation and under-medicalisation. It does so through specific case studies and the in-depth analysis of the more specific example of Chronic Lymphocytic

Leukemia (CLL), based on field work, interviews and questionnaire submissions carried out in the Pitié-Salpêtrière hospital in Paris.

References

Conrad, Peter. 1992. 'Medicalization and Social Control'. Annual Review of Sociology 18 (1): 209–32.

Foucault, Michel. 1994. Histoire de la sexualité I. Gallimard.

Moynihan, Ray, Jenny Doust, and David Henry. 2012. 'Preventing Overdiagnosis: How to Stop Harming the Healthy'. BMJ (Clinical Research Ed.) 344 (May)

Nettleton, Sarah. 2006. '"I Just Want Permission to Be Ill": Towards a Sociology of Medically Unexplained Symptoms'. Social Science & Medicine 62 (5): 1167–78.

Rose, Nikolas. 2007. 'Beyond Medicalisation'. The Lancet 369 (9562): 700–702.

Szasz, Thomas S. 1992. The Myth of Mental Illness. The Restoration of Dialogue: Readings in the Philosophy of Clinical Psychology. Page 175-182. United States, American Psychological Association.

Zola, Irving Kenneth. 1982. Missing Pieces: A Chronicle of Living with a Disability. Temple University Press.

---

## Continuity in Medical Classification

*Ariane Hanemaayer and Lara Keuck*

*Brandon University, Canada*

HanemaayerA@BrandonU.CA

Since the 1950s, philosophers of science have argued for principles to guide the design of medical classification systems and diagnostic categories. Whereas taxonomic change has been theorized at length, the notion of continuity has received less attention. What is continuity? And how can we study it? What kinds of continuity claims have been employed? And what are the subsequent implications for historical, philosophical, and social explanations for how we understand the nature of medical classification? We will present work from an ongoing project to answer these questions.

Drawing on the neurological example of Multiple Sclerosis (MS), we explore the tensions within what Rosenberg (2002) has called the natural history of disease continuity, such as classification within disease states (e.g., there are 4 categories of MS), and the epistemic structures that necessitate the revision of these categories over time. MS is an interesting case study for considering continuity in medicine because its classification and diagnostic criteria have gone through a period of ongoing revision starting in the mid-1990s until present. Some have attributed this revolution of diagnosis to the development of MRI techniques, as “seeing into” the brain has provided novel ways to map the states of demyelination, an essential criterium for establishing a diagnosis of MS. These revisions, while increasing specificity have decreased sensitivity, resulting in a worry within the medical community about over- or mis-diagnosis. We argue that epistemic structures guide how certain continuities are maintained even in processes that claim to be about change and revision. For instance, the processes of classifying disease, the techniques and technologies that render disease states knowable, and the objects that traverse localities (“demyelination events”) are shaped by the evaluative concepts in science and principles of evidence-based medicine. In elucidating and systemizing various understandings of continuity, we hope to also contribute to more exchange between philosophy and social studies of medicine.

---

## Diagnosing bodies in a messy reality: finding the right fit between concrete and abstract

*Helene Scott-Fordsmand*

*University of Cambridge, UK*

helene.scottfordsmand@hotmail.com

It is perhaps obvious to state that diagnostic categories are abstractions or idealisations in comparison to the concrete patient bodies that they are applied to every day in clinical practice; as one clinician told me: “It’s just a box that we made up, and then a lot of things fit into it”. They are so, because a diagnosis should not only provide adequate description of the patient at hand, but also act as a hook into the vast reservoir of medical knowledge; pointing towards potential treatment or prevention strategies or, at least, revealing something about the prognosis. As ideal objects, diagnostic categories are thus meant to group, relate, and sort medical cases, more than mirror clinical reality 1:1. At the same time, it is of course important that a diagnosis is “right” – the hook into medical knowledge only helps the patient, if it hooks into the right reserve. This leaves the clinical diagnostic practice with a curious tension between depending on categories that deliberately neutralise differences to obtain relatability across patients, while also aiming at correctness and rightness for the individual patient (a conundrum often phrased as the art/science ambiguity of medicine). Discussions in philosophy of medicine address this in the notion clinical judgement as something specific to medical practice (Upshur and Chin-Yee 2017). However, studies of laboratory practices have shown that similar forms of judgement (tacit and intangible) play a major role in translating between scientific theories and experimental setups and results (e.g., Rheinberger 1997, Knorr-Cetina 1999). Taking point of departure in Alex Broadbent's inquiry model of medicine (Broadbent 2019), I will attempt to map out ways in which the tension between abstract and concrete in the clinical encounter – between medical practitioner and patient; diagnostic categories and suffering bodies – is similar to or differs from abstract-concrete tensions in "the lab". Particularly, I will argue that the question of focus (choosing which aspects of reality count as significant) and interpretation (choosing which aspect of the abstract category are allowed flexibility) are shared between the clinical and the lab when passing judgement and choosing "the right fit", while differences appear in terms of ethics, tempo, and measure of success. Keeping these variations in mind, I will ask whether clinical diagnostic practices can enlighten us on the relation between knowledge, abstraction, and concrete reality more broadly. The paper foreshadows ethnographic fieldwork in medical practice and thus aims to develop preliminary thoughts on diagnosing bodies in the clinic, and to ask questions and raise ideas, more than present findings and finished arguments.

References

Broadbent, Alex. 2019. 'The Inquiry Model of Medicine'. Canadian Medical Association Journal 191 (4): E105–6.

Knorr-Cetina, Karin. 1999. Epistemic Cultures: How the Sciences Make Knowledge. Cambridge, Mass: Harvard University Press.

Rheinberger, Hans-Jörg. 1997. Toward a History of Epistemic Things: Synthesizing Proteins in the Test Tube. Writing Science. Stanford, Calif: Stanford University Press.

Upshur, Ross, and Benjamin Chin-Yee. 2017. 'Clinical Judgement'. In The Routledge Companion to Philosophy of Medicine, edited by Miriam Solomon, Jeremy R. Simon, and Harold Kincaid. Routledge Philosophy Companions. New York: Routledge, Taylor & Francis Group.

## From bedside to bench: Using diagnosis to understand the relation between lab and clinic

*Sarah Yvonnet*[a] *and Karin Tybjerg*[b]
[a,b] *University of Copenhagen, Denmark*
[a] sarah.yvonnet@sund.ku.dk; [b] tybjerg@sund.ku.dk

Translational research has been increasingly presented in the literature as a way to fill in the gap in biomedicine between what we know and what we do – captured in the catch-phrase from bench to bedside. However, several authors stress the importance of reframing translational research as a two-way process (Marincola, 2003). In other words, translational research should also ensure that biomedical research is informed by problems and evidence met in clinical practice.

One way for clinics to enter the lab is through clinical samples. The main benefit from using patients' samples is to avoid the downsides of the standardized tools such as model organisms and cell lines that can be criticized for being far from in vivo conditions (Edmonson et al., 2014). In this case, the sample stands for the disease rather than diseased body (Rosenberg 2002) moving from the lesion to the disease categorization. The sample is then subdivided and re-organized into molecular information (Crabu, 2016) that give rise to disease stratification.

This process could be regarded as a case of reduction of medicine and pathology to biology (Keating and Cambrosio, 2004). However, we claim that samples entering the lab should be regarded as a crosspoint where the clinic and the laboratory are realigned instead of one being reduced to the other. We will trace the shift from diagnoses to research categories through the use of medical samples which provide an interface for medicine and biology to interact and for clinical evidence to become research hypotheses and ultimately diagnostic categories.

Our analysis will be developed through the following of the SEGMENT project that aims at subtyping T2D and obesity patients based on tissue samples from biopsies from different organs. Therefore, our paper proposes a unique opportunity to track how the diagnosed samples enter the biomedical research center and influence relationships between lab and clinic. Ultimately, our goal will be to understand how samples from patients diagnosed with T2D are not merely reduced to biology but translated into and impact research categories.

References

Crabu, S. (2016). Translational biomedicine in action: Constructing biomarkers across laboratory and benchside. Social Theory & Health, 14(3), 312-331.

Edmondson, R., Broglie, J. J., Adcock, A. F., & Yang, L. (2014). Three-dimensional cell culture systems and their applications in drug discovery and cell-based biosensors. Assay and Drug Development Technologies, 12(4), 207–218.

Keating, P., & Cambrosio, A. (2004). Does biomedicine entail the successful reduction of pathology to biology?. Perspectives in biology and medicine, 47(3), 357-371.

Marincola, F. M. (2011). The trouble with translational medicine. Journal of internal medicine, 270(2), 123-127.

Rosenberg, Charles E (2002) "The Tyranny of Diagnosis: specific entities and individual experience" Milbank Quarterly 80(2):237-60.

---

## Purification, manipulation, and replication: The making of biological entities in experimental biology

**Organizers & contributors: Gabriel Vallejos-Baccelliere, Stephan Guttinger and Maurizio Esposito**

The diversification of biological research agendas usually triggers fierce discussions among scholars outside lecture halls of most faculties of biology. Biochemists and cellular biologists, organismic and molecular biologists, physiologists, and structural biologists, etc. accuse each other of underpinning their knowledge with artificial constructions and misleading extrapolations. Questions such as; What can you learn about cells and organisms by studying isolated proteins? What can you learn about human physiology by studying rats? What can you learn about anything natural by studying things designed and selected to work and being manipulated in a lab (including model organisms)? Are therefore hotly debated.

However, biologists know that, in most areas of the life sciences, reliable knowledge can only be obtained by controlling a specific portion of the world using artificial experimental systems. No doubt, biological theories and models, which have been the main concern in philosophical work, aim to describe what "really" happens in the living systems. But most knowledge used and implied in those models and theories is originated in laboratories. Hence, the clash between the artificial and the natural is ubiquitous in biology.

For generations, scientists have developed a vast number of methodologies and strategies to deal with the many epistemological problems that emerge in their day-to-day laboratory practice. So, the question that we must ask is how is it that experimental biology has succeeded in many cases and how such success is explicable.

In this symposium, we address some key aspects of knowledge production in biology, focusing, especially, on the intricate trajectories linking laboratory practices and biological theories. Maurizio Esposito will talk about the historical and philosophical relationship between the maker's knowledge tradition and the philosophy of experimental practices. He will suggest that we should reconsider the maker's knowledge approach in epistemology for a better understanding of the scientific labor in artificial settings and with "artificial" entities.

Stephan Guttinger will talk about how reliable knowledge is produced in the artificial setting of the laboratory space. Looking at the case of protein biology, he will analyse how stabilising practices are used by researchers to create replicable outcomes. He will argue that a focus on these practices and their inherent shortcomings illustrates a need to re-think what we count as "normal" failure rates for replication attempts in experimental practice.

Gabriel Vallejos-Baccelliere will talk about the material connection between experimental systems in biology, taking protein biochemistry as an example. He will argue that paying attention to the purification processes of biological entities provides a more comprehensive picture of the practices and knowledge production in experimental biology and makes it possible to glimpse new solutions to the in vitro/in vivo problem and other issues that could emerge from biochemical practice.

---

## The maker's knowledge tradition and the philosophy of scientific practice: restoring a lost (and useful) connection

*Maurizio Esposito*

*University of Lisbon, Portugal*

mauriespo@gmail.com

It is widely known that back in the 1980s philosophers and historians of science started to challenge the theory-centric view of science. Such a "practical" turn opened a lively intellectual space where traditional dichotomies such as realism/antirealism, theory/practice, fact/value could be retooled. It also spurred a genuine and productive interest over many elements surrounding experimental practices, instruments, and laboratories (3). What is less known is that historians and philosophers of science had been deeply interested in experimental practices well before the 1980s, even though they did not express it necessarily in English (7).

In the talk, I first argue that the so-called "practical turn" in the 80s needs to be contextualized. It should not be considered as a "general" paradigmatic shift but, rather, as a reaction against one specific offshoot of 20th-century anglophone epistemology. In fact, such a reaction left out one of the richest intellectual traditions that would have been attuned with the aims of the new pragmatic sensibilities. The tradition has been properly (or unproperly) called "maker's knowledge tradition" and assumes that making and knowing are fundamentally entrenched.

The history of this tradition is long and largely uncharted. As far as we know, one of his first advocates was Hippocrates (5). The tradition was successfully overshadowed by Plato and Aristotle, while Hellenistic, medieval, and renaissance scholars resurrected it sporadically (1-4). In the modern period, Francis Bacon and Giambattista Vico proposed new sophisticated versions of it (6). In 1710, Vico suggested that "scientific" knowledge could not be reduced to true beliefs or representations because knowledge was mainly about action and production. His view of epistemic action could refer to abstract or material entities, whether we consider numbers, figures, or concrete artifacts (8). The maker's knowledge tradition thrived into the 19th century but, in the 20th century, it was overshadowed again by post-positivist and analytic offshoots of Anglophonic philosophies of science and then "unwittingly" revived in the 1980s "practical turn" (4). The tradition has been recently resurrected by Luciano Floridi, who has distinguished between two general epistemic approaches: the user's knowledge approach and the maker's knowledge approach (2).

In discussing some of the most interesting Floridi's insights about the maker's approach and his distinction between constructivism and constructionism, I show how (and why) we should seriously reconsider the maker's knowledge tradition for a better comprehension of experimental practices, in biochemistry and beyond.

References

Farrington B 1947, Head and Hand in Ancient Greece, Watts & Co. London

Floridi L 2011, A Defense of Constructionism: Philosophy as a Conceptual Engineering, Metaphilosophy, Vol. 42, N. 3

Hacking I 1983 Representing and Intervening: Introductory Topics in the Philosophy of Natural Science, Cambridge University Press, Cambridge

Lobkowicz N. 1970. Theory and Practice: History of a Concept, University Press of America

Mondolfo R, 1971, Verum-Factum: Desde antes de Vico hasta Marx, Siglo XXI Editores

Pérez-Ramos A, 1988, Francis Bacon's Idea of Science and the Maker's Knowledge Tradition. Oxford: Oxford University Press

Simons M, Vagelli M 2021, Were experiments ever neglected? Ian Hacking and the history of the philosophy of experiments, Philosophical Inquiries. 9 (1). p.167-188

Vico G, 2008, Metafisica e Metodo, Bompiani, Milano

---

**The question of "normal" failure rates in laboratory science**

*Stephan Guttinger*

*University of Exeter, United Kingdom*

s.m.guttinger@exeter.ac.uk

Replicability is seen as a key attribute of science: if scientific results cannot be replicated, they are at best questionable and at worst wrong. An underlying assumption of this view is that nature is fundamentally stable and regular. This means that if an entity or process is interrogated in the same way, under the same conditions, it will give the same response. Any failure of regularity becomes a failure of the interrogator. Replicability is not an achievement but something that is lost through malpractice. This implies that in well-conducted research the failure rate for replications should be close to zero.

In this talk I want to explore the question of what counts as a "normal" failure rate by turning the above approach on its head. I will assume that replicability is something that is always gained, not lost. Based on a process view of nature, I will start from the idea that regularity and stability are not a given; the only constant in nature is change. If that is the case then the stabilities that appear in an experimental system are often hard-won achievements, rather than a reflection of the default state of the entities analysed.

This shift in thinking is supported by the fact that a central aspect of doing laboratory-based research is to establish and maintain the stability of the objects of interest. To analyse this struggle for stability in more detail, I will focus on the case of protein biology. Here researchers are fighting on several fronts. Within and across experiments, they are trying to keep their protein of interest stable, managing its constant degradation and unfolding. Without stabilisation of these processes, the output in protein biology would vary from experiment to experiment, often in non-replicable ways.

The analysis will show how protein biologists have developed numerous practices and technologies to generate stable objects of inquiry. The technologies involved are a dominant but often-overlooked part of the laboratory space and include, among other things, the common fridge, the Styrofoam ice bucket, or the buffer solution.

The analysis of the various technologies and practices of stabilisation will show how researchers use them as surrogates for native stabilising processes. This surrogacy process, however, is fraught with challenges. The case of "intrinsically disordered proteins" will illustrate how even slight variations in the implementation and alignment of these processes can lead to significant deviations in what is observed, which can in turn lead to failures to replicate other researchers' results. These are not instances of lost replicability or signs of malpractice. They are simply cases in which replicability could not be produced.

I will argue that this alternative framing of replication and stability suggests that our notion of "normal" failure rates for replications has to be adjusted upwards. This also has implications for how we assess recent claims about a "replication crisis" in the experimental sciences.

---

## Purifying entities, connecting experimental systems, and extrapolating biochemical knowledge

*Gabriel Vallejos-Baccelliere*

*Universidad de Chile, Chile*

gvallejos@ug.uchile.cl

Experimentation with purified entities is a central practice in biochemistry, but it is also the source of its major epistemological issue: the in vitro/in vivo problem, which consists in justifying how is it possible to obtain knowledge about the nature of biological systems studying its parts in isolation and in contexts different than their natural milieu. This issue is part of the most general problem of extrapolation in experimental biology, but it has been barely addressed.

Most of the philosophical literature about the subject [1-5] has been focused on theoretical and representational aspects of the problem, considering the entity already purified as the starting point of analysis. I argue that if we focus on practices surrounding the purifying processes, we can explore new possible solutions to the in vitro/in vivo problem.

Successful experimental practices require highly standardized processes for preparing reliable materials for constructing stable and replicable experimental systems (ESs) [6]. Many times, this preparative experimentation

[7, 8] occupies much of the time of the experimental work; purifying entities, like proteins, is an example of that. To be successful, the process must rely on robust causal factors.

A purification process is a concatenation of ESs in which a sample produced by one of them is an ingredient to construct the next one. For example, in purifying a recombinant protein, we start with a cell culture and, through many intermediate ESs (e.g., cell lysis, selective precipitation, dialysis, chromatographic separation, affinity-based separation, etc.), a solution of purified protein is obtained. The process also has many ramifications, like protein quantification, purity assessment, etc. In each step, a sample is produced and is used to construct the next. Each ES consists in manipulating and controlling a phenomenon using many robust causal properties of the protein, like its shape, electrical charge, solubility, thermal stability, the capacity of interaction with other entities, etc. So, paying attention solely to the already purified entity and neglecting the whole purification process will produce a very limited picture of the practices and knowledge production in biochemistry.

Preparative experimentation is just a strand in a whole material network of experimentation in which each ES is materially connected with many others via sharing different common parts. Herein, many properties of the same entity can be studied and, above all, used to manipulate a vast number of other entities and, in turn, control many other phenomena in different ES.

This material network of experimentation is the substrate for most of the knowledge produced in experimental biology (and, I dare to say, also in the rest of biology). To doubt about the existence of the properties and entities used and studied in it, would also undermine a huge number of practices [6]. So, they can be connected to many models and theories about the real functioning of biological systems. In emphasizing the consistency and robustness of this material network, it is possible to glimpse new solutions to the in vitro/in vivo problem and other issues that could emerge from biochemical practice.

References

[1] Strand R, Fjelland R & Flatmark T. (1996) “In vivo interpretation of in vitro effect studies with a detailed analysis of the method of in vitro transcription in isolated cell nuclei”. Acta Biotheoretica 44: 1–21

[2] Strand R. (1999) “Towards a useful philosophy of biochemistry: Sketches and examples”. Foundations of Chemistry 1 (3): 269-292.

[3] Jacob C. (2002) “Philosophy and biochemistry: Research at the interface between chemistry and biology”. Foundations of Chemistry 4 (2): 97-125.

[4] Ibarra A & Mormann T. (2006) "Scientific Theories as Intervening Representations" Theoria 21, Nº 55: 21-38

[5] García P. (2015) "Computer simulations and experiments: in vivo–in vitro conditions in biochemistry". Foundations of Chemistry 17 (1):49-65

[6] Esposito M. & Vallejos G. (2020) "Performative Epistemology and the Philosophy of Experimental Biology: A Synoptic Overview". en Baravalle L. & Zaterka L. (eds.) Life and Evolution - Latin American Essays on the History and Philosophy of Biology. Springer.

[7] Weber M. (2005) Philosophy of Experimental Biology. Cambridge University Press.

[8] Waters K. (2008) "How Practical Know-How Contextualizes Theoretical Knowledge: Exporting Causal Knowledge from Laboratory to Nature". Philosophy of science 75 (5): 707–719.

## Feminist Philosophy of Biology Beyond Gender

**Organizers & contributors: Sophie Veigl, Tamar Schneider, Azita Chellappoo, Maya Roman**

Feminist epistemology and philosophy of science have become major branches within philosophy of science, with insights from these fields bearing on longstanding debates surrounding notions of objectivity, knowledge, and scientific methodology, and the role of values in science. Although feminist philosophers of science have held a wide variety of (sometimes conflicting) positions, there has been a general consensus regarding the inseparability of the epistemic from the social, political and cultural, and the construction of knowers not as isolated islands, but rather as fundamentally and inextricably socially situated.

The biological sciences in particular have often been a central target of feminist philosophical critique. Feminist philosophers of biology have, for example, challenged sexist and androcentric biases in fields such as sociobiology, evolutionary psychology, and primatology (Bleier, 1984; Haraway, 1989; Lloyd, 2003), queried our practices of sex categorisation (Fausto-Sterling, 2000), and challenged reductionist tendencies in models of the effects of sex hormones in development (Longino, 1990). Perhaps unsurprisingly, feminist philosophy of biology has often primarily been concerned with gender as a central analytical category. Many of the areas in which significant contributions have been made have been those where gendered assumptions, biases, or values have a clear role in shaping scientific knowledge production.

We do, however, think that this preoccupation restricts the full potential of feminist epistemology in the philosophy of science. We regard feminist epistemologies as first and foremost tools to uncover how societal power relations are inscribed in scientific inquiry. Given the wealth of feminist scholarship demonstrating the long reach of societal power relations, and the social situatedness of knowledge, there should be no conceptual or pragmatic restriction on which phenomena are subject to 'feminist' critique. The tools of feminist epistemology in general, and feminist philosophy of biology in particular, can and should be fruitfully applied in areas beyond the 'usual suspects'. This perspective leads us to the identification of two lacunae in the literature: firstly, the comparative lack of attention to other forms of social hierarchy and their intersections in the context of critiques of science and scientific knowledge production. Although there is a wealth of literature on intersectionality and its implications for epistemology, the application of the insights of feminist philosophy of biology to the operation of social categories other than gender remains underdeveloped. Secondly, the tools that feminist philosophy of biology can offer, including the subtle role of non-epistemic values in guiding theory choice, have not been widely deployed to interrogate areas of the biosciences that do not appear to directly engage with social hierarchies or social relations. Nevertheless, these tools could be useful in understanding how knowledge production works in these areas.

In this symposium we aim to explore and go some way towards resolving these lacuna or areas of underdevelopment by (1) addressing broad conceptual questions that arise when understanding how feminist philosophy of biology can be fruitfully applied 'beyond gender', and (2) exploring concrete examples within scientific practice where drawing on feminist philosophy of biology could be generative and is, as yet, underdiscussed.

---

**Conceptual Resources for a Feminist Philosophy Beyond Gender**

*Sophie Veigl*
*University of Vienna, Austria*
sophie_veigl@hotmail.com

Feminist epistemology and feminist philosophy of science in particular are a diverse set of undertakings to unravel the bearing of societal power relations, particularly the power-relations pertaining to gender, onto the study material of the sciences. Given this approach most feminist philosophy of science is preoccupied with certain realms within the sciences. For instance, evolutionary biology, fertilization research, or anthropology have been primary areas of interest. One explanation for this preoccupation could be the following: First of all, we regard certain disciplines as more "instrumental" than others (Rosenberg, 1994), these disciplines bear stronger and closer on human lives because they study aspects of human life (physics would, in that sense of the word, be less instrumental than, say psychology). We expect thus in those areas more gendered assumptions, particularly in parts of those disciplines that seem directly associated with the biological construction of gender. In this talk, I will try to sample and conceptually explore instances and directions that feminist philosophy of science could take that go beyond the usual areas of interest. The aim is to provide a preliminary conceptual toolbox for such endeavors.

I shall particularly consider these issues regarding 1) disciplines, 2) case studies, and 3) methods/approaches. The first issue concerns investigating what feminist philosophy of science in less instrumental disciplines looks like. How to do feminist philosophy of chemistry or physics? Second, I will investigate the potential of case studies beyond the usual area of interest. That is – what is there to say about quarks, the Krebs cycle, a coral reef? And thirdly, how can different feminist approaches so far established contribute to such questions? On the one hand, what is the analytical lens of such approaches, e.g. the forgotten contributions of women, filling in the gaps of what is studied, or tackling and rethinking theoretical frameworks and methodologies (Crasnow, 2014)? On the other hand, what is the theoretical orientation of these approaches - how to provide a feminist empiricist/ standpoint theorist/ deconstructive/ post-millennial feminist critique in such case studies?

From these deliberations, I will formulate a first and tentative characterization of feminist philosophy of science beyond gender. I will argue for an understanding of feminist epistemology and philosophy of science that first and foremost sees it as a toolbox to uncover power relations within society. Such power relations, however, are and cannot be confined to issues regarding gender. On the one hand, as many scholars have shown, the (re-)construction of gender in study materials is intertwined with racist, classist, and ableist biases. These also need to be considered when expanding feminist philosophy to unexplored territory. On the other hand, as power relations hold also between human and non-human actors, such as animals, plants, and land, feminist epistemology is naturally geared towards addressing and deconstructing these relations. Once this is fully realized, it will be possible to enter a new phase of feminist investigations of the study of the biotic and abiotic world around us.

## Bacteriology between Chemical Interactions and Pathogenic Individuals

*Tamar Schneider*
*Tel Aviv University, Israel*
tamisch0106@gmail.com

Placing feminist epistemologies as an analytic project of rationality and scientific practices, Longino emphasizes interactions as the subject of study in contrast with individuals (Longino 1997, 2008, 2021). Using Longino's analytic tool, I examine two methodological and conceptual approaches in microbiology. One is widely known as the germ theory and the microbial causality in diseases, and the other took the direction of soil microbiology, later establishing the foundation of biogeochemical study in ecology. Historically, both emerged from Pasteur's laboratory although the latter only in a non-directed way (Ackert 2006, 2007). Following these two scientific frameworks I show their different methodologies, classification and research questions depending on the purposes of the study and its objectives. Then, I discuss their different notions of causality, the specificity and control in the germ theory, and that of the thermodynamic process of life cycle in ecology. Analyzing these differences also reveals the tension between the emphasis on the individuals in the former and that of interactions in the latter.

The scientific observation that centers on the individual follows the examination of causal factors shaping an individual (i.e., forces and vectors on the object, or genetic and environmental factors on an organism). In contrast, Longino suggests following a scientific observation advancing "interactions not as explanatory factors, but as the objects of explanation." (Longino 2021, 12). Interactions as the objects of explanation move the focus of observation from the individuals to that of the processes of exchange between individuals (De Jaeghere et al. 2010; Schneider 2021; Longino 2021). The object of explanation is the process of exchange and from that to the exchanging individuals. Longino's analysis shows that the classification of a phenomenon such as behavior (also, I argue, that of metabolism, or niche construction) within an individualist or interactionist perspective is not theory-neutral. Such an act of classification will "already set limits to the kinds of theoretical/explanatory approaches that will be relevant." (Longino 2021, 14). This perspective also acknowledges the ontological significance of the individuals' interdependence and their environmental context (both biotic and abiotic).

Centering on the different role interactions and individuals have in microbiology I reveal the constraints of each approach and different perspectives discussing the background beliefs. Each approach, I argue, also follows a different notion of causation looking at the functional-role of microbes within the phenomenon in question (i.e., disease or plant/soil parasites). The pathogenic/non-pathogenic study follows the notion of one causal direction and the life cycle process follows the mutuality of interactions which is the ontological heterogeneity notion of causality. Each notion of causality entails different apparatus leading to different ways of observation and interpretation thus ways of seeing and interacting with microbes living around and inside us. Furthermore, in looking at these different emphases from the perspective of power relations such as the relations of dominance of humans on natural phenomena can critically examine our epistemic attitudes in the scientific inquiry towered the microbial world.

---

### "Obesity Science" & Standpoint Theory

*Azita Chellappoo*
*The Open University, United Kingdom*
azita.chellappoo@open.ac.uk

"Obesity" has been designated as a public health crisis for several decades, receiving sustained attention from scientists, clinicians, public health experts and policymakers. Mainstream biomedical understandings of "obesity" typically hold that "obesity" is inherently unhealthy or dysfunctional, or even a disease in itself. However, this conception of "obesity" has not gone without pushback: work within the growing field of fat studies challenges the connection between weight and health, as well as highlighting the damaging effects of weight stigma and reclaiming the term 'fat' as a pushback against the medicalizing and pathologizing connotations of the term "obesity". Research in the various fields that make up "obesity science" has received attention from sociologists and science studies scholars in recent years (Rich et al, 2011; Warin, 2015). However, surprisingly, given the contested nature of the terrain, and the clear potential for the influence of social values, scientific investigations into "obesity" have been almost entirely neglected by philosophers of science.

In this talk I will argue that standpoint theory provides a fruitful resource for investigating the role of values in knowledge production in "obesity science". As a feminist epistemology, standpoint theory is distinctive in its endorsement of the 'thesis of epistemic privilege', which broadly holds that those who are socially marginalised are more likely to generate perspectives that are "less partial and less distorted" (Harding, 1991, 121).

Although many standpoint theorists have focused on the epistemic advantage accruing to marginalised genders, similar claims can be made about other social hierarchies. Empirical evidence suggests that fatphobia and fat discrimination is widespread, both on an interpersonal and structural level. Given this, scholars have argued that fatness constitutes a salient social category, in that the lives of fat people differ systematically from others (Eller, 2014; Mollow, 2015).

I draw on a case study taken from microbiome science to illustrate the ways in which standpoint theory can illuminate the role of values in scientific research into “obesity”. As next generation sequencing technologies have developed, enabling the characterisation of whole microbial communities in the gut, vagina, mouth, and so on, a subset of microbiome researchers have been increasingly focused on the potential to identify characteristics of an ‘obesogenic’ microbiome, which could cause or predispose an individual to be “obese”. I analyse the causal pathways that are mapped in this research, and suggest that the pathway from the social environment to the microbiome has been notably neglected. In particular, the effect that fatphobia or fat discrimination might have on the microbiome, and therefore on the inferences made by microbiome scientists, has received little attention, despite lines of evidence that would suggest the potential for this to play an important role. I argue that following an amended version of Harding’s call - to start out research from fat people’s lives - would result in a more robust understanding of the complex interactions.

---

## The relevance of feminist philosophy of science for philosophy of mind

*Maya Roman*
*Tel Aviv University, Israel*
mayar.eretz@gmail.com

In this paper, I will argue that the feminist movement’s influence on science (Schibinger 1999) is a highly relevant historical case study that should inform ongoing debates in philosophy of mind and cognitive science, particularly regarding reductive theories of mind such as Daniel Dennett’s.

The feminist movement, feminist epistemology, and feminist philosophy of science have influenced generations of women and feminist scientists who advanced and changed their respective fields, moving science forward. This change is particularly evident in disciplines such as archeology (Wylie 1997) and evolutionary biology (Longino and Doell 1996), where feminist critiques have become widely accepted. Such changes point to a clear connection between social structure and scientific knowledge, as several feminist philosophers of science have pointed out (Harding 1987, Keller 1985). However, this evident connection has seldom been studied in connection with philosophy of mind.

In this talk, I will present the impactful contribution of feminist epistemologies to deciphering what Wilfrid Sellars termed “The Clash of the Images” in philosophy of mind. Sellars noted two ways of describing and explaining perceivable phenomena – the scientific and the manifest image. The scientific image explains by referring to imperceptible entities who interact mechanistically. The manifest image refers to people, agents who can act intentionally, for a reason. These two images are incommensurable and thus, clash.

Reductive accounts in philosophy of mind argue that the manifest image is utterly reducible without remainder to the scientific image. Dennett, for example, argues that the way to resolve the incommensurability of the images is to consider the manifest image as non-referring, unable to causally impact the world (Dennett 2017).

I examine cases where feminism has changed science, arguing that they pose a problem for reductive accounts of the mind. The second wave of feminism in the U.S., for example, changed the scientific image by changing the manifest image. Feminists changed the manifest image by broadening the definition of a person to include women. This change was created using resources unique to the manifest image, such as consciousness-raising, coalition building, and organizing for collective action, resources that require the existence of norms and the ability to hold people accountable. This is a case where manifest changes to the manifest image led to the advancement of the scientific image, once feminist scientists and philosophers began critiquing science. I argue that this historical development resists reductive accounts of the mind. According to reductive accounts, such advancements can, at best, be considered random. They cannot be explained as rational. Thus, the influence feminism has had on science creates a problem for reductive accounts which must explain how such scientific developments happened, why they happened in such vicinity to changes in the manifest image, and how they could have been achieved without the resources of the manifest image.

This argument showcases another vital aspect of feminist philosophy and history of science as a case study whose success must be accounted for by future philosophies of science and mind.

## Making Geologic Time

**Organizers & contributors: Joeri Witteveen, Alisa Bokulich, and Hernan Bobadilla**

The study of deep time is at the center of many sciences, including geology, evolutionary biology, and climate science. While exposed layers of rock around the world can provide a peek into the earth's past, they far from deliver a readily legible record of our planet's history. Geologists need to wrestle information about the chronology of the earth from the partial, perturbed, and incompletely preserved rock strata and their contents. In the second half of the 20th century, stratigraphers worldwide initiated an effort to construct a global Geologic Time Scale based on data about the lithologic, magnetic, chemical, biological, and other attributes of rock layers. This ongoing endeavor to construct a time-calibrated periodization of earth history provides fertile ground for a practice-based philosophy of geologic time. For example, the complex array of methods, models, and varieties of data involved in the measurement of geologic time provides new insights into the epistemic dynamics of calibration and correlation in the context of the historical sciences. Closely related to these themes about measuring geologic time are issues about the standardization and periodization of geologic time. The effort to construct a Geologic Time Scale prompts questions about the presuppositions and implications of the aim to formalize and unify the hierarchical units of geologic history. Some of these questions concern the epistemic and normative dimensions of conventionality and naturalness that are familiar from the philosophy of metrology and biological taxonomy, but that are manifested in the geologic context in new and unexpected ways. What is more, the governance of geochronology raises topical social epistemic questions about the relation between geology, other sciences, and society. This is illustrated in particular by the heated debate over the recognition of the 'Anthropocene' as geochronologic unit, which is thrusting questions about measuring and making geologic time into the limelight of the public eye.

**Learning to Measure What Isn't There: The Problem of Missing Time**

*Alisa Bokulich*
*Boston University, United States*
abokulic@bu.edu

The primary source of our knowledge about geologic time and Earth's 4.5 billion-year history is the stratigraphic (rock) record and the various different clues that are preserved in its layers. Although there are many interesting challenges in reconstructing geologic time from these records, one of the most difficult—and seemingly intractable—issues in the foundations of geologic time is what we might call the "problem of missing time." This problem results from gaps or "hiatuses" in the geologic record, which can arise either from stasis (no sediment deposited) or because the sedimentary layers once deposited were subsequently eroded away. Gaps appear in the stratigraphic record as an "unconformity"—a boundary between two different bodies of rock representing two discontinuous periods of time. he most famous of these is the Great Unconformity, first identified in the Grand Canyon (Powell 1875) and believed to represent anywhere from 100 million to 1 billion years of missing time. The Great Unconformity lies just below the Cambrian strata and its erosion history has been speculated to be a cause or effect of some of the most puzzling and important events in Earth's history (e.g., Snowball Earth, onset of plate tectonics, rise of free O2 in atmosphere, and the Cambrian explosion). How much time is missing from the geologic record? And precisely which periods of Earth's history do they represent? Answering these questions is essential not just for reconstructing geologic time, but also for beginning to discriminate among the above causal hypotheses. While one might have thought these were intractable questions, whose answers were lost to time, surprisingly geoscientists are developing a new suite of methods, known as "deep-time thermochronology," to quantitatively measure the timing and duration of the rock record that isn't there.

The rise of deep-time thermochronology provides a striking example of what I call "unconceived opportunities" in the historical sciences, that is, the discovery of new sources of data about phenomena that we would have antecedently thought were not empirically accessible. These new sources of data are typically not "ready made" in the historical sciences, but rather require vast amounts of foundational laboratory work, field studies, and advances in modeling and theory to come together in order to extract this data, and turn "detritus into evidence" (Jeffares 2010). In this talk I analyze how geoscientists are learning to quantitatively measure the duration and timing of gaps in the stratigraphic record using deep-time thermochronology. I draw three philosophical lessons from this case: First, there is far more experimental laboratory work that goes into the historical sciences than is often appreciated (e.g., Cleland 2001, 2002). Second, thermochronology provides a philosophically rich example of scientific measurement, advancing work in the philosophy of data, our understanding of derived measurements (e.g., Parker 2017), and model-data symbiosis (e.g., Edwards 2010; Bokulich forthcoming). Third, and finally, I draw out the implications of this case for the "optimism vs. pessimism" debate about the historical sciences (e.g., Turner 2007, 2016; Jeffares 2010; Currie 2018).

---

## What is at Stake in the Formalization of a Chronostratigraphic Unit? A Case Study on the Anthropocene

*Hernan Bobadilla*
*University of Vienna, Austria*
hernan.bobadilla@univie.ac.at

In 2000, Paul Crutzen and Eugene Stoermer proposed to use the term "Anthropocene" for the current geological epoch, to acknowledge the ongoing impact of human activities on the earth. This impact has been well-documented in a vast number of scientific publications. However, most of this impact is reported at the critical zone, i.e. the interface between atmosphere, hydrosphere, biosphere, cryosphere and pedosphere. There is less compelling evidence about the impact of humans on the lithosphere. This is the predicament: It is in the lithospheric domain that geological epochs have been traditionally formalized, based on stratigraphic evidence.

The aim of this paper is to explore and assess some of the stakes of the formalization of the Anthropocene as a geological epoch from a philosophical point of view. I focus on the predicaments for the International Commission on Stratigraphy (ICS) of formalizing the Anthropocene as a chronostratigraphic unit. The Anthropocene Working Group (AWG), a panel of scientists within the ICS, is preparing a rigorous proposal to formalize the Anthropocene as a new chronostratigraphic unit. The AWG's official stance is that the Anthropocene should be treated as a formal chronostratigraphic unit and that its base should be established based on a stratigraphic signal around the mid-twentieth century. Still, the AWG's stance and related endeavours have been intensely debated and some of their merits have been questioned. Rejection by the ICS remains a realistic scenario.

In order to explore and assess the stakes of this process, I distinguish and explicate two senses of formalization, namely the descriptive and the evaluative senses. In the descriptive sense, formalization amounts to providing an explicit and rigorous articulation of a concept, method or theory. In the evaluative sense, formalizing means giving approval to or endorsing of a concept, method, or theory by relevant institutions or groups.

With this distinction at hand, I assert the following. First, I submit that there are formalizations of the Anthropocene, in both descriptive and evaluative senses, beyond the confines of the ICS, which reveals a disunity of the sciences. Second, I suggest that some calls for rejecting the formalization of the Anthropocene in the context of the ICS are concerned with a lack of descriptive formality of the proposals in the form of incoherencies. I argue that these incoherencies are not a decisive reason for rejection because: i) they could be transient states towards more coherent arrangements; and ii) the ICS has accepted and keeps operating under less-than-coherent arrangements. Third, I claim that the ICS could take a stance in terms of the evaluative formalization of the Anthropocene, given its orthogonality to its descriptive formalization and its potential political consequences. In this regard, I attempt to dispel some of the scepticism concerning the political impact of formalizing the Anthropocene in its evaluative sense.

---

## Golden Spikes, Silver Bullets, and the Ma(r)king of Chronostratigraphic Boundaries

*Joeri Witteveen*
*University of Copenhagen, Denmark*
jw@ind.ku.dk

The geologic time scale divides the history of the earth into a hierarchy of geochronologic (or chronostratigraphic) units, from eons down to eras, periods, epochs, and ages. Since the 1970s, the International Commission on Stratigraphy and its subcommissions have been tasked with formally establishing the boundaries of these units by designating so-called 'Global Stratotype Section and Points' (GSSPs). A GSSP is a reference standard for a chronostratigraphic boundary. It is often marked by driving a 'golden spike' into a rock section at an agreed point.

A prima facie puzzling aspect of the GSSP approach is the recommendation that a golden spike be placed at a horizon where, geologically speaking, "nothing happened" (McLaren 1970). Even a chronostratigraphic unit was introduced because of a perceived natural break in the geologic record, the GSSP approach mandates that the unit's formal boundaries are to be placed in sections that lack any abrupt changes in lithology or fossil content. The rationale for assigning GSSPs to horizons of geologic non-events is to establish boundaries that do not shift under advances in stratigraphic resolution or with changing perspectives on what is 'natural'. By thus divorcing questions about definition from empirical disputes geological events, GSSPs have been regarded as providing a silver bullet solution to the problem of creating consensus about chronostratigraphic boundaries.

However, the practice of agreeing on, designating, and using GSSPs has proven to be considerably more complicated that the abstract principle. Indeed, an increasing number of GSSPs is being redesignated, because of problems with using the original boundary markers in practice. My co-symposiast Alisa Bokulich has recently argued that this makes GSSPs akin to other 'scientific types', such as type specimens and measurement prototypes (Bokulich, 2020). All these scientific types are concrete, tangible reference standards that suffer from problems of application due to their fragile material nature.

In this presentation, I argue that although the category of scientific types is useful prompt for comparing material reference standards from different areas of scientific practice, on a conceptual level there is more that divides these standards than what unites them. Moreover, I show that while the existing philosophical literature allows us to recognize key conceptual differences type specimens and measurement prototypes qua reference standards, GSSPs are a case apart. I address this in the final part of the talk, by offering a systematic classification of (material) reference standards that helps to appreciate the differences and similarities between reference fixing practices in different areas of science.

# Abstracts of Contributed Papers (alphabetical by last name of lead author)

## The Experimentalism of Jacques Rohault

*Ovidiu Babeș*
*University of Bucharest, Romania*
ovidiu.babes@icub.unibuc.ro

Jacques Rohault was a prominent Cartesians of the second half of the seventeenth century, as well as a renowned experimentalist. Rohault gave public lectures on experimental practices, and he devised new experimental setups to enforce Cartesian explanations of natural phenomena. His 1671 Traité de Physique remained a standard textbook on natural philosophy for a long time. The Traité was translated into Latin and annotated by Samuel Clarke, with subsequent editions being published until 1735. Rohault's text, along with Clarke's annotations, proved to be an important battleground of Cartesian and Newtonian physics at the beginning of the eighteenth century. Yet the confrontation between the two physical systems reframed the methodological role of Rohault's experiments, which initially had quite a different aim. Today's historiography of Rohault is still informed by Clarke's reading of the Traité. This contribution challenges this narrative.

I argue that that Jacques Rohault's experimentalism in his successful Traité de Physique is best understood as a practice of trial experimenting. Most saliently, the experimentalism in the Traité deals with the motions commonly ascribed to the fear of vacuum. The purpose of Rohault's practice was explore and support the hypothesis that the cause of such motions was not the fear of vacuum (as Aristotelians, atomists or Galileans would have claimed) but the weight and spring of air. To make the claim I focus on Rohault's experimental descriptions of motions happening inside a syringe, in a barometric tube of mercury (or water), as well as in his own "vacuum within a vacuum" device.

The role of experimentalism in Rohault's philosophy is controversial. For Spink (2017), Rohault is the typical experimental philosopher: he is the pure experimenter concerned with proving and disproving various hypotheses. For others (e.g. Dobre 2013, 2019; Schmaltz 2016), he took a mitigated experimental approach: Rohault did his best to fill the gaps of Descartes' physics by experimentally illustrating Cartesian explanations of phenomena. Yet, on this latter view, Rohault did not experimentally test the basic Cartesian ontological assumptions, thereby making no real progress towards a more substantive experimentalism (cf. also McClauglin 1972, 1979; Roux 2013).

Such historiographical depictions heavily rely on Rohault's own philosophical classification of experiments. Experiments, Rohault claims, are of three types: mere perceptions, trials and tests. Mere perception consists in the simple, unguided usage of our senses. Trial experiments are deliberate setups in which various devices, materials, or natural effects are explored and recorded. Explorations do not have predetermined outcomes—their aim is to display the multitude of possible results and to secure experimental replicability. Lastly, experiments of testing corroborate or disprove physical explanations by confronting theoretical predictions with experimental outcomes. Both readings of Rohault, either as a pure or as a mitigated experimentalist, rest on interpreting Rohault's syringe and barometric experiments as putting forward experiments of testing.

However, the function Rohault's experiments on the fear of vacuum is more complex. They comprise all three types of experiments, with a focus on trial experiments. His experiments do not concern the empirical refutation of vacuum, but the establishment of the weight of air as a cause for the phenomena in question. With the new explanation in place, Rohault then describes several experiments which explore various properties of air and subtle matter. These experiments are best described as trials, not tests. It was Clarke who took these experiments as tests when annotating the several subsequent editions of Rohault's Traité. The notes were polemical, and Clarke heavily advocated for the existence of vacuum and its role in explaining the experimental phenomena. Rohault's aim, however, was different, and should be distinguished from how Clarke read it.

**Mechanistic Reductionism**

*Tudor Baetu*
*Université du Québec à Trois-Rivières, Canada*
tmbaetu@gmail.com

Whether mechanistic explanations in neuroscience are compatible with a physicalist variety of reductionism depends on the nature of the relationship between mechanisms and the phenomena they explain. According to the constitutive account, mechanistic explanations show how higher-level phenomena consist of concerted behaviours of mechanistic components (Craver and Bechtel 2007). This constitution relationship providing a straightforward means of demonstrating the physicochemical nature of biological phenomena or the biological nature of psychological phenomena. Alternatively, according to the etiological account, mechanisms cause phenomena (Craver 2007). Since causality is construed as a relationship between ontologically distinct 'cause' and 'effect' items, some authors concluded that there is no obvious way in which mechanistic explanations could support reductionism. In this paper, I argue for a third alternative. This account is motivated by the fact that, in most cases, scientific findings consist of evidence for causal relevance and causal mediation generated by controlled experiments. This kind of evidence supports neither a constitution nor an etiological account (Baetu 2012; Harinen 2014). According to this account, descriptions as diverse as 'black box' phenomena, mechanistic sketches and detailed mechanistic explanations refer to the same causal structure circumscribed within the spatiotemporal boundaries of a replicable experimental setup (Baetu 2019). I argue that despite being framed in causal terms, this account allows for a physicalist variety of reductionism. Since the referents of variables probed by experiments are not known to stand in identity or part-whole constitution relationships, reduction cannot be driven by a direct mapping of variables onto other variables. Nevertheless, reductionism can proceed via the discovery of biophysical mediators of higher-level, psychosocial causal processes. Using an example drawn from pain research (Woo et al. 2017), I show how evidence for mediation can lead to the elimination of higher-level, biophysically-unmediated, pathways and that, under the assumption of parsimony, certain psychosocial variables can be collapsed onto biophysical variables. Causal mediation analysis resulting in the ruling out of putative explanations in terms of mental or higher-level causation, along with the possible collapsing of psychosocial variables onto biophysical ones opens new possibilities for implementing physicalism. In particular, they may provide an experimental confirmation of the principles of physical causal completeness and of causal exclusion thus far assumed on strictly a priori grounds by certain physicalist accounts of the mind [e.g. (Kim 2005)].

REFERENCES

Baetu, T. M. 2012. "Filling In the Mechanistic Details: Two-Variable Experiments as Tests for Constitutive Relevance." European Journal for Philosophy of Science 2 (3):337-53.

———. 2019. Mechanisms in Molecular Biology. Edited by Grant Ramsey and Michael Ruse, Elements in the Philosophy of Biology. Cambridge: Cambridge University Press.

Craver, C. 2007. Explaining the Brain: Mechanisms and the Mosaic Unity of Neuroscience. Oxford: Clarendon Press.

Craver, C., and W. Bechtel. 2007. "Top-Down Causation without Top-Down Causes." Biology and Philosophy 22:547-63.

Harinen, T. 2014. "Mutual Manipulability and Causal Inbetweenness." Synthese 195 (1):34-54.

Kim, J. 2005. Physicalism or Something Near Enough Princeton, NJ: Princeton University Press.

Woo, C. W., L. Schmidt, et al. 2017. "Quantifying Cerebral Contributions to Pain beyond Nociception." Nature Communications.

---

## Emotions as Conceptual and Extended: A Comparison of the Act and Scaffolded Mind Theories of Emotions

*Joshua Barden*

*University of Alberta, Canada*

jtbarden@ualberta.ca

In various publications over the past two decades, Lisa Feldman Barrett has advanced the controversial idea that emotions are natural kinds despite being socially constructed. She argues that those who claim that emotions are not natural kinds because of their diverse biological underpinnings (e.g., Paul Griffiths) fail to consider that "collective agreement" on what functions certain behaviours play is partially constitutive of the reality of emotions. Her "Conceptual Act Theory of Emotion" uses philosophical resources provided by John Searle to show how collective intentionality constitutes emotions as ontologically subjective entities that are nonetheless real aspects of our social life. In brief, Barrett believes that social ontology, in addition to physical or biological ontology, must be considered by any account of human emotions for it to be complete.

In this paper, I demonstrate that Barrett's social ontology fails to do the work she thinks it can in her attempt to show that emotions are real. I show that her emphasis on societal members' in-principle, unconscious agreement as a central element of collective intentionality is ill-founded insofar as it allows for multiple, distinct emotions to be considered the same despite their different affective qualities and behavioural functions. I further argue that Barrett's account can be improved if she augments her agreement-based social ontology with the broader notion of a "scaffolded mind," which was initially developed by Kim Sterelny as a contribution to the growing literature on so-called "4E cognition." Sterelny believes that by producing specialized cognitive tools (such as telescopes and smartphones) and making other modifications to our environment, human beings develop "epistemic niches" that facilitate otherwise complex cognitive tasks and effective behavioural regulation. Giovanna Colombetti and Joel Krueger have further developed this account to show how we also produce "affective niches" to facilitate affective and emotional regulation. Notably, both Sterelny and Colombetti hold that modifications in the social environment are central to both epistemic and affective niche construction. Using the concept of affective niche construction, I show that "agreement" is just one part of the social environment that constitutes emotions. By doing so, I relieve Barrett of the necessity of showing that conscious or unconscious agreement is the only social factor that can play a role in the development of emotions as ontologically subjective entities that are nonetheless real aspects of our social experience. In doing so, I show how the resources from the 4E cognition lexicon can inform the cognitive science of emotion and how theories in cognitive science (such as the Conceptual Act Theory of Emotions) can contribute to our philosophical understanding of how human beings experience both themselves and their environment.

---

## Between STRUCTURE and ADMIXTURE: "modeling" models in human population genomics software packages

*Carlos Andrés Barragán*[a] *and James Griesemer*[b]

[a,b] *University of California–Davis, United States*

[a]cabarragan@ucdavis.edu; [b]jrgriesemer@ucdavis.edu

We present in-progress archival, ethnographical, and conceptual analyses of how life-scientists working in the field of human populations genomics ancestry studies (HPGA) currently develop different software packages and experiment with them to answer different and overlapping human genomic problems, such as: the detection of population structure and the number of subpopulations in a given sample / dataset(s), the definition of ancestral populations for admixed populations, and the assignment of ancestral population proportions to sample donors (e.g., ancient, admixed or non-admixed). Yet, despite the mathematical complexity and analytical sophistication behind population genomic software, single packages are not equally useful for answering all the questions being asked for the types of problems mentioned above. Such particularity leaves life-scientists (in both their roles as developers and users of algorithms) with the need to identify strengths and weaknesses of available software and to evaluate their comparative potential for answering specific research problems and questions and their pitfalls for producing misleading or problematic results. Such evaluations have also been used to gesture at the potential for combining uses and/or the need to develop new algorithms that adjust to the particular interests of a research laboratory. We illustrate this dynamic playing out in the larger context of software development in human population genomics, focusing on and comparing two of the early and key software packages: STRUCTURE and ADMIXTURE. Although both are considered to be answering similar problems (e.g., the estimation of global genetic ancestry), there are key differences in how each package models certain human genomic population structures, making them an interesting case study for tracking how such nuances have powerful implications for the robustness assessment and comparability of research findings. In addition to emerging as scientific objects in their own right, we argue that human population genomic software packages are not just tools applied at specific steps in analytical research workflows, they are sites for the re-situation of a variety of kinds of objects involved (e.g., models, datasets, metadata, findings) and means of fitting such objects into the coherent workflows needed to produce new models, findings, and software.

## Rational Reconstruction in the Time of Practice

*William Bausman*
*University of Zurich, Switzerland*
wcbausman@gmail.com

The turn to scientific practice in the epistemology of science renews pressure on philosophers to abandon the Logical Empiricist project of Rational Reconstruction (Carnap 1963) and to dissolve the distinction between the Context of Discovery (the psychology of research) and Context of Justification (the logic and epistemology of research) (Reichenbach 1938). After all, one of the main aims of the practice turn is to capture the epistemology of "actual science" (Soler et al. 2014).

I argue however that we should adapt rational reconstruction to concern practices rather than committing it to the flames. After all, rational reconstruction serves to critique the justification of science and to convince others, and these remain our goals. I propose that we shift away from analyzing scientific results in terms of a formal argument and towards analyzing scientific projects in terms of the combinations of tools and methods they use to investigate the world. For this, we can draw on Hasok Chang's concept of operational coherence: "... the various actions coming together in an effective way towards the achievement of one's aims" (Chang 2017). Evaluating a system's coherence comprises justifying scientific knowledge (both as ability and as information) and understanding the conditions of its success. Therefore, it falls under the context of justification. Moreover, rationally reconstructing, e.g., aspects of a research project in terms of its tools and methods is the best way to evaluate the program and its products for both scholars of science and scientists themselves.

My case study rationally reconstructs the research project Michael Fortelius (Helsinki) leads that uses fossil mammalian teeth as proxies for past climate. Their research consists in collecting and analyzing fossil teeth, understanding the relationships between teethshape and climate, predicting past climate, and corroborating and calibrating with other climate proxies. To show the operational coherence of their practices, I focus on one prediction they make: water in the environment decreased in the late Miocene in Europe (Fortelius et al. 2002). This prediction would follow from two results: 1. Fossil data shows that hypsodonty (molar tooth height) increased in herbivores in the late Miocene (10.5-5 myo) in Europe. 2. Hypsodonty is a proxy for water level on the grounds that increased mean hypsodonty in herbivores is an adaptation to eating plants in an environment with decreased water. Therefore, we need to understand how these claims are justified in terms of the operational coherence many kinds of practices, from fossil collection and analysis, to statistics and modeling, to laboratory and comparative analysis of living organisms. For example, to support adaptation, they must combine chemical analysis of fossil teeth and plants, laboratory study of teeth and abrasion, and natural comparative study of the structure of teeth across groups and functional demands (Fortelius 1985).

Rational reconstruction serves epistemologists but also scientists. It provides an ideal against which we can all see the work to be done by a research project. Not only can outsiders use this reconstruction of practices to assess whether we should believe that water in the environment in Europe decreased in the late Miocene, the scientists themselves perform a similar kind of analysis to construct their research plan and approach. A reliable method for constructing a research plan is to think about whether the plan can achieve its aims and whether it will convince other scientists. In this way, the context of justification considerations should influence the context of discovery considerations. While the justification for trusting the decisions of scientists is not just the reasons the scientists used for making those decisions, scientists do well to consider the potential justification of their approach in their decision making.

References

Carnap, Rudolf. 1963. “Intellectual autobiography.” In The Philosophy of Rudolf Carnap, edited by Paul Arthur Schilpp.

Chang, Hasok. 2017. “VI—Operational Coherence as the Source of Truth.” Proceedings of the Aristotelian Society.

Fortelius, Mikael. 1985. “Ungulate cheek teeth: developmental, functional, and evolutionary interrelations.” Acta Zoologica Fennica 180:1-76.

Fortelius, Mikael, Jussi Eronen, Jukka Jernvall, Liping Liu, Diana Pushkina, Juhani Rinne, Alexey Tesakov, Inesa Vislobokova, Zhaoqun Zhang, and Liping Zhou. 2002. “Fossil mammals resolve regional patterns of Eurasian climate change over 20 million years.” Evolutionary Ecology Research 4 (7):1005-1016.

Reichenbach, Hans. 1938. Experience and prediction: An analysis of the foundations and the structure of knowledge. Chicago: The University of Chicago Press.

Soler, Léna, Sjoerd Zwart, Vincent Israel-Jost, and Michael Lynch. 2014. “Introduction.” In Science After the Practice Turn in the Philosophy, History, and Social Studies of Science, edited by Léna Soler, Sjoerd Zwart, Vincent Israel-Jost and Michael Lynch. New York: Routledge.

# Learning in the world: Van Musschenbroek's lessons for the philosophy of experimentation

*Pieter Beck*
*Ghent University, Belgium*
pieter.beck@ugent.be

In this paper, I discuss Petrus van Musschenbroek's (1692-1761) philosophy and practice of experimentation. In the current literature, van Musschenbroek is mostly mentioned for his "discovery" of the Leiden jar or in the context of his role in the spread of Newton's ideas on the Continent. In his own time, van Musschenbroek was a well-known natural philosopher and a celebrated experimentalist. In an oration titled "On the method of performing physical experiments", van Musschenbroek gave an overview of what we could call his philosophy of experimentation. In my discussion of this philosophy, I will show how the complexity of nature played in important role in his thinking on the method of performing experiments. Van Musschenbroek emphasised that there are always a lot of (unknown) variables at play in experimental research. One therefore needs to repeat and vary one's experiments in order to identify as much relevant variables as possible and to remove hidden sources of disturbances. However, for van Musschenbroek, there were other reasons to vary and repeat an experiment. I show how van Musschenbroek also characterised the process of repeating experiments as a learning process. I argue that this learning process should be seen as a process of augmenting one's practical grasp and understanding of the experimental set-up and the phenomena under investigation. To illustrate these views, I discuss two fields in which van Musschenbroek performed experimental research: the strength of materials and electricity. I show how many points made by van Musschenbroek in his methodological writings were instantiated in his experimental research practice. In both cases, his research was characterised by an emphasis on the variety and heterogeneity of the phenomena under investigation, the need to explore bodies in different ways by means of experiments, and attention for the details of the experimental set-up. In the second part of this paper, I will build upon the discussion of van Musschenbroek's theory and practice of experimentation to provide a more elaborate philosophical discussion of experimental learning as a process of learning in the world. More specifically, I show how the choice to speak about learning in the world, instead of learning about the world, reflects a non-representationalist view on science. It is also connected to a view on science as a practice, more specifically as a situated and dynamic collection of activities. The main aim of this is to provide a philosophical view on the role of experimentation and the nature of scientific learning which allows me to do justice to the experimental research performed by van Musschenbroek. However, I will also make some more general philosophical points. More specifically, I will argue that van Musschenbroek's work and ideas provide an interesting starting point to build further upon Friedrich Steinle's concept of "exploratory experimentation (EE)". Whereas Steinle's notion of EE is still (I would argue) mainly centered on propositional knowledge, my discussion of van Musschenbroek's work will allow me to expand Steinle's notion of EE to include other kinds of learning. As mentioned, I argue that we should understand scientific practice as a process of learning in the world. According to this view, experimental learning is a process of actively engaging with and reshaping the world. The results of this learning process are not limited to propositions, but are also embodied in instruments, processes, procedures, standardised objects, and the skills of practitioners.

---

## Understanding early 20th-century British eugenics: a case study in the history of biomedical validation practices

*Nicola Bertoldi*

*Centre Interuniversitaire de Recherche sur la Science et la Technologie, Canada*

nicola.bertoldi87@gmail.com

Though eugenics "swept the world from the late 19th to the mid-20th century in a remarkable transnational phenomenon" (Bashford & Levine 2010), it flourished with exceptional vigor in the Anglo-American context, where the likes of Francis Galton, Karl Pearson and R. A. Fisher conceived it as "the science which deals with all influences that improve the inborn qualities of a race" (Galton 1904, p. 1). However, by the end of the 1950s, the British eugenic tradition seemed to have been durably discredited. Although recent inquiries into the relations between 20th-century eugenics and British universities have reignited controversies about his role (Cain 2021), a considerable body of historical scholarship has established that Lionel S. Penrose (geneticist, pediatrician and third "Galton Professor of Eugenics" at UCL) was instrumental in the unfolding of the epistemic crisis that led to British eugenics' demise (Kevles 1985; Mazumdar 1992; Ramsden 2013). Based on the role that Penrose's research on the biology of intellectual disability played in this process, we argue that the crisis of British eugenics resulted from a momentous change in the validation practices that had characterized early 20th-century British genetics.

By "validation practices", we understand a manifold of activities organized and regulated according to specific scientific, technical, political and ethical norms and values. In the case of British eugenics, those activities encompassed data-gathering methodologies, theoretical modelling practices, institutions and public policies, which all aimed to ensure the correspondence of theories on human heredity with the relevant biological phenomena and the applicability of those same theories to medical and social problems. Nevertheless, how did such validation practices concretely change? Furthermore, what does this invalidation process tell us about how eugenics, both as a political ideology and as the science of self-directed human evolution, could be "revalidated" due to possible future shifts in biomedical validation practices? We provide some elements for answering those questions by highlighting how Penrose's understanding of the nature of medical practices, both from an individual patient-based and a social perspective, prompted him to adopt new methodologies for collecting and analyzing survey data and reconsider the underlying assumptions of population genetics models for the evolution of hereditary disabilities, i.e. to question the theoretical foundations of British eugenics.

References

Bashford, A., Levine, P. (2010). The Oxford Handbook of the History of Eugenics. Oxford: Oxford University Press.

Cain, J. (2021). Lionel Penrose on Eugenics and anti-Eugenics at UCL. https://profjoecain.net/lionel-penrose-eugenics-anti-eugenics-ucl/

Galton, F. (1904). Eugenics: Its definition, scope, and aims. American Journal of Sociology American Journal of Sociology, 10(1), 1–6.

Kevles, D. J. (1985). In the Name of Eugenics. Genetics and the Uses of Human Heredity. Cambridge, MA: Harvard University Press.

Mazumdar, P. M. H. (1992). Eugenics, Human Genetics and Human Failings. The Eugenics Society, Its Sources and Its Critics in Britain. London: Routledge.

Ramsden, E. (2013). Remodeling the Boundaries of Normality: Lionel S. Penrose and Population Surveys of Mental Ability. In Bernd Gausemeier, S. Müller-Wille, & E. Ramsden (eds.). Human Heredity in the Twentieth Century. London: Pickering and Chatto, pp. 39-54.

## Organizations and Values in Science and Technology

*Justin Biddle*
*Georgia Institute of Technology, United States*
justin.biddle@pubpolicy.gatech.edu

This presentation explores the relationship between values in scientific and technological research, on the one hand, and features of the organizations that conduct that research, on the other. There are at least three different levels at which the nature and role of values can be examined. One is at the level of the individual, which is the level that most philosophers of science have emphasized (e.g., Douglas 2009). At a much higher level, one might examine the ways in which societal considerations – including regulatory decisions, national funding strategies, and political priorities – impact values embedded in research. The focus here is at an intermediate level – namely, the level of the organization. I will draw upon literatures in philosophy of science and organization theory to provide a framework for examining how organizational features impact values in science and technology, with a focus on data sciences and machine learning.

There are at least three aspects of organizations that can significantly impact values embedded in research: organizational aims, organizational structure, and organizational culture. Organizational aims include financial profit (e.g., for-profit firms), provision of public services (e.g., government agencies), and fostering social or political change (e.g., NGOs). Organizational aims can impact values in several ways, including by impacting decisions about problem framing – which problems it considers to be worthy of pursuit and how it frames those problems. Organizational aims also impact the framing of ethical issues – which ethical issues are prioritized and which are not.

Organizational structure concerns the relations that hold between different intra-organizational entities, such as offices and individuals. Organizational structure – in particular, the division of labor between researchers or research activities – influences the range of values brought to bear on research decisions. In the case of research conducted by organizations with strict divisions of labor, values that influence one set of decision tasks will tend to remain unexamined by those involved in other decisions tasks. The impact of division of labor on values is particularly significant in research that involves different organizations with distinct organizational aims, as is the case in many data-driven systems.

Organizational culture includes the norms, values, and assumptions that operate in an organization and impact social relations and decision making. An organization's culture impacts which behaviors are incentivized and which are sanctioned, as well as who is included in decision making. I distinguish between inclusive culture (c.f., Longino 2002) and cut-throat culture and show how the culture of an organization impacts how it manages epistemic risks, which in turn reflects value judgments about the permissibility of various types of impacts and their distribution over different groups.

The relationship between values and organizations is under-studied by philosophers of science, and the framework developed in this presentation can provide a starting point for further research into organizational levers for the management of values in science and technology.

References Douglas, Heather (2009). Science, Policy, and the Value-Free Ideal. Pittsburgh: University of Pittsburgh Press.

Longino, Helen (2002). The Fate of Knowledge. Princeton: Princeton University Press.

---

## Discordant Evidence, Evidential Reasoning and Scientific Practice

*Sofia Blanco Sequeiros*

*University of Helsinki / TINT, Finland*

sofia.blancosequeiros@helsinki.fi

In this paper, I analyze the concept of discordant scientific evidence and the epistemic and methodological challenges that discordant evidence poses for scientific practice. The advantages of concordant scientific evidence are fairly intuitive. Ideally, we want and need scientific evidence that stands clearly for or against a hypothesis. The concept of discordant, i.e. contradictory, evidence remains unclear. How exactly can evidence contradict itself? How do processes of scientific inquiry produce evidence that supports contradictory hypotheses or facts? Can discordant evidence be used in decision making, in science or elsewhere? These are also significant questions in areas such as evidence-informed policy. If the evidence at hand contradicts itself, both scientific reasoning and policymaking become harder.

At its most general, evidential discordance refers to scientific evidence that both confirms and disconfirms the same scientific hypothesis, theory or claim (Stegenga 2009, 2012; Hey 2015). I approach discordance as a phenomenon that both results from and causes more uncertainty in scientific decision making and practice. I argue that discordance is an issue particularly in decision making contexts, and it makes reasoning with scientific evidence harder. I use the distinction between data and phenomena (Woodward and Bogen, 1988) to analyze how discordance develops during the evidence-generating process. Next, I use the concept of enriched evidence (Boyd, 2018) to show how comparisons between pieces of discordant evidence and the uncertainties they contain can be made. In this sense, my analysis is meant to complement the more formal accounts of evidential discordance and amalgamation (e.g. Claveau 2013, Landes 2020).

Last, I use a case example from criminology and economics of crime to illuminate my conceptual analysis. I show how epistemic uncertainty about prison sentences and their effectiveness against recidivism has meant that the relationship between the available data and the phenomenon that the data is meant to track has on has been misconstrued. This has resulted in a discordant body of evidence regarding the effectiveness of prison sentences on recidivism: some evidence points to short prison sentences being more effective against recidivism, other evidence points towards long prison sentences being key. In other words, I show that discordant evidence about the effectiveness of prison sentences against recidivism is not a result only of contextual variation, such as prison systems in different countries, but also the evidence-generating processes with which different pieces of evidence are produced. I conclude with a discussion on how the relationship between uncertainty and discordance should be understood, as well as the role that values can play in mitigating this epistemic uncertainty.

References

Bogen, J., & Woodward, J. (1988). Saving the phenomena. The philosophical review, 97(3), 303-352.

Boyd, N. M. (2018). Evidence enriched. Philosophy of Science, 85(3), 403-421.

Claveau, F. (2013). The independence condition in the variety-of-evidence thesis. Philosophy of Science, 80(1), 94-118.

Hey, S. P. (2015). “Robust and discordant evidence: Methodological lessons from clinical research”. Philosophy of Science, 82(1), 55-75.

Landes, J. (2020). Variety of evidence. Erkenntnis, 85(1), 183-223.

Stegenga J. (2012) “Rerum Concordia Discors: Robustness and Discordant Multimodal Evidence”. In: Soler L., Trizio E., Nickles T., Wimsatt W. (eds) Characterizing the Robustness of Science. Boston Studies in the Philosophy of Science, vol 292. Springer, Dordrecht

Stegenga, J. (2009). “Robustness, discordance, and relevance”. Philosophy of Science, 76(5), 650-661.

## Scientific publishing: a new life for the distinction between the context of discovery and the context of justification?

*Federico Boem*
*University of Twente, Netherlands*
federico.boem@gmail.com

The problem of scientific reproducibility constitutes one of the most pressing and current challenges of contemporary research (both from a scientific and socio-political point of view). One of the aspects of this question concerns the dimension of scientific production and in particular the mechanisms linked to scientific journals. A naive view (often portrayed in the public sphere) sees scientific journals just as tools for disseminating research. However, they also have a normative/constructive dimension. Indeed, a scientific journal is also where the meaning of those who do science and their work is established and negotiated. The scientific journal thus becomes an epistemological and political arena, where the criteria, according to which a study can be called "scientific", is determined. This also shows how the language (the semantic choices) of a scientific publication is not neutral and therefore reflects implicit and explicit choices and needs (both epistemic and not-epistemic). The "rhetoric of science" is therefore not something to be exclusively intended and attributed to a journalistic distortion but can also represent the more or less legitimate forms of narration, associated with scientific studies and discoveries. If it is true that the philosophy of science had re-dimensioned the sharp break between the "context of discovery" and "context of justification", if we consider the practice of scientific publication, this distinction returns to have an interesting role that deserves discussion. This means analyzing the fact that the final scientific publication (i.e. the paper that is actually published, discussed, and taken and source of scientific evidence) is not a faithful/adequate description of the steps actually occurred during the discovery process. Rather, it is often the rational, a posteriori, reconstruction of the reasons and practical choices that lead to the idea behind the study and thus the final "product" as such. Importantly, this reconstruction is often thought in view of a possible scientific explanation but also of a possible "coherence" with the general frame (e.g. a paradigm or an epistemic culture), which that "piece of science" will be part of.

This work aims to provide a new analysis of the difference between the "context of discovery" and the "context of justification" in light of the mechanisms and practices inherent in the world of scientific publications. This will also involve an evaluation of the role and efficacy of the scientific vocabulary in relation to the various purposes (both epistemic and non-epistemic) of research, such as "explanatory efficacy" (ie the chance of being well considered within the scientific community) vs "communicative efficacy"(ie the chance of being favorably viewed by the general public) vs "efficacy in funding possibilities" (ie the chance to have one's own research funded also according to its applicative relevance and its social benefits).

---

## The Context of Construction: Cognitive agents' higher-order thinking skills in knowledge construction processes

*Mieke Boon*[a] *and Mariana Orozco*[b]
[a,b] *University of Twente, Department Philosophy (BMS)*
[a] m.boon@utwente.nl; [b] m.orozco@utwente.nl

Giere (2010) argues that understanding the representational relationship between model and world requires bringing cognitive (scientific) agents and their intentions into the picture: Agents intend to use model, M, to represent a part of the world, W, for some purpose, P. Similarly, we argue that understanding the knowledge construction process —such as the construction of a scientific model, M, for epistemic purpose, P— requires bringing cognitive agents and their intentions into the picture. More specifically, we argue that the quality of the cognitive agents' higher-order thinking skills (HOTS) is crucial for the epistemic and pragmatic quality of the knowledge (e.g., model M) constructed for an epistemic purpose, P. The traditional distinction between the context of discovery (CoD) and justification (CoJ) ensured that the quality of the researchers' thinking processes do not play a role in the epistemic quality of the knowledge. This assumption may still be defensible for research practices dealing with fundamental theories such as the Higgs boson in elementary particle physics. But for inherently interdisciplinary research practices where continuous knowledge construction takes place aimed at specific epistemic purposes, such as in engineering, geo, climate, agriculture, and medical sciences, this assumption is less appropriate. For those practices, we propose "the context of construction," because the construction and justification of knowledge are largely intertwined. We suggest that the context of construction requires novel epistemological accounts that encompass the quality of knowledge construction processes aimed at epistemic results for specific epistemic purposes. Our interdisciplinary (educational-philosophical) contribution to such epistemological accounts is to study how university students learn to do research, by focusing on their HOTS. These thinking skills present qualitative differences (ranging from poorly to extensively developed), which directly impacts the epistemic and pragmatic quality of the knowledge construction process and the epistemic result (i.e., the resulting conceptual model). We studied groups of first-year students in biomedical engineering whose research project assignment was to construct a conceptual model of a biomaterials solution for a physiological defect, using knowledge from scientific literature and textbooks. The epistemological issue is that the conceptual model cannot be constructed straightforwardly (e.g., by inductive or deductive reasoning) based on existing scientific knowledge. The students have to search, decide, and combine. Crucial for this is the ability to ask proper questions and make decisions (e.g., about relevance and assumptions). Our focus in analysing the data was on the quality of their questions (i.e., whether and in what ways such questions are both critical and genuine) and decisions. We argue that the ability to ask questions and make decisions during research processes aimed at constructing a conceptual model, are crucial higher-order thinking skills. Based on our analysis, we will propose a (preliminary) conceptual framework by which the character and quality of questions can be determined. We conclude that the quality of cognitive agents' HOTS, such as 'question asking' and 'decision making,' is crucial to the epistemic and pragmatic quality of the epistemic results, and should therefore be part of epistemological analyses of knowledge construction processes in scientific research practices aimed at knowledge M for epistemic purpose P.

References

Boon, M. 2020a. The role of disciplinary perspectives in an epistemology of scientific models. European journal for philosophy of science, 10(3): 1-34.

Boon, M. 2020b. Scientific methodology in the engineering sciences. Chapter 4 in The Routledge Handbook of the Philosophy of Engineering, ed. D. Michelfelder, and N. Doorn: 80-94. Routledge Taylor and Francis Group.

Giere, R. N. (2010). An agent-based conception of models and scientific representation. Synthese, 172, 269–281.

Knuuttila, T., and Boon, M. 2011. How do Models give us Knowledge? The case of Carnot's ideal Heat Engine. European Journal for Philosophy of Science, 1(3): 309-334.

Luxton-Reilly, A., Denny, P., Plimmer, B., & Sheehan, R. (2012). Activities, affordances and attitude - How student-generated questions assist learning. Paper presented at the Annual Conference on Innovation and Technology in Computer Science Education, ITiCSE.

Marzano, R. J., & al., e. (1988). Dimensions of Thinking: A Framework for Curriculum and Instruction: The Association for Supervision and

Curriculum Development.

Nersessian, N. J. 2009a. How do engineering scientists think? Model-based simulation in biomedical engineering research laboratories. Topics in Cognitive Science, 1(4): 730-757.

Nersessian, N. J., and Newstetter, W. C. 2014. Interdisciplinarity in engineering research and learning. In Cambridge handbook of engineering education research, eds. A. Johri and B. M. Olds, 713-730. Cambridge, UK: Cambridge University Press.

Nersessian, N. J., and Patton, C. 2009. Model-based reasoning in interdisciplinary engineering. In: Philosophy of technology and engineering-sciences, 727-757. North-Holland.

Newstetter, W. C. 2005. Designing cognitive apprenticeships for biomedical engineering. Journal of Engineering Education, 94(2): 207.

Schinkel, A. (2017). The Educational Importance of Deep Wonder. Journal of Philosophy of Education, 51(2), 538-553.

Small, W. (2020). Practical Knowledge and Habits of Mind. JOPE Journal of Philosophy of Education, 54(2), 377-397.

---

## Interactive Kinds and Norm-Enforcement

*Danielle Brown*
*University of Alberta, Canada*
dlbrown@ualberta.ca

This paper investigates the mechanism that undergirds interactive kinds, a concept put forward by Ian Hacking (1995, 1999) and defends an account of the mechanism underlying human interactive kinds relating to their dual-status as epistemological and normative. Hacking argues that kinds in the social or human sciences–psychiatry, sociology, economics–are interactive in that those who are classified interact with their classification, generating feedback loops which may result in changes to the kind itself. This, according to Hacking, produces problems with the stability of the kind that can impede our epistemic practices of empirical generalization and predictions. The two main questions raised with respect to interactive kinds are (1) whether interactivity is exclusive to human kinds, and (2) whether interactivity is a substantial problem for our epistemic practices. After a review of the literature (Khalidi 2013; Cooper 2004; Laimann 2020), I defend an affirmative answer to both questions. Though there exist other sorts of feedback mechanisms in nature, the phenonenon Hacking identifies is unique to human interactive kinds and stems from the fact that human beings are sensitive to norms and values and that kind assignment functions as a norm enforcing mechanism. I accept Laimann's (2020) view that challenge of human interactive kinds to our epistemic practices is not merely instability, but the way both stabilizing and destabilizing feedback obscure or interfere with other relevant causal processes. I push these ideas a step further, arguing that both stabilizing and destabilizing feedback in human interactive kinds are a byproduct of the much larger problem of the role that kinds play in the enforcement of norms.

---

### Inferentialism and Maker's Knowledge

*Dan Burnston*
*Tulane University, United States*
dburnsto@tulane.edu

There are two broad approaches to scientific representation, and how it relates to explanation. The first is what I call "referentialism," which locates the representational and explanatory relationship between a model and the world in some correspondence relation between them. The second approach is "inferentialism," which argues that the representational and explanatory power of a model lies in the relation it has to scientists – namely the way it affords and constrains inferences about the world.

At first glance, referentialism seems to have clear epistemic advantages over inferentialism, since explanatory success is closely tied to referential success. Hence, a successful explanation bears a very close relationship to the world. On the other hand, inferentialism seems inextricably tied to human reasoning processes, and hence risks being psychologistic – i.e., granting explanatory success to models without guaranteeing any objective relation to the world we are trying to explain.

My moves in this paper are two-fold. First, I argue that the apparent advantage of referentialism in explanatory objectivity is misleading. Referentialist views, when faced with widespread abstraction and idealization in science, trend strongly towards fictionalism – the view that models explain something, but that thing isn't the world. Inferentialism, since it does not base explanatory power on structural correspondence, is in no such situation.

Second, I then propose a positive epistemology for inferentialism based on the notion of maker's knowledge. I briefly consider and reject two accounts of maker's knowledge, the "recipe model" and the "looping model," since they fail to solve the epistemic problems for inferentialism. On the recipe model, maker's knowledge is due to having the ability to literally create the thing that is understood. But this fares no better than referentialism at generating knowledge of the actual world. On the looping model, one's actions shape the world into being a certain way (hence, looping is often discussed in the context of social images like pornography, and diagnostic categories such as mental disorders). However, this is not an adequate account of what scientists do, since modelers do not literally create the real-world systems they understand through their models.

As an alternative to the recipe and looping views, I propose an "engineering model" of maker's knowledge, on which practitioners know how to construct artifacts (scientific representations), and learn about the world through what those artifacts allow them to do. I show how, as with other artifacts, scientific models are successful or unsuccessful depending on how well they fulfill their purposes. Unlike many other artifacts, however, the purpose of models is epistemic. A model succeeds or fails depending on whether it can be used to generate confirmable predictions about real-world system. In sum, scientific models are artifacts and scientific modelers are conceptual engineers. I support my claims with examples from systems biology and systems neuroscience.

---

## Scale-Dependent Concepts in Multiscale Modeling: Surfaces, Alloys, and Nucleation

*Julia R.S. Bursten*

*University of Kentucky, United States*

jrbursten@uky.edu

In the past few decades, the philosophical literature on scientific modeling has exploded, and more recently, significant attention has turned to multiscale modeling. A central challenge in multiscale modeling is the problem of rationalizing, or justifying, the use of multiple models that make apparently contradictory or competing assumptions about the nature of the target system. For instance, recent debates over the epistemic and ontological implications of the mathematical approach to multiscale modeling in physics known as the renormalization group have centered on the question of whether a reductive interpretation of the renormalization group sufficiently explains the success of that strategy.

One response to this problem is to home in on the dynamical autonomy of higher-scale models from lower-scale models. For instance, in a recent monograph, Robert Batterman has argued that the only way to justify the use of the same macroscale dynamical model to predict and describe multiple microscale systems is by attention to the mathematical and physical details of the particular systems in question. (A Middle Way: A Non-Fundamental Approach to Many-Body Physics, 2021; ch. 2).

In this talk, I build on Batterman's argument for the dynamical autonomy of higher-scale systems by introducing the notion of the conceptual details of a multiscale model. While Batterman's emphasis is on the mathematical and physical details of multiscale models, I show that in at least some cases of physical modeling, it is details of the theoretical concepts involved in a model that underwrite the use of that particular model to generate explanations and predictions of a given system.

The cases I focus on draw from nanoscience, where multiscale modeling is heavily employed to reconcile the need to describe material behavior simultaneously at both continuum and atomic scales. I use three concepts — the concept of a surface, the concept of an alloy, and the concept of nucleation — to illustrate how multiscale modeling strategies are rationalized, or justified, not solely by appeal to mathematical and physical details but by appeal to the concepts that imbue those details with meaning. A significant consequence of this concepts-forward approach is that at the nanoscale, these concepts themselves shift in response to the shifting mathematical and physical details of the multiscale models. I identify this shift as an instance of the concepts themselves behaving in a scale-dependent manner, and I show how the scale-dependence of these concepts factors into a variety of practical challenges to developing adequate multiscale models of nanoscale systems.

---

## The Importance of Indicating How Researchers Maintain Trustworthiness During the Interview-based Research

*Chang Fang-Chi*

*Institute of Philosophy of Mind and Cognition, National Yang Ming Chao Tung University, Taiwan*

`fun.u4m4.y@nycu.edu.tw`

Typically, when writing research papers, people tend to present problems or difficulties that have already been solved while keeping unsolved ones quiet. However, in some cases, revealing more details about the unsolved difficulties is important. Especially, when the researched data can't be replicated or recollected. This paper aims to argue that it is important to reveal how researchers cope with a specific kind of difficulty in interview-based research: maintaining trustworthiness. Since 'trustworthiness' is crucial to interview and has a direct influence on data, it requests the interviewer to face a dilemma of how actively they should be to maintain trustworthiness while keeping a proper distance from participants for not participating in answering when collecting data.

To demonstrate my argument, I will first introduce Anna Alexandrova's (2017) examination of measures of well-being and her improved proposal to enlighten the issues of validation of measures; then, I will show that even if Alexandrova doesn't maintain the validation of interview directly, we would need to face with it inevitably if we try to put her proposal into practice. Additionally, I will illustrate what specific difficulties we might encounter based upon my experience of conducting an open-end interview with 40 patients about how they understand a measure of well-being, Spiritual Index of Well-Being.

This paper is divided into three parts: To begin with, I will summarize Alexandrova's examination of measures of well-being and her improved proposal to introduce the issue of construct validation; then, instead of discussing how to put her improved proposal into practice as a whole, I will only focus on the process of collecting data and showing that adding a procedure of interview would be the best way to practice her proposal. Finally, I will point out that there are at least three difficulties that interviewers might encounter. Without handling those difficulties properly, the research would collect data they want but in a way that violates the spirit that Alexanrova's proposal aims to preserve, the spirit of accommodating the perspective of the subjects.

The three difficulties are: First, the constantly changing cognitive status of participants, which is hard to identify their understanding exactly; second, the complexity of maintaining trustworthiness. Interviewers would need to be actively maintain's the trustworthiness with participants and be cautious not to participate in answering questions; third, the lack of restriction of how to use. Without proper guidance, it will make interviewers act with discretion easily when it comes to mass application: from collecting data to manipulating it for efficiency.

I will conclude that when the target of interview-based research is relevant to the issues of validation, it would be better off to indicate more details about how researchers maintain trustworthiness both successfully and unsuccessfully. By so doing, we would not only get a better understanding of the complexity of interviews but help us to rethink the concept of validation in practice.

**Realism for Realistic People: Tips for the Practitioners of PSP**

*Hasok Chang*
*University of Cambridge, United Kingdom*
hc372@cam.ac.uk

Building on the ideas and arguments developed in my forthcoming book, Realism for Realistic People, I offer some suggestions for the effective practice of 'philosophy of science in practice' (PSP).

The book proposes new pragmatist conceptions of knowledge, truth and reality, designed for better understanding and facilitation of practices. I focus on 'active knowledge', which consists in knowing how to do things. Active knowledge both enables and utilizes propositional knowledge. The quality of active knowledge consists in the 'operational coherence' of epistemic activities. I re-conceive the very notions of reality and truth in terms of operational coherence, thereby rendering them as concepts operative in actual practices: true propositions facilitate operationally coherent activities, which deal in real entities. Empirical truth is not a matter of correspondence to an inaccessible sort of 'mind-independent' reality; the correspondence achieved in real practices is among accessible realities that are 'mind-framed' yet not 'mind-controlled'. I call for 'activist realism' in the realistic spirit, in and about science: a commitment to do whatever we can actually do in order to improve knowledge. Following the imperative of progress naturally results in a plurality of systems of practice, each with its real entities and its true propositions.

With those ideas in the background, I will outline the following 10 methodological suggestions for scholars interested in PSP, which encompasses the philosophy of scientific practices, the philosophy of practical sciences, and the practical philosophy of science. (1) A new operationalism: always ask what it is that people do in practices that are linked to scientific and philosophical concepts. (2) Semantic moves: think about what it is that we actually mean in practice when we put philosophical notions to work. (3) Conceptual engineering: think about the usefulness of concepts in scientific, quotidian and philosophical practices, and find ways of enhancing their usefulness. (4) Aim-orientation: always understand and evaluate activities in relation to their purpose, both their inherent aims and their external functions. (5) Relentless empiricism: recognize no other sources of learning than experience, in science or in philosophy; apply this empiricism to the improvement of methodology and logic as well. (6) Epistemic iteration: recognizing that inquiry must always start from some accepted basis without ultimate warrant, identify and promote iterative developmental patterns, including empirically driven changes of aims. (7) Qualitative perspective: be suspicious of dichotomies, including the judgement of true/false; recognize even truth and reality as multi-dimensional qualities. (8) Iteration in philosophical inquiry: seek to improve your philosophical framework in light of the fruits of your philosophical inquiry. (9) Active principle of charity: in studying history or contemporary practices, identify and articulate maximally coherent systems of practice, and enhance their coherence where possible. (10) Proliferation: assist in the effort to conserve multiple coherent systems of practice, protecting them from extinction imposed by a monist regime; create new systems of practice where possible, and resurrect bygone coherent systems where plausible.

---

### What does it take to justify a research moratorium?

*Alexander Christian*
*Heinrich Heine Universität Düsseldorf, Germany*
christian@phil.hhu.de

Public debates about controversial research topics often evoke requests for the introduction of research moratoria. Examples include requests for moratoria on military research in public universities, research with human embryonic stem cells and human germline editing. In all of these cases, experts widely disagree on the justificatory status of particular moratoria. For instance, in the context of human germline editing, a permanent ban (Guttinger, 2018), a temporary ban (Lander et al., 2019) as well as a rejection of a research moratorium (König, 2019; Macintosh, 2019) are currently debated. Participants in this debate agree that a research moratorium could potentially violate constitutionally established rights to scientific freedom / academic freedom, but also has the potential to give policymakers time for the development of research policies that could address the moral issues of the corresponding research activities. There is, however, widespread disagreement on both, practical and theoretical aspects of research moratoria. For instance, it is controversial what an adequate moral framework for assessing the moral justifiability of specific research aims and research methods would be. The standard of evidence for conducting risk-benefit analysis and the role of the precautionary principle is notoriously controversial. Furthermore, the practical implementation of research moratoria is contentious: they could take the form of a legislative research ban or voluntary self-commitment of individual scientists, research institutions or funding agencies. Because of this widespread disagreement, many debates on specific proposed moratoria develop towards standoffs between risk averse and risk affine scholars. These situations are often only resolved when scientific or technical developments render certain reservations against a controversial research topic or research method obsolete or research scandals provoke a change of opinion with regard to the moral imperative of a research moratorium. The latter happened in the case of heritable genome editing between 2018 and 2021, when many moral and scientific experts joined the camp of supporters for the introduction of a temporary clinical moratorium with regard to germline editing, in particular CRISPR/Cas-based genome interventions. This change occurred after it became known that a rogue scientist had used CRISPR/Cas to induce a mutation (CCR5 $\delta$32) in several human embryos in order to bring about an immunity against HIV-infections (Baylis, 2019; Greely, 2019, 2021). So, debates about moratoria tend to be gridlocked until rendered pointless by technical developments or until an outright scandal stirs up sufficient indignation to cause a change of heart in the moratorium-opposing party. In this situation, an explication of the precise features of a well-argued justification for temporary or permanent research moratoria seems to be an important desideratum. In this talk, I explicate the criteria for a well-argued justification for a temporary or permanent research moratorium. Such a justification must (i) overcome autonomy-based, epistemic and political arguments for the freedom of science (Wilholt, 2010). It needs to (ii) include a negative risk-benefit analysis of the prospective research outcome, provide evidence for the absence of strategies for risk minimization or demonstrate a violation of fundamental moral rights in research processes. Finally, it has to (iii) result from a process of moral deliberation which includes informed experts as well as representatives of all potentially affected groups. I will then discuss whether these criteria are met with regard to requests for a permanent moratorium on CRISPR/Cas-based human germline editing.

References

Baylis, F. (2019). Altered inheritance: CRISPR and the ethics of human genome editing. Harvard University Press.

Christian, A. (2020). Gute wissenschaftliche Praxis. De Gruyter.

Greely, H. T. (2019). CRISPR'd babies: human germline genome editing in the 'He Jiankui affair'*. Journal of Law and the Biosciences, 6(1), 111–183.

Greely, H. T. (2021). CRISPR People - The Science and Ethics of editing Humans. The MIT Press.

Guttinger, S. (2018). Trust in Science: CRISPR–Cas9 and the Ban on Human Germline Editing. Science and Engineering Ethics, 24(4), 1077–1096.

Kitcher, P. (2001). Science, Truth, and Democracy. Oxford University Press.

Kitcher, P. (2011). Science in a Democratic Society. Prometheus Books.

König, H. (2019). Germline-editing moratorium — why we should resist it. Nature, 568(7753), 458–458.

Lander, E. S., Baylis, F., Zhang, F., Charpentier, E., Berg, P., Bourgain, C., Friedrich, B., Joung, J. K., Li, J., Liu, D., Naldini, L., Nie, J.-B., Qiu, R., Schoene-Seifert, B., Shao, F., Terry, S., Wei, W., & Winnacker, E.-L. (2019). Adopt a moratorium on heritable genome editing. Nature, 567(7747), 165–168.

Macintosh, K. L. (2019). Heritable Genome Editing and the Downsides of a Global Moratorium. The CRISPR Journal, 2(5), 272–279.

Wilholt, T. (2010). Scientific freedom: its grounds and their limitations. Studies in History and Philosophy of Science Part A, 41(2), 174–181.

---

## Transparency and the Remediation of Artifacts: Head motion in fMRI

*David Colaço*

*LMU Munich, Germany*

davidjcolaco@gmail.com

Scientists aim to remediate artifacts in their datasets, which result from confounding factors present in an experimental arrangement. While philosophers have taken interest in artifacts and confounds (Colaço 2018; Schickore 2019; Craver & Dan-Cohen 2021), parallel investigation has not been directed towards their remediation. However, if artifacts are not remediated, they undercut researchers' ability to use data as evidence.

In some cases, researchers modify their experimental arrangement to prevent or reduce artifacts. If these methods are not feasible, researchers may opt to correct for artifacts. While the correction of artifacts is an effective method in many cases, correction of an artifact can result in another artifact, where the data would not have the second artifact had researchers not corrected for the first. Why might this happen, and what does this consequence tell us about artifacts and our methods for remediating them?

In this talk, I answer these questions by exploring a case in which the remediation of an artifact caused this need: head motion in fMRI research. Recent studies highlight that the motion of subjects' heads causes "spurious but systematic correlation structures" in fMRI datasets (Power et al. 2012). However, correction of head motion made fMRI datasets susceptible to respiratory artifacts (Fair et al. 2020). Respiration is a known factor in fMRI arrangements, but the correction of head motion resulted in respiration confounding researchers' studies.

This case shows how correction transforms a dataset: based on an estimate, the correction makes the dataset appear more like how it would appear were there no artifact. However, these transformations may make another factor of the experimental arrangement leave its mark in data. To avoid this scenario, researchers must determine how both these factors and their remediation methods affect data. At the same time, correction may "black box" how it corrects, making possible an overcorrection that results in a novel artifact. This problem is salient when algorithms are used to correct data, which is present in the head motion case. Thus, a threat to structural transparency (Creel 2020) may result if researchers cannot determine how their method corrects for artifacts.

References

Colaço, D. (2018). Rip it up and start again: The rejection of a characterization of a phenomenon. Studies in History and Philosophy of Science Part A, 72, 32-40.

Craver, C., & Dan-Cohen, T. (2021). Experimental Artefacts. To appear in The British Journal for the Philosophy of Science.

Creel, K. A. (2020). Transparency in complex computational systems. Philosophy of Science, 87(4), 568-589. Fair, D. A., Miranda-Dominguez, O., Snyder, A. Z., Perrone, A., Earl, E. A., Van, A. N., ... & Klein, R. L. (2020). Correction of respiratory artifacts in MRI head motion estimates. Neuroimage, 208, 116400.

Power, J. D., Barnes, K. A., Snyder, A. Z., Schlaggar, B. L., & Petersen, S. E. (2012). Spurious but systematic correlations in functional connectivity MRI networks arise from subject motion. Neuroimage, 59(3), 2142-2154.

Schickore, J. (2019). The structure and function of experimental control in the life sciences. Philosophy of Science, 86(2), 203-218.

## A Trap for 'Engaged' Science? Lessons from the study of Interdisciplinarity

*Stephen Crowley and Michael O'Rourke*
*Boise State University, United States*
stephencrowley@boisestate.edu

One common story about the nature of interdisciplinary knowledge making (call it the MacGyver model) invites its users to see problem solving as i) central to interdisciplinarity and ii) involving making changes to the world rather than our understanding of it. This approach can lead to an unwarranted insulation from criticism of the models used in the problem solving. Since much contemporary science, in its desire to be of direct value to its community, is turning to interdisciplinary research there is reason to worry that it too will adopt a MacGyver style approach to this work and so fail to evaluate its models appropriately.

A standard model of interdisciplinary knowledge making (Repko and Szostak) identifies two key tasks. First, the knowledge makers must establish common ground between the disciplinary knowledge bases and then relevant parts of the disciplinary knowledge bases must be integrated. While it has been argued (O'Rourke et al) that integration can be understood as a single, parameterized activity an equally popular view (see Holbrook for a summary) is that every instance of integration is sui generis. What drives the sui generis view? We suggest this view derives from the combination of two common notions about 'problem solving'. First, that interdisciplinary knowledge making is driven by problem solving (again see Holbrook). Second, that problem solving itself involves the utilization of existing resources in novel ways to change the world in ways we find congenial (this is the MacGyver model of problem solving in honor of the TV character of that name who possessed an unequaled ability to utilize existing resources in novel ways!). If each instance of interdisciplinary integration involves the use of existing (conceptual) resources in novel ways it's easy to see why integration seems sui generis. Each instance of integration will begin with different sets of resources and be required to combine them in new ways. Each instance of integration is an adaptive response to a unique set of circumstances. To sum up, the popularity of the sui generis view of integration suggests the widespread acceptance of the MacGyver model of problem solving.

The MacGyver model directs our attention towards certain features of the situation and away from others. If you are MacGyvering you are paying attention to what elements of your situation might serve as resources and how they might be combined. You are not focussed on, because it is taken for granted, how the situation is framed in order to give rise to the 'problem' you are confronting. But sometimes it is exactly the framing that needs to be examined. Sometimes a 'problem' is resolved not by changing the world but by changing our understanding of the world. This is easy to overlook if you are using the MacGyver model. Putting aside the metaphor, our worry is that engaged science will adopt the MacGyver model and so overlook situations where real world problems are signs that our scientific models (aka framing) need to be revised rather than merely opportunities for clever conceptual engineering.

References

Repko, A. F. and Szostak, R. (2020) Interdisciplinary Research: Process and Theory (4th ed.) Sage Publications Inc.

Holbrook, J. B. (2013) What is interdisciplinary communication? Reflections on the very idea of disciplinary integration. Synthese 190(11), p. 1865-1879

O'Rourke, M., Crowley, S., Gonnerman, C. (2016). On the nature of cross-disciplinary integration: A philosophical framework. Studies in History and Philosophy of Biological and Biomedical Sciences 56: 62-70.

---

## An oak is an oak is an oak, or not?

*Vincent Cuypers[a] and Thomas Reydon[b]*
*[a]UHasselt, Belgium; [b]Leibniz Universität Hannover, Germany*
[a]vincent.cuypers@uhasselt.be; [b]reydon@ww.uni-hannover.de

For millennia, oaks (Fagaceae: Quercus) have been standard-bearers of northern hemisphere forests, playing important roles in the structuring of ecosystems, serving as objects of veneration in many cultures and mythologies, and being a valuable resource for many economic sectors. As such, our understanding and our use of oaks depend on the classification of individual trees and populations into species. When using an oak tree for a particular purpose, such as building houses or the making of furniture or wine, it is important to choose the right kind of tree. In such practical contexts, required quality and properties of the wood that is used, determined by the kind of tree, in interaction with the ecological context in which the tree grows, are crucial.

However, the classification of oaks into species is problematic. For example, although well-established as species in everyday contexts, the taxonomic status of pedunculate oaks (Q. robur) and sessile oaks (Q. petraea) entails difficulties, mostly because of ubiquitous hybridisation between the two groups. This puts both species in violation of the popular Biological Species Concept, and makes distinguishing between them in practice difficult, because of the blurring of phenotypic boundaries. This uncertainty has led to an accumulation of literature exploring different ways of telling both groups apart, relying for example on morphological traits and multivariate integrations thereof, a variety of molecular markers, and aspects of wood anatomy. These have met varying degrees of success, but haven't taken the sting out of the more fundamental classificatory question.

Our paper aims to clarify the classificatory tensions surrounding oaks, by showing how the classification of pedunculate and sessile oaks is dealt with in the interaction between scientific taxonomy and practical applications. We will review the various considerations, both theoretical and practical, that surround the construction and use of oak groupings, and show how various epistemic and non-epistemic aims seem to interfere. Different sets of criteria yield different groupings and highlight other similarities and dissimilarities between individuals and between groups. Then, we will use our findings to inform the general philosophical discussion on the role of aims and values, both epistemic and non-epistemic, in (biological) classification. More particularly, we will argue that the interference of different aims, epistemic and practical, supports a position of taxonomic pluralism: perhaps different cross-cutting classifications ought to be used in different contexts. We will contrast this plurality of groupings with the longstanding aim of biological taxonomy to provide one single classification, and explore how taxonomic pluralism can be a workable approach for both science and practical applications.

---

## Revisiting Debates on Scientific Dissent and Diversity in the light of the Practice of Journal Peer Review

*O. Çağlar Dede*

*Vrije Universiteit Amsterdam, Netherlands*

dedecaglar@gmail.com

An important epistemic function of scientific peer review is to facilitate learning from one's peers and revising one's assumptions, observations, and results based on the criticism of researchers from diverse backgrounds. Accordingly, many philosophers and social epistemologists, most notably Helen Longino (1990, 2002), have considered peer reviewing as a leading example of a scientific practice where diversity and critical exchanges among peers successfully improve epistemic outcomes. In this paper, I offer a closer look at the practice of the pre-publication journal peer review to contribute to the debates in the social epistemology of science regarding the epistemic value of diversity and interactions in knowledge-producing communities.

The available empirical literature on journal peer review suggests that the practice of scientific peer review is vulnerable to a number of biases curbing its potential epistemic benefits (e.g., Lee et al. 2013). Some of these biases are related to the social and demographic characteristics of the reviewers and the reviewees such as their gender or institutional affiliation. Some of them are associated with the content of the submissions. For instance, novel and innovative or interdisciplinary submissions tend to receive stricter evaluations; positive results are more likely to be published than negative or inconclusive results (also known as publication bias).

These kinds of results suggest that the practice of peer review sometimes fails to fulfill Helen Longino's principles such as "tempered intellectual authority" or "uptake" that are key to realizing the expected benefits of dissent and diversity in science. In this sense, the empirical results about the practice of journal peer review could be interpreted in support of the general philosophical arguments that question the epistemic value and contribution of the critical interactions in mediating the positive effects of diversity in science (e.g., Solomon 2001, Zollmann 2010, Steel et al. 2021). However, I propose and defend an alternative interpretation of the failures in the practice of peer review in favor of Helen Longino's account. Specifically, I argue that the reported biases in peer review pertain to the behaviors of reviewers and reviewees as individual epistemic agents, whereas the proper target of Longino's norms are not individual agents' behavior but community-level institutions and practices in which critical interactions between individual agents take place. This argument supports and reinforces Jukola's (2017) analysis of bias in peer review and Longino's recent qualifications regarding the social character of her framework (Longino 2021).

In the light of this alternative interpretation, I propose that the empirical results about the failures in peer review motivate further research on alternative institutions and settings of peer review which adhere to Longino's norms and are therefore expected to foster the epistemic benefits of critical interactions and diversity in science. To this end, I review and evaluate some of the recent developments in the practice of scientific peer review, such as the proposal of Registered Reports (see, for instance, Chambers and Tzavella 2021).

## Exploring dark matter with stellar streams

*Siska De Baerdemaeker*
*Stockholm University, Sweden*
siska.debaerdemaeker@philosophy.su.se

Dark matter constitutes approximately 26% of the current energy density of the universe, plays a central role in large-scale structure formation, and could be an important lead to physics beyond the standard model of particle physics. Yet, aside from its gravitational effects, very little is known about dark matter's fundamental structure and the space of possibilities remains vast. Two types of experiments are underway to make progress on dark matter's fundamental structure: experiments that aim to positively confirm a dark matter candidate, and cosmological and astrophysical observations that aim to put tighter constraints on the dark matter space of possibilities.

This paper investigates the epistemological underpinnings of one recent set of such observations: the use of stellar streams to map out the substructure of the Milky Way halo and thereby further constrain possible dark matter candidates (Banik et al. 2021; De Boer, Erkal, and Gieles 2020; Bonaca et al. 2019). Stellar streams are clusters of stars orbiting a galaxy that have been torn apart and stretched out due to tidal effects. They move through the presumed dark matter halo of that galaxy, which means that they could encounter substructure in that halo. Any encounters with substructure would affect the density profile of the stream. Observations of density profiles of stellar streams thus help to map out the dark substructure in the Milky Way halo, which, in turn, could lead to constraints on the space of possibilities for dark matter's fundamental structure on the one hand, and on the range of possible solutions to the small-scale challenges in cosmology on the other.

I argue three related points about the stellar streams. First, I show that stellar streams are 'multi-purpose': the same observations are used to constrain multiple worldly targets at once. Second, I submit that stellar stream observations fulfill a dual role qua observations: they are exploratory insofar as they are used to map out the substructure of the Milky Way halo, but they are hypothesis-driven insofar as they are used to investigate how stellar streams are affected by that substructure. Here, I draw on the existing literature on exploratory experiments (Burian 1997; Colaço 2018; Elliott 2007; Franklin 2005; Karaca 2017; O'Malley 2007; Steinle 1997; Waters 2007) and extend it to the current case. Exploratory experiments are commonly defined in contrast with confirmatory or hypothesis-driven experiments: they are not aimed at testing any specific local theory about the target. Although exploratory experiments are not aimed at theory testing, they do rely on background theory as guidance (Franklin 2005; Karaca 2017). Building on (Colaço 2018), I show that the stellar streams case reveals that sometimes observations and experiments can take on a dual role as both exploratory and hypothesis-driven, but with regards to different targets: dark matter haloes, and stellar streams, respectively. Finally, I argue that, due to the peculiar nature of the dark matter problem, the hypothesis-driven role can only be fulfilled through an explicit reliance on eliminative induction.

References

Banik, Nilanjan et al. 2021. "Novel Constraints on the Particle Nature of Dark Matter from Stellar Streams." Journal of Cosmology and Astroparticle Physics 2021(10): 1–5.

De Boer, T. J.L., D. Erkal, and M. Gieles. 2020. "A Closer Look at the Spur, Blob, Wiggle, and Gaps in GD-1." Monthly Notices of the Royal Astronomical Society 494(4): 5315–32.

Bonaca, Ana, David W. Hogg, Adrian M. Price-Whelan, and Charlie Conroy. 2019. "The Spur and the Gap in GD-1: Dynamical Evidence for a Dark Substructure in the Milky Way Halo." The Astrophysical Journal 880(1): 38.

Burian, Richard M. 1997. "Exploratory Experimentation and the Role of Histochemical Techniques in the Work of Jean Brachet , 1938-1952." History and Philosophy of the Life Sciences 19(1): 27–45.

Colaço, David. 2018. "Rethinking the Role of Theory in Exploratory Experimentation." Biology & Philosophy 33(5): 1–17.

Elliott, Kevin C. 2007. "Varieties of Exploratory Experimentation in Nanotoxicology." History and Philosophy of the Life Sciences 29(3): 313–36.

Franklin, L. R. 2005. "Exploratory Experiments." Philosophy of Science 72(5): 888–99.

Karaca, Koray. 2017. "A Case Study in Experimental Exploration: Exploratory Data Selection at the Large Hadron Collider." Synthese 194(2): 333–54.

O'Malley, Maureen A. 2007. "Exploratory Experimentation and Scientific Practice: Metagenomics and the Proteorhodopsin Case." History and Philosophy of the Life Sciences 29(3): 337–60.

Steinle, Friedrich. 1997. "Entering New Fields: Exploratory Use of Experimentation." Philosophy of Science 64: S65–74.

Waters, C Kenneth. 2007. "The Nature and Context of Exploratory Experimentation: An Introduction to Three Case Studies of Exploratory Research." History and Philosophy of the Life Sciences 29(3): 275–84.

# Grünbaum and Salmon at Work: Transforming Philosophy of Science into a Discipline

*Fons Dewulf*

*KU Leuven, Belgium*

fons.dewulf@outlook.com

Based on new archival research, I argue that the conception of philosophy of science as a professional subdiscipline of philosophy only emerged in the second half of the 1960s, as a result of the institutional renewal of the Philosophy of Science Association (PSA) by Adolf Grünbaum and Wesley Salmon. Their conception of philosophy of science as a profession also entailed the demise of competing non-professional conceptions. In 1955, Harry Alpert and Raymond Seeger, both directors at the National Science Foundation (NSF), organized a conference on the History, Philosophy and Sociology of Science to discuss the question whether the NSF had reason to fund research in those areas. During the conference, both Philipp Frank, representing the Institute for the Unity of Science (IUS), and Henry Margenau, representing the PSA, defended that the NSF should fund research in the study of science broadly conceived, including both logical, historical and cultural aspects of the scientific enterprise, to the benefit of scientific education both in specialized scientific programs and society at large. At the time, neither Frank nor Margenau conceived philosophy of science as a field of inquiry, distinct from the history and sociology of science or distinct from the scientific enterprise itself, nor did they conceive their own institutions (IUS and PSA) as representatives of philosophy of science as a discipline. Only a decade later, however, the situation had drastically changed. In 1966, Margenau complained to Richard Rudner, the editor of Philosophy of Science, that the PSA had been taken over by "a small, self-centered group" and that the crucial contact with the scientific world had been neglected. In a similar vein, Raymond Seeger, lamented to Adolf Grünbaum in 1968 that the professional success of philosophers of science had resulted in a steadily decreasing interaction between philosophers and scientists. After Frank's death in 1966, the IUS imploded and its remaining funds were transferred to the PSA, which was being transformed into an institute that represented the philosophy of science as a profession and as a specialized domain of inquiry within academic, American philosophy. I argue that this dramatic reversal crucially revolves around the joint activity of Wesley Salmon and Adolf Grünbaum between 1966 and 1970. Based on an extensive analysis of their correspondence, I discuss how they sought to take over either the PSA or the IUS in order to create a professional institution that could support the growing job market for American philosophers of science, organize a dedicated annual conference series for them and represent their interests vis-à-vis the NSF and other official institutions, like the International Union of the History and Philosophy of Science. After Grünbaum and Salmon succeeded in transforming the PSA in this way between 1966 and 1970, none of the original supporters of the Unity of Science movement, like Carl Hempel, Ernst Nagel, Herbert Feigl or Rudolf Carnap, saw any remaining institutional relevance for the IUS, or its original ideals. The Unity of Science movement thus died with the rise of professional philosophy of science.

---

## Understanding Evolutionary Novelty and Co-Option in light of Character Identity Mechanisms

*James DiFrisco, Gunter Wagner and Alan Love*
*KU Leuven, Belgium*
james.difrisco@gmail.com

A central topic in evo-devo research is the origin of novel characters. Despite progress on understanding how developmental mechanisms generate patterns of diversity in the history of life, the problem of novelty continues to pose a challenge. This is partly because the problem is not only empirical, but is also conceptual: in order to know what counts as a novel character, one needs criteria for deciding what counts as "the same" character. A major difficulty with the latter requirement is that characters can be similar or homologous at one level of organization but not at others. For example, if the same gene-regulatory mechanism involved in the development of character X is re-used to build a morphologically different character Y—a widespread phenomenon commonly known as "co-option"—can Y be a novelty, or is it instead homologous with X? What is needed is not only an account of character identity, but one that provides correspondence principles showing when shared identity at one level implies identity at another level, and when not.

This talk argues that research on evolutionary novelty and the associated phenomenon of co-option can be reframed fruitfully by: (1) specifying a conceptual model of mechanisms that underwrite character identity, and (2) providing a richer and more empirically precise notion of co-option that clarifies the implications of co-option for identities at different levels. For

(1), we utilize the model of Character Identity Mechanisms or "ChIMs" (DiFrisco, Love, and Wagner; Biol Philos, 2020), which hypothesizes that morphological character identity is grounded in classes of mechanisms specific to cell types, tissue types, and organs, and united by a common causal profile. For (2), we propose the following principles: when an underlying mechanism is reused from one character to another, if that mechanism is a ChIM, then this reuse constitutes a duplication of a serial homologue. By contrast, if the mechanism is not a ChIM, then the reuse is co-option and possibly involves a novelty. These principles are lacking from common appeals to the notion of "deep homology," which does not have any specific implications about the meaning of gene sharing for character identity at higher levels of organization.

Understanding novelty and co-option through the frame of ChIMs not only aids in conceptual clarification, but also productively feeds back on experimental investigations by identifying which kinds of experiments and data are needed to explain the origination of novelties via co-option. To illustrate how this can work in practice, we apply the proposed reframing to debates over the alleged serial homology of treehopper helmets and insect wings (Prud'homme et al, Nature; 2011). We show how the co-option of components from the "wing GRN" in helmet development does not constitute re-use of a ChIM and is thus insufficient for inferring serial homology, suggesting that the treehopper helmet is an evolutionary novelty.

---

## Release the Kraken? Well-Controlled and Dangerous Speculation in Geohistory

*Max Dresow*

*University of Minnesota, Twin Cities, United States*

dreso004@umn.edu

In 2011, two paleontologists submitted an abstract to the annual meeting of the Geological Society of America. It proposed that an assemblage of ichthyosaur skeletons in Nevada represents the refuse of an enormous cephalopod, or "kraken," with an estimated length of 30 meters. It also claimed that the kraken possessed extraordinary intelligence on the grounds that certain of the skeletons are arranged in a way that resembles a (modern) cephalopod's sucker discs. In the authors' words, "the tessellated vertebral disc pavement may represent the earliest known self-portrait."

These suggestions are almost certainly wrong. As critics delighted to observe, evidence for the kraken is entirely circumstantial, and the interpretation of the bonebed as a refuse heap flouts the principle of parsimony.

But so what? It is no sin for a scientific hypothesis to be wrong. Nor is it necessarily wicked to sin against parsimony. It is true that simplicity provides a convenient standard for evaluating explanatory claims, but it is hardly unprecedented for more complicated hypotheses to win out over simpler ones. Even a seemingly implausible hypothesis may have its virtues. In a famous article, the geologist William Morris Davis defended the value of "outrageous" hypotheses on epistemic grounds (Davis 1926). His rationale was that, while speculative hypotheses will often be wrong, they are sometimes needed to advance knowledge in an area. Adrian Currie has recently made a similar proposal: "Especially when the going gets tough…historical science should be wild, messy and creative [i.e., speculative]" (Currie 2018, 291; but see Turner 2019 and the response in Currie 2019).

This talk is about wild, messy and creative speculation in geohistory. Specifically, it is about what I term dangerous speculation, and the circumstances under which it is likely to be well received. Dangerous speculation is speculation that departs from an ideal of "well controlled" speculation in one or more of several ways. These departures correspond to familiar epistemic sins, and are individually sufficient to render a hypothesis epistemically suspect. But an epistemically suspect hypothesis may still be seen as viable under special conditions. This paper explores these conditions using case studies from geohistory. All are outrageous and probably wrong; in addition to the Triassic kraken, I will consider Adolf Seilacher's interpretation of the Ediacaran biota and the so-called "Nemesis" or "Death Star" hypothesis (Seilacher 1989; Raup 1986). However, despite having certain features in common, these hypotheses prompted highly divergent reactions from the relevant communities.

My question is why? What accounts for the relatively enthusiastic reception of Seilacher's hypothesis, the more complicated reception of Nemesis, and the heckling dismissal of the Triassic kraken? And what epistemic lessons (if any) can be extracted from the comparison? This talk attempts to answer these questions by drawing on the conceptual resources mentioned above. One important payoff is an enriched vocabulary for analyzing "dangerous" scientific speculation, as well as tools for scrutinizing its uptake in scientific practice. While my proximate interest is geohistory, I argue that these tools can inform broader discussions of speculation in the sciences.

References

Currie, A.M. (2018). Rock, Bone and Ruin: An Optimist's Guide to the Historical Sciences. Cambridge (MA): The MIT Press.

Currie, A.M. (2019). "Epistemic optimism, speculation and the historical sciences." Philosophy, Theory and Practice in Biology 11.

Davis, W.M. (1926). "The value of outrageous geological hypotheses." Science 1636:463–8.

Raup, D. (1986). The Nemesis Affair: A Story of the Death of Dinosaurs and the Ways of Science. New York: W.W. Norton & Co.

Seilacher, A. (1989). "Vendozoa: organismic construction in the Proterozoic biosphere." Lethaia 22:229–239.

Turner, D. (2019). "Speculation in the historical sciences." Philosophy, Theory and Practice in Biology 11.

---

## The found science of meat alternatives - How food biotech is creating new meat concepts

*Sophia Efstathiou*

*NTNU, Norway*

sophia.efstathiou@ntnu.no

This paper explores how emerging food biotech is transforming concepts of meat. One of my recent culinary fascinations comes in the form of Beyond Sausage– a sausage made of plants. With a capped super-cow as its logo, Beyond Meat claim: "We started with simple questions. Why do you need an animal to create meat? Why can't you build meat directly from plants? That's our company's mission. We hope our plant-based meats allow you and your family to eat more, not less, of the traditional dishes you love. Together, we can truly bring exciting change to the plate -and beyond. GO BEYOND!" (Beyond Sausage packaging.)

Philosophy of science has been increasingly engaging with applied science and technoscience fields, including agricultural and food research. This is because scientific work itself is changing, responding to calls for societal impact, addressing grand societal challenges, and achieving sustainable development goals. One key field of action -scientific and societal- is transforming the global food system. In the last fifty years, per capita meat consumption has, on average, across the world, doubled, while the earth's population itself doubled (Weis 2013). This quadrupling of meat consumption has relied on the technological intensification of livestock production and of systems of provision: leaving a significant ecological 'hoofprint' on the planet's air, lands and waters, and on other life, or biodiversity. Reducing the consumption of intensively farmed animals is thus key for reducing climate emissions. This paper explores how technoscientific work on 'alternative proteins' is changing ideas about meat as exclusively animal-based.

I have previously argued that everyday ideas can get transfigured into new scientific concepts, by being founded in scientific knowledge-making practices. These new founded concepts often keep their everyday names but work as scientific ideas sustaining and generating more science. For example, when economists measure ‘wellbeing’, they are not using some everyday idea of wellbeing to do this, but found a common idea in an epistemic-metaphysical-social context of economics, and articulate it as a new, founded, economics concept that they can operationalise and measure (Efstathiou 2016). But can founded concepts jump back to everyday life and how? This talk explores how founded concepts travel back to everyday life by examining meat and meat concepts.

I propose that this type of creative meaning-making is happening with ideas of ‘meat’ (and ‘burger’, ‘mince’, etc.) within food science and technology practices. Companies like Impossible Burger, Beyond Meat or, cultured meat company, GOOD Meat are founding everyday ideas of meat into novel plant- or cell-based food biotechnology contexts creating new founded, meat concepts. They do this through activities ranging from imitating the molecular properties of (animal-based) meat or growing tissue in a lab, to vision-statements and marketing matching the “good stuffs” of meat (Sexton 2016). Though the result here is not, or not only, found science but found meat. This paper shows how meat is founded in science but also re-entering the culinary practices of everyday life, bringing ‘meat’ back to the plate in new forms.

---

## Taking the social value of clinical trials into account: from unbiasedness to fitness for purpose

*Michaela Egli*

*University of Geneva, Switzerland*

michaela.egli@unige.ch

Medical knowledge is constantly being used to make practical decisions. Doctors decide between treatment options for their patients and regulators evaluate therapies for market approval. However, a seminal 1967 paper by Schwartz and Lellouch points out that many clinical trial designs are not fit for their intended purpose. To help researchers tailor the design of a study to practical goals, Schwartz and Lellouch introduced the “pragmatic clinical trial” into the clinical research methodology. These are randomised trials that examine the effectiveness of treatment strategies under non-idealised conditions of routine care. Some fifty years later, Schwartz and Lellouch’s concerns are increasingly finding their way into scientific practice and regulatory policy – a trend that has been called the "rise of pragmatism" (Patsopoulos 2011). A milestone in this development is the forthcoming revised version of the International Council of Harmonization guideline "General considerations for clinical studies (ICH E8)", which proposes to describe the quality of a clinical trial as its fitness for purpose. Philosophers of medicine recognise the problem that findings from ideal randomised trials can hardly be applied to real patients – often discussed as the problem of external validity. Some have therefore even fundamentally questioned randomised trials as an appropriate epistemic gold standard. In the debate on external validity, pragmatic clinical trials have not gone unnoticed and have been mentioned several times as a potential solution to the problem (Fuller 2019; Howick et al. 2013; Cartwright 2017; La Caze 2017). However, these studies have also been criticised for a lack of epistemic rigour (Navarro et al. 2021) and in turn defended on the basis that their added social value justifies a potential lack of scientific rigour (Borgerson 2013).

In my contribution, I present the case study of the Relvar Ellipta inhaler developed by GlaxoSmithKline to analyse the knowledge gain of a pragmatic clinical trial on the one hand, and to highlight the difficulties that standard clinical trial epistemology encounters when trying to explain the value of this knowledge on the other. I conclude with briefly presenting an alternative. My analysis of the case study compares a pragmatic study with an ideal study of the Relvar Ellipta inhaler. I focus on how the pragmatic trial could leverage the practical advantage of a less burdensome administration of the Relvar Ellipta inhaler and factor it into the effect size. Since regular adherence to therapy is known to be a key challenge for successful treatment of patients with chronic diseases, I argue that our epistemology should be able to recognise evidence already accounting for such a difficulty as potentially high-value evidence for decision-making. I then show that the usual epistemological framework, which focuses on the unbiasedness of evidence, however, has difficulty recognising the value of this experiment and instead describes it as a highly biased, low-quality experiment, which is actively discouraged from being included in decision-making. I conclude by briefly presenting an alternative that includes the idea of quality as fitness for purpose and accordingly thinks of basic methodological concepts such as validity or bias as purpose-relative.

# 'We do not speak about emissions responsibility': Carbon accounting and the value-ladenness of visual epistemic representation

*Ahmad Elabbar*
*Department of History and Philosophy of Science, University of Cambridge, United Kingdom*
ae423@cam.ac.uk

In recent years, philosophers have explored the various roles of non-epistemic values in science. The results of these investigations challenge traditional ideals of objectivity in science and force a fundamental rethink of the social responsibilities of scientists. Yet, within this rich scholarship, the role played by non-epistemic values in the construction of figures, graphs, maps, and other visual epistemic representations remains largely unexplored, despite the prominent role of visual representation in scientific practice, especially in communication with non-expert audiences and policymakers. My aim is to work towards closing this gap in the literature. I do so by drawing on the adequacy for purpose view of epistemic representation, which recognises and draws attention to the fact that all representations are partial: they abstract from the target system they represent, capturing some features to the neglect of others, and thus adequacy for purpose, rather than truth, is the proper standard of success for representation. As such, the construction of visual representations involves a host of epistemically unforced choices of salience, including which features of the target system to represent, what time scales to cover, how to categorise and aggregate data, and which syntactic features to foreground and make aesthetically attractive. These salience choices raise, in turn, a mixture of epistemic and ethical risks, as salience making can induce audiences to draw false inferences about the target system or draw attention to specific features of the target system while backgrounding others, which may in turn lend credibility to certain policy interventions over others. I argue, therefore, that visual epistemic representations are never neutral, and that their responsible construction requires attention to non-epistemic values. Taking this analysis beyond the theoretical level, I analyse a controversy over carbon accounting in the Intergovernmental Panel on Climate Change (IPCC). In its 5th Assessment Report (2014), the IPCC presented in the summary for policymakers a figure that showed a gap between countries' production emissions and their consumption emissions, a gap that suggests that rich countries have reduced their carbon footprints by outsourcing production to poorer countries. However, the figure was ultimately deleted from the summary, along with four other carbon accounting figures, because governments could not agree on a policy-neutral method for representing global emissions. Although the removal of these figures was described by authors and media outlets as governments failing to accept hard truths, I offer a more complex analysis that draws on the value-ladenness of visual epistemic representation. I demonstrate, through archival analysis, that the authors had in fact developed nine iterations of the figure comparing consumption and production emissions, each time testing different representational choices in search for neutrality. The final figure removed by policymakers was the last in a lineage of representations that all fail to satisfy what I contend is an unachievable mandate: to develop a policy-neutral representation of carbon emissions. The case study serves both to strengthen the theoretical analysis of value-ladenness in visual epistemic representation and highlights the practical cost of holding scientific advisors to the value-free ideal.

---

## Naturalness and the Heuristic Role of Scientific Principles

*Enno Fischer*

*Interdisziplinäres Zentrum für Wissenschafts- und Technikforschung, Bergische Universität Wuppertal, Germany*

enno-fischer@gmx.de

The naturalness principle roughly demands that a theory should not involve independent parameters that are finely tuned. This principle was employed heavily over the last 40 years by theoretical physicists as a guideline for developing theories of beyond the Standard Model physics (BSM). However, since experiments at the Large Hadron Collider (LHC) have not found conclusive signs for new physics, the significance of naturalness arguments has been questioned and it has been suggested that high-energy physics has reached the "dawn of the post-naturalness era."

In this talk I argue that there is a mode of justification for scientific principles that is forward looking. The forward-looking justification derives from coherence with ideas that the principle gives rise to. This form of justification differs from more traditional forms of justification that relate a principle to scientific claims that have been secured already. The naturalness principle has experienced this kind of justification because it has given rise to a number of promising but yet unconfirmed BSM theories.

More specifically, the assessment of naturalness had differed if major proposals had failed to solve the naturalness problem already at the theoretical stage. Here I distinguish two kinds of cases. First, there are theories like Technicolor or theories involving extra dimensions that are natural by construction. One can imagine various ways in which such a construction could have failed. For example, the theories could have turned out to be I conflict with major principles of theory building, or to be in conflict with experimental constraints available at the time of their development. Second, in the case of supersymmetry the support is even stronger because supersymmetry was developed independently of naturalness arguments. While it has been argued that such forms of unexpected coherence can support theories, I argue that this mechanism extends to scientific principles such as naturalness.

I argue that an explanation of the current shift in attitude towards naturalness is available if we acknowledge that the naturalness principle has experienced forward-looking justification. Before the discovery of the Higgs the potential coherence between major BSM proposals and the naturalness principle had led to an increasing degree of credibility of the principle. Moreover, the naturalness problem was a relevant problem to the Standard Model (SM) because major BSM proposals had the potential to solve it. Once the experiments turned out to show no signs of new physics, doubts started to rise with regard to the naturalness principle. The options of coherence between the principle and promising BSM approaches have become more and more limited. Moreover, the relevance of the naturalness problem of the SM appears to be deflated since major BSM proposals appear unable to solve it.

The naturalness principle is sometimes described as a guiding principle or an important heuristic in high-energy physics. My account provides an approach to explaining why naturalness has gained this status: it accrued this status in virtue of its coherence with promising BSM proposals that it has given rise to.

---

## Exploratory in vitro models in toxicology

*Grant Fisher*

*Korea Advanced Istitutue of Science and Technology, South Korea*

fisher@kaist.ac.kr

Recent developments in toxicology have resulted in the use of novel model systems in toxicological risk assessment including stem cell cultures and organoids. These developments are motivated by commercial, ethical, as well as epistemological reasons. However, they are sometimes regarded as a threat to established standards of evidence, institutional arrangements, and the extent to which they might provide the opportunity for reform in animal experimentation is contentious (Fisher 2021). This paper addresses what we can expect from in vitro stem cell cultures in toxicology and argues that they perform an important exploratory function by probing problems of extrapolation from non-human animals to humans in toxicological risk assessments. Nevertheless, the epistemological as well ethical problems associated with animal experimentation will not be addressed in the near term by these developments. At first blush stem cell toxicology models (SCT) appear to be a form of in vitro representation – analogous to what Marcel Weber (2014) calls "in vivo representations". However, SCT are exploratory models in the sense that they operate as models for. Drawing on Evelyn Fox Keller's (2000) distinction between models of and models for, Emanuele Ratti (2018) has recently argued that in molecular biology, scientists can adopt alternative cognitive dispositions towards the same model – as accurate representation of their targets (models of) or as a means to enable new forms of experimental intervention by redeployment (models for). Similarly, SCT models are models for because they can be thought of as the "redeployment" of stem cell cultures from therapeutic stem cell biology to toxicological risk assessment thereby enabling new forms of experimental intervention. But they cannot be taken to be more accurate representations of their targets even if they make use of human stem cells as surrogate systems for human toxicological studies. SCT are problematic if regarded simply as model of due in part to their dependence for validation on data from model organisms. It will be argued instead that SCT are in vitro models for and as such perform an important exploratory function within and beyond toxicology. For example, practitioners refer to the ways in which models in biomedicine and toxicology can be used to "mediate" between model organisms and humans (Duronio et al. 2017). In vitro models for provide new means of intervention somewhere between what LaFollette and Shanks (1995) call "hypothetical analogue models" and "causal analogue models".

References

Duronio, R.J. O'Farrell, P.H., Sluder, G., & Su, T.T., "Sophisticated Lessons from Simple Organisms: Appreciating the Value of Curiosity-driven Research", Disease Models and Mechanisms, 10 (2017), 1381-1389.

Fisher, G. "Stem Cell Toxicology: Ethical and Epistemic Constraints on In Vitro Models", HYLE – International Journal for Philosophy of Chemistry 27 (2021), 67-89.

Fisher, G, Gelfert, A., & Steinle, F. (Eds.), "Exploratory Models and Exploratory Modelling in Science", Perspectives on Science 29 (4) (July-August 2021).

Keller, E.F.:, "Models of and Models for: Theory and Practice in Contemporary Biology", Philosophy of Science, 67, Supplement. Proceedings of the 1998 Biennial Meetings of the Philosophy of Science Association. Part II: Symposia Papers (2000), S72-S86.

LaFollette H., & Shanks, N., "Two Models of Models in Biomedical Research", The Philosophical Quarterly, 45 (1995), 141-160.

Ratti, E., "'Models of' and 'Models for': On the Relation between Mechanistic Models and Experimental Strategies in Molecular Biology", British Journal for the Philosophy of Science, 71 (2018), 773-797.

Weber, M., "Experimental Modeling in Biology: In Vivo Representations and Stand-Ins as Modeling Strategies", Philosophy of Science, 81(5) (2014), 756-769.

---

## What is the point of models, diagrams, and idealisations for scientific understanding? A zetetic approach

*Leonardo Flamini*

*University of Pavia - University of Zurich, Italy*

leonardo.flamini3@gmail.com

Philosophers of science and epistemologists significantly agree in considering understanding the aim of science and its successful product (De Regt, 2017; Elgin, 1996, 2007, 2017; Grimm, 2008; Kelp, 2014, 2021; Kosso, 2007; Lipton, 2004; Potochnik, 2017; Salmon, 1998; Strevens, 2006). Within this perspective, scientists aim to understand the world and its phenomena when engaged in their inquiries. Furthermore, many philosophers have underlined the importance of models, diagrams, and idealisations to produce and attain the understanding science strives for (De Regt, 2015, 2017; Elgin, 1996, 2007, 2017; Riggs, 2003; Potochnik, 2017). However, some questions arise naturally: Why do models, diagrams, and idealisations help scientists understand phenomena? And what is the point of using them to attain scientific understanding? In this talk, I propose answering these questions using a zetetic approach (Friedman, 2020, forthcoming), i.e., considering how inquiries work.

The first introductory part of the talk notes that the philosophical literature about understanding distinguishes two senses of this concept. On the one hand, understanding is what I presented above and what epistemologists and philosophers of science call scientific understanding. On the other, understanding is taken to be a mental state or an ability that is related to conceptual matters and how we represent things and their meaningful structures (Bengson, 2015; Horvath, 2020; B. Jackson & B. Jackson, 2012; Soysal, 2018). Namely, it is what can be called conceptual understanding. Finally, I remark that how these two kinds of understanding relate is a neglected argument in epistemology and philosophy of science.

The second part aims at filling in the gap between these two concepts of understanding by claiming that the following conditional holds (C): If scientific understanding is the aim of scientific inquiries and represents their successful product, then it must imply high levels of conceptual understanding of the phenomena being investigated. I start by illustrating how inquiries come in degrees: Some inquirers can investigate more a given phenomenon and conduct more demanding inquiries than others. Secondly, I show that inquiries that do not aim at any conceptual understanding of the phenomena under investigation cannot exist: Any inquiring effort naturally aims to produce a conceptual grasp of what is inquired. Thirdly, I highlight that the more one investigates a phenomenon, the more one aims at its conceptual penetration. Finally, based on these three points, I identify scientific investigation as a high-level inquiry that naturally aims at a high level of conceptual understanding, and I conclude that the conditional (C) is correct.

The final part of the talk draws some results on diagrams, models, and idealisations based on the picture of scientific inquiry provided in the second part. I claim that all the aforementioned items have a functional role in scientific investigations, i.e., they are tools that help scientists achieve the high conceptual penetration of the phenomena their inquiries aims at. Moreover, if (C) is true, scientists need these tools because they can provide the kind of conceptual representation of the phenomena that is necessary to understand them scientifically. Therefore, I conclude that the point of using diagrams, models, and idealisations is that they enable scientists to achieve the conceptual grasp of the phenomena their inquiries demand in order to reach their aim, i.e., scientific understanding.

References

B. Jackson, M. & B. Jackson, B. (2012). Understanding and Philosophical Methodology. Philosophical Studies, 161(2): 185-205.

Bengson, J. (2015). A Noetic Theory of Understanding and Intuition as Sense-Maker. Inquiry: An Interdisciplinary Journal of Philosophy, 58(7-8): 633-668.

De Regt, H. W. (2015). Scientific Understanding: Truth or Dare? Synthese, 192(12): 3781-3797.

De Regt, H. W. (2017). Understanding Scientific Understanding. New York: Oxford University Press.

Elgin, C. Z. (1996). Considered Judgment. Princeton: Princeton University Press.

Elgin, C. Z. (2007). Understanding and the Facts. Philosophical Studies, 132(1): 33-42.

Elgin, C. Z. (2017). True Enough. Cambridge: MIT Press.

Friedman, J. (forthcoming). Zetetic Epistemology. In B. Reed & K. Flowerree (eds.), Towards an Expansive Epistemology: Norms, Action, and the Social Sphere. Routledge.

Grimm, S. R. (2008). Explanatory Inquiry and the Need for Explanation. British Journal for the Philosophy of Science, 59(3): 481-497.

Horvath, J. (2020). Understanding as a Source of Justification. Mind, 129(514): 509-534.

Kelp, C. (2014). Knowledge, Understanding, and Virtue. In A. Fairweather (ed.), Virtue Epistemology Naturalized: Bridges Between Virtue Epistemology and Philosophy of Science (pp. 347-360). Synthese Library.

Kelp, C. (2021). Inquiry, Knowledge, and Understanding. New York: Oxford University Press.

Kosso, P. (2007). Scientific Understanding. Foundations of Science, 12(2): 173-188.

Lipton, P. (2004). Inference to the Best Explanation. New York: Routledge.

Potochnik, A. (2017). Idealisation and the Aims of Science. Chicago: The University of Chicago Press.

Salmon, W. (1998). Causality and Explanation. New York: Oxford University Press.

Soysal, Z. (2018). Formal Analyticity. Philosophical Studies, 175(11): 2791-2811.

Strevens, M. (2006). Explanation. In D. M. Borchert (ed.), The Encyclopedia of Philosophy, Vol 3 (pp 518-526). New York: Macmillan.

Riggs, W. D. (2003). Understanding 'Virtue' and the Virtue of Understanding. In M. DePaul & L. Zagzebski (pp. 203-226). New York: Oxford University Press.

## Jurisdiction in disciplinary intersections

*Elihu Gerson*[a] *and James Griesemer*[b]
[a]*Tremont Research Institute, United States;* [b]*Department of Philosophy, University of California, Davis, United States*
[a]emg@tremontresearch.org; [b]jrgriesemer@ucdavis.edu

Intersections among technical specialties have gained increasing attention in recent years as the course of research requires increasing intellectual integration (collaboration) at the project level, as well as increasing specification of protocols governing interaction among lines of work (cooperation). One of many problems raised by intersectional collaboration and cooperation is the manner in which jurisdiction is claimed, recognized, contested, enforced, and, especially, shared. By "jurisdiction", we mean the right to express an opinion on a subject and have it taken seriously, or even definitively. Specialties typically exercise jurisdiction over specific problem agendas, models, concepts, and data-handling methods. The opinion of a geneticist, for example, is more likely than that of an ecologist to "count" on a matter of inheritance. Conversely, the opinion of the ecologist is more likely to "count" than that of the geneticist on a matter of shifting patterns of species richness in a region. But what happens when both are jointly considering the relationships among genetic factors and the limits of species richness? How are the relative weights of similarities and differences of opinion among researchers from different specialties to be formulated, assessed, negotiated, resolved, or even overcome?

These considerations give rise to a rich family of questions about the ways in which different specialties come to share jurisdiction over issues of joint concern in ways that permit the programs of all participating specialties (and particular collaborative projects) to continue to successful conclusions. The relatively unspecific character of jurisdictions (as opposed to, say, property rights or contractual obligations) provides a zone of vagueness which facilitates negotiations over particular arrangements that can satisfy both the technical/intellectual and the administrative requirements of participants with different perspectives, repertoires, and resources through patient, often informal, negotiation. Jurisdictional boundary negotiation and sharing are thus very important in the early stages of a research program's life cycle, when flexibility in meeting as-yet unanticipatable contingencies is crucial to success.

The history of evo-devo as an emergent juncture of overlapping cooperation and collaboration among specialties is an example of jurisdictional sharing which has sought to maintain equality among participating specialties. At the same time, the very success of this effort has at times raised problems of jurisdiction between the "home" disciplines and emergent new programs. These contingencies seem to have expanded in recent years with the emergence of the extended evolutionary synthesis. We illustrate some of the questions and possibilities of understanding cooperation and collaboration from the perspective of jurisdiction management with preliminary data from our sociological study of the Purpose program (https://www.biologicalpurpose.org/), a group of projects studying different aspects of agency, directionality, and function in living systems.

---

## On ghosts and guests: European graduates students' perspective on good and questionable authorship practices

*Mads Goddiksen, Mikkel Willum Johansen, Anna Catharina Viera, Christine Clavien, Linda Hogan, Nóra Kovács, Marcus Merit, Anna Olsson, Una Quinn, Julio Santos, Rita Santos, Celine Schöpfer, Orsolya Varga, P. J. Wall, Peter Sandøe and Thomas Bøker Lund*

*University of Copenhagen, Denmark*

mpg@ifro.ku.dk

Questionable authorship practices are often pointed to as a major problem for research integrity, partly because they are relatively frequent. While there is a substantial literature discussing the prevalence of questionable authorship practices and practicing researchers' perception of fair authorship attribution (reviewed in Marusic, Bosnjak & Jeroncic [2011] and Hosseini & Gordijn [2020]), this literature focuses largely on the medical sciences, and young researchers are underrepresented. This study therefore included PhD students from all major faculties (STEM, medicine, humanities and social sciences). The study aimed to investigate PhD students' view of fair authorship attribution, their experience with awarding guest authorships to more powerful researchers, and their reasons for doing so. In particular it aimed to investigate how PhD students' views and experiences regarding authorship attribution varies across releant variables including faculty and country of work. To this end, a survey was constructed and circulated among PhD students in nine European countries. The final dataset includes N=1336 responses from five European countries (Denmark, Hungary, Ireland, Portugal, and Switzerland) representing all major disciplines.

A latent class analysis revealed three general views on authorship among the participants: 1) an inclusive view (low threshold for accepting co-authorship), 2) a restrictive view (high threshold for accepting co-authorship), and 3) a neutral view, where the participants did not a have a strong opinion on whether specific contributions were sufficient for co-authorship.

Abound a third of the participants who had been engaged in research collaborations had awarded an undeserved guest authorship to a person in power. This number however covers significant differences among various sub-groups in the population. Especially, we saw significant differences between the faculties, and country of work, and between participants from the three latent class groups described above. In other words, there are significant differences in the authorship practices of these groups. In our talk we will describe and analyze these differences. In our talk, we will also describe the variation in the reasons the participants gave for awarding (undeserved) guest authorships to persons in power, and discuss the implications our results have for the way academic integrity and good authorship practice is taught and enforced in the various scientific disciplines.

References

Marusic, A, Bosnjak, L and Jeroncic, A (2011). A Systematic Review of Research on the Meaning, Ethics and Practices of Authorship across Scholarly Disciplines, PLoS ONE 6(9): e23477.

Hosseini, M. & Gorjidn, B. (2020). A review of the literature on ethical issues related to scientific authorship. Accountability in Research 27:5, 284-324,

---

## Evaluation of Accident Causation Models in Engineering Science

*Kristian Gonzalez Barman*

*Ghent University, Belgium*

kristiancampbell.gonzalezbarman@ugent.be

Accident Causation Models (ACMs) are general models employed by engineering scientists to understand the general causal structure of accidents. Good ACMs are used to help answer both why and how accidents occur. These general models form the basis for different accident investigation methodologies, which in turn result in particular models of concrete accidents. These particular models are then used to prevent, repair, or understand concrete accidents, or to adjudicate legal responsibility.

ACMs affect how engineers and safety scientists gather, organize, and prioritize data and evidence; they constrain the space of possible explanations for accidents and influence how engineers understand accidents and their causes. Since different ACMs encode differing causal information, their scope, effectiveness, and explanatory power is dependent on the context in which they are applied.

I here review three popular ACMs and clarify their epistemic value: (i) sequential models, where accident causation is considered a sequence of events connected as a linear causal chain, (ii) epidemiological models, that consider accident causation as the accumulation of errors and defects in barriers that offer protection (such as protective equipment, or safety inspections and regulations), and (iii) systemic models, which take the causes of an accident to be a lack of constraints imposed on a (socio-technical) system's design, its control operations, and the behaviour of said system at each level of an organizational hierarchy.

The last two decades have witnessed a push towards the idea that systemic models are epistemically superior. Many engineering scientists argue that, because of the increased complexity of new technologies, a proper understanding of accidents (with the practical ramifications it entails) cannot be achieved by models employing linear causality (as found in sequential and epidemiological models). Instead, it is argued, we should take the systemic approach.

In order to evaluate this claim, I consider the potential of ACMs to generate models which afford a greater number of relevant counterfactual inferences, where I take relevant inferences to be the ones that provide useful safety (re)design information or suggest countermeasures (safety design interventions). This criterion for epistemic value is based on Ylikoski and Kuorikoski's account of explanatory power (2010).

If the set of relevant inferences afforded by 'old' models is a subset of the inferences afforded by the 'new' models, and the 'new' ones afford more relevant inferences, then the new ones are epistemically superior. If not, the understanding they provide and their usefulness is complementary, but there is not necessarily epistemic superiority between models (we could simply consider the 'toolbox' growing wider).

I argue that the systemic approach is in fact epistemically superior: the models it generates afford the same inferences as the other two approaches, but they also afford a greater number of relevant inferences. They achieve this, in part, by being able to represent non-linear causal relationships. I then consider whether there are pragmatic reasons to use applications of other ACMs in certain situations. The results of this analysis clarify the debate among engineers and provide tools for justifying the choice for certain ACMs over others.

## Hope and hype in preventive precision medicine

*Sara Green, Melanie Weilguny and Hanne Andersen*
*University of Copenhagen, Denmark*
sara.green@ind.ku.dk

The significance of precision medicine (PM) is currently greatest within the diagnostics and treatment of cancers and rare hereditary diseases, where "personalization" of treatments raises new and interesting questions about evidence standards (Plutynski forthcoming; Vogt and Hoffmann 2022). But another key aim of PM is to revolutionize disease prevention, i.e., to turn medicine into a predictive and proactive endeavour (Flores et al. 2013). In this talk, we examine the epistemic opportunities and challenges from the perspective of primary care. Primary care is often described as central to the overall realization of precision medicine, through the mutual benefits of individualized health monitoring and data-intensive research. Yet, implementation of PM in primary care is perceived as lagging behind scientific development (Vitone 2019). Scientific strategy papers stress an urgent need for genomic education for health personnel, while admitting that "it is unclear what evidence is necessary to convince doctors to clinically adopt new technologies" (Prichard et al. 2017). Exploring how the promises of PM are viewed from the perspective of primary care is therefore not only philosophically interesting but also of practical relevance. Our philosophical analysis is empirically informed by field observations at policy meetings on precision medicine, as well as a survey and interviews with practicing GPs in the Danish healthcare system. We explore the relation between optimistic promises and the views of health professionals, who are often faced with uncertainty about the clinical utility of preventive testing, concerns about medicalization and overdiagnosis, and the necessity of prioritizing their time and health care resources. Our analysis addresses the following questions. Is genetic testing and other forms of individualized risk profiling of "healthy patients" seen as an opportunity or a challenge by GPs? How do new testing opportunities and data practices affect primary care, including the roles and responsibilities of patients and health providers? We uncover the spectrum of expectations to preventive precision medicine and analyse the background for different views on the potential and challenges of precision medicine. Moreover, we shed light on how developments within big data science can impact questions about the purpose and organization of health care, as well the relation between disease and health.

References

Flores, M., Glusman, G., Brogaard, K., Price, N. D., & Hood, L. (2013).

P4 medicine: how systems medicine will transform the healthcare sector and society. Personalized medicine, 10(6), 565-576.

Plutynski, A. (forthcoming). Why precision oncology is not very precise (and why this should not surprise us). In Beneduce, C. & Bertolaso, M. (Eds.) Personalized Medicine: A Multidisciplinary Approach to Complexity. Springer International Publishing.

Pritchard, D.E., Moeckel, F., Villa, M.S., Housman, L.T., McCarty, C.A., McLeod, H.L. (2017). Strategies for integrating personalized medicine into healthcare practice. Personalized Medicine, 14(2), 141-152.

Vitone, E. (2019). Precision medicine for the masses. Mylynda Massart brings genomics to primary care. Feature article in PITMED, Summer 2019, 15-19.

Vogt, H., & Hofmann, B. (2021). How precision medicine changes medical epistemology: A formative case from Norway. Journal of evaluation in clinical practice,

---

## Revisiting The Problem of Context: A Critique of the Resources and Constraints Model

*Samara Greenwood*

*University of Melbourne, Australia*

sjgreenwood@student.unimelb.edu.au

In 2008, Peter Galison famously outlined ten problems for history and philosophy of science, the first of which was the problem of context, “that elusive explanatory structure always invoked, never explained.” Galison highlighted what he saw as two problematic proto-theories; first, that contexts deterministically ‘cause’ changes in science and second, that contexts merely provide novel ‘resources’ which scientists may choose to adopt or adapt as they see fit. He described the first approach as too strong and the second as too weak. In recent literature, we find the causal model of context has largely disappeared, however, strong support remains for what I’ve termed the Resources and Constraints model of context. While this expanded Resources approach has been supported by prominent scholars including John Schuster, M. Norton Wise and Theodore Arabatzis, it has yet to receive significant critical analysis. As such, I suggest a useful starting point for revisiting the problem of context is to analyse this model using an Integrated HPS approach with a focus on scientific practice.

In my analysis, I begin by describing the key features of the Resources and Constraints model, showing how social and intellectual contexts are depicted as constrained sets of resources or 'conditions of possibility' which enable and restrict the options for scientific practice in a specific place and time. Importantly, advocates of the model take pains to reject any 'causal' notion of contexts, instead underlining their conviction that scientists are autonomous agents able to freely choose resources from their environment. As Wise put it, he endorses a model "in which individuals are fully responsible for their choice of 'influences', or rather resources." This critical emphasis on individual agency then casts contexts as pools of passive, value-free provisions unable to influence scientific practice in any active way.

In analysing the validity of the model, I focus on two key concerns. First, I critique the sustained spotlight on individual action, arguing such a constrained focus neglects the importance of group practices to understanding the role of context. Second, I critique the unbending belief in the autonomy of scientific 'choice', arguing this approach devalues, even denies, the charged nature of contexts which (subtly and not-so-subtly) work to 'push and pull' scientific practices in particular directions. To illustrate this critique, I use several historical examples of scientific practices to show the ways in which both individual action and group practices are strongly, but often imperceptibly, shaped by their contextual environment. In particular, I focus on the role of changing social norms and values in shaping specific scientific practices. In this way, I argue the concept of 'constrained resources' is inadequate to describe the role of social and intellectual contexts. Rather, contexts are better depicted as dynamically charged landscapes which help actively shape scientific practices (and thus, products) in a variety of important ways.

References

Arabatzis, Theodore. 'Explaining Science Historically'. Isis 110, no. 2 (22 May 2019): 354–59.

Galison, Peter. 'Ten Problems in History and Philosophy of Science'. Isis 99, no. 1 (March 2008): 111–24.

Schuster, John A. 'Pitfalls and Opportunities of Contextual Explanation: The Case of Isaac Beeckman's Invention of the Mechanical Philosophy'. Isis 110, no. 2 (22 May 2019): 308–11.

Wise, M. Norton. 'Forman Reformed, Again'. In Weimar Culture and Quantum Mechanics, edited by Cathryn Carson, Alexei Kojevnikov, and Helmuth Trischler, 415–31. London: Imperial College Press, 2011.

# The Allure of Simplicity: On Interpretable Machine Learning Models in Healthcare

*Thomas Grote*
*Cluster of Excellence: Machine Learning: New Perspectives for Science; Tübingen University, Germany*
thomas.grote@uni-tuebingen.de

Clinical decisions entail high stakes, while being pervaded by uncertainty. Fueled by breakthroughs in deep learning, the assistance of machine learning (ML) models promises to improve medical diagnosis/prognosis (Esteva et al., 2019; Tomasev et al., 2019). However, the opacity of ML models (Sullivan, 2019; Creel, 2020) poses a key barrier for their implementation into clinical environments. After all, how to trust ML-based diagnoses/prognoses if their underlying logic remains elusive (Watson et al., 2019; Grote and Berens, 2020)? While the problem is well-established, there is little agreement on the appropriate opacity mitigation strategy. So far, the prevailing approach is to explain the functioning of ML models post-hoc, either through statistical summaries or visualizations of the predictors for single instances (hereinafter: XAI) (for review, see Samek et al., 2021). Unlike XAI, revisionary strategies seek to overcome the problem by replacing opacity with a different concept, deemed more crucial. One such candidate, computational reliabilism, is guided by the assumption that accuracy trumps explainability (London, 2019; Durán and Jongsma, 2021). Its rationale is a coherence argument: ML models should be evaluated according to the same standards as (other) medical interventions. Conversely, this entails that certain kinds of opacity are acceptable – be it the underlying physiological mechanisms of a drug or model opacity – if the reliability and clinical benefit of a given intervention has been established.

The second revisionary strand has been borne out of skepticism regarding current XAI methods (e.g., saliency maps), that have shown to be easily foolable in sanity checks (Adebayo et al., 2018). As an upshot, they are claimed to be necessarily misleading. Rather than deploying XAI methods, the proposed solution is to use models that are interpretable (Rudin, 2019; Babic et al., 2021). This includes classes of models, defined through certain structural properties (e.g., sparsity and linearity) or that possess built-in domain knowledge. That said, while calls for interpretable models are intuitively appealing, the research program lies on conceptually shaky grounds, once moving beyond toy examples. Furthermore, some of the fierce rhetoric in favor of intrinsic interpretability ultimately misses its mark. Hence, while I think that this approach has many merits, there is a need for more thorough grounding.

This paper has two aims: First, I set out to clarify the scope of using interpretable models within the context of healthcare. Here, I particularly draw on a recent study by Barnett et al. (2021), using an interpretable neural network, incorporating case-based reasoning strategies of radiologists to examine mammographic images. This example delineates interpretable models from XAI methods. Second, I argue that interpretable models are indeed superior to XAI methods and to computational reliabilism. Especially the latter suffers from some profound flaws and is not a viable solution when ML models ought to be used for clinical decision-support. For once, clinical trials or by benchmark performance tests only provide evidence for a model‘s reliability at the population-level. In turn, the relevant evidence does not clinch the reliability of a ML model for single instances. More critically, malfunctions of ML models due to domain shifts (cf. Finlayson et al., 2021) highlight stark discontinuities between the transferability of a model‘s performance – when used outside training conditions – and the causal effects of medical interventions, established via randomized controlled trials (RCTs).

Finally, while revisionary approaches use XAI as a foil, it will be argued that the more interesting contrast class is an evidentially pluralist approach, combining evidence regarding a ML model‘s predictive performance with XAI methods, while also providing estimates of model uncertainty. However, this approach has its own weaknesses, specifically because the different kinds of evidence are not sufficiently independent of each other.

---

## Deep neural networks as mechanistic explanations - in search of the explanans

*Bojana Grujicic*

*Max Planck School of Cognition; Humboldt-Universitaet zu Berlin; University College London, Germany*

bojana.grujicic@gmail.com

Although deep neural networks (DNNs) were initially developed in the engineering field of computer vision, an array of recent results suggests that DNNs trained for object recognition are currently the best neuroscientific models for predicting neural responses in the human ventral stream (Lindsay 2021, Kriegeskorte 2015). This novel field that involves engineering directly in the pursuit of neuroscientific goals, aims to offer a novel methodology for neuroscience in contrast to how it was traditionally done (Nastase et al. 2020), having hopes of fulfilling not just its predictive but its explanatory goals as well (Cichy & Kaiser 2019). The way to understand a system, such as the visual system, is to try to build it (Cao & Yamins 2021), according to this view.

According to some neuroscientists, DNNs are somewhat explanatory in virtue of capturing relevant neural and behavioural data, which enables us to generate some understanding of biological vision (Lindsay 2021, Kietzmann et al. 2019, Kriegeskorte 2015). In addition, philosophical interest in neural networks has been lately rising, with several arguments offered suggesting how neural network models could in principle explain mechanistically (Stinson 2018) or that they are already mechanistic explanations of the object recognition capacity (Cao & Yamins 2021, see also Buckner 2018). I analyse the claim of DNNs being mechanistic explanatory models of the object recognition capacity in humans (Cao & Yamins 2021, Buckner 2018) and I inquire what exactly the DNN-based mechanistic explanans would be. Looking at the current research practice at the intersection of deep learning and neuroscience, I outline three different options. 1) Individual nodes in a DNN, their point-to-point connections and their organisation are the relevant entities, activities and organisational properties that map adequately onto the brain (Kaplan & Craver 2011). 2) Neural manifolds in high-dimensional state spaces are basic entities and transformations over them are basic operations. 3) The variables of architecture of a DNN, objective function and the learning rule based on which a DNN was trained.

I will not try to arbitrate between these mechanistic explanantia. Rather, I focus on the findings of Mehrer et al. (2020) and Storrs et al. (2021). The former study suggests that there are individual differences in processing identical stimuli between instances of architecturally identical DNNs. What is then a shared mechanism between these instances of architecturally identical DNNs that should be the explanans of the object recognition capacity in humans? The comparison of complex systems such as instances of DNNs as well as DNNs and brains crucially relies on similarity measures. I analyse different similarity measures in play in contemporary neuroscience and show that some of them deliver opposing verdicts regarding the similarity of instances of architecturally identical DNNs. Which similarity measure is the adequate one has to be arbitrated relative to the explanandum capacity, in this case the object recognition capacity. I offer a methodological proposal to help arbitrate between similarity measures in this research context. Before we progress on this ground, I will claim, it is underdetermined which of the three options of DNN-based mechanistic explanantia is the adequate one for the object recognition capacity, contra Cao & Yamins (2021) and Buckner (2018).

References

Buckner, C. (2018). Empiricism without magic: Transformational abstraction in deep convolutional neural networks. Synthese, 195(12), 5339 – 5372.

Cao, R., & Yamins, D. (n.d.). Making sense of mechanism: How neural network models can explain brain function.

Cichy, R. M., & Kaiser, D. (2019). Deep Neural Networks as Scientific Models. Trends in Cognitive Sciences, 23(4), 305–317.

Kaplan, D. M., & Craver, C. F. (2011). The Explanatory Force of Dynamical and Mathematical Models in Neuroscience: A Mechanistic Perspective*. Philosophy of Science, 78(4), 601–627

Kietzmann, T., McClure, P., & Kriegeskorte, N. Deep Neural Networks in Computational Neuroscience. Oxford Research Encyclopedia of Neuroscience

Kriegeskorte, N. (2015). Deep Neural Networks: A New Framework for Modeling Biological Vision and Brain Information Processing. Annual Review of Vision Science, 1(1), 417–446

Lindsay, G. W. (2021). Convolutional Neural Networks as a Model of the Visual System: Past, Present, and Future. Journal of Cognitive Neuroscience, 33(10), 2017–2031

Mehrer, J., Spoerer, C. J., Kriegeskorte, N., & Kietzmann, T. C. (2020). Individual differences among deep neural network models. Nature Communications, 11(1), 5725

Nastase, S. A., Goldstein, A., & Hasson, U. (2020). Keep it real: Rethinking the primacy of experimental control in cognitive neuroscience. NeuroImage, 222, 117254

Stinson, C. (2018). Explanation and connectionist models. In M. Sprevak & M. Colombo (Eds.), The Routledge Handbook of the Computational Mind

Storrs, K. R., Kietzmann, T. C., Walther, A., Mehrer, J., & Kriegeskorte, N. (2021). Diverse Deep Neural Networks All Predict Human Inferior Temporal Cortex Well, After Training and Fitting. Journal of Cognitive Neuroscience, 1–21

## Pursuit Worthiness of Scientific Models: Lessons from QSAR and Drug Design

*Hyejeong Han*
*Korea Advanced Institute of Science and Technology (KAIST), South Korea*
hhj29@kaist.ac.kr

Since Larry Laudan's (1977) context distinction between pursuit and acceptance, discussions about the pursuit-worthiness in scientific inquiry have increased. On the other hand, the pursuit-worthiness of models has received relatively little attention, with the majority of discussion centred on theory pursuit-worthiness. Several other philosophers of science have provided notable accounts of model pursuit-worthiness, but the most of them treated models as theoretical representations, with no regard for the distinguishing features of models from theories. The primary goal of this talk is to show that the pursuit of models should be taken seriously on its own. More precisely, this talk discusses the heuristic function of models by emphasising their role in mediating between data and phenomena, based on accounts of the data/phenomenon distinction (Bogen & Woodward, 1988; Woodard, 2011; Bailer-Jones, 2009).

This talk discusses lessons learned from a historical episode in the field of drug design, specifically the 1960s and early 1970s pursuit of Quantitative Structure-Activity Relationship (QSAR) models. QSAR is a statistical modelling method for associating the molecular structure of a chemical compound with its biological activity. Pharmaceutical industry scientists pursued QSAR models in the expectation that they would support in the efficient development of new drugs. The expectation, however, was not based on the models' connection to chemistry or biology theories, or on their demonstrated success in developing new drugs. At the heart of this talk lies the puzzle of why scientists spent time and money on QSAR models when there appeared to be no reason.

In terms of history, this talk fills a gap in the historiography of drug research by developing a narrative describing the practices of pharmaceutical industry scientists in the 1960s and early 1970s. Given the extremely high costs of drug candidate synthesis-and-test cycles, deciding which drug candidates to synthesis-and-test next posed a significant challenge for the scientists. What made QSAR models attractive from an epistemological standpoint was their ability to assist in determining the "test-worthiness" of hypotheses about drug candidates, a concept that I propose in order to focus on the practical aspects of testing. By assisting in the identification of hypotheses that were more "test-worthy," QSAR had the potential to significantly reduce the costs associated with the synthesis-and-testing of drug candidates.

In terms of philosophy, this talk develops an account of model pursuit-worthiness by critically examining Steven French's (1995, 1997) concept of heuristic fruitfulness. While French's insight linking models' heuristic function to their pursuit-worthiness is remarkable, the concept of models' heuristic function should be interpreted differently to capture the instrumental nature of models. As demonstrated in the QSAR episode, models can serve a heuristic function by "constructing" phenomena from data rather than necessarily developing into successful theories on their own. In this way, models can guide future epistemic practices, such as assisting in determining the test-worthiness of hypotheses.

The central argument of this talk is that when assessing the pursuit-worthiness of scientific models, it is critical to consider whether the phenomena constructed by the models will aid in determining the direction of future epistemic practices. Models can effectively guide and advance us toward our epistemic goals by assisting inferences between data and phenomena, without requiring us to rely on high-level theories.

---

## A Strategy to What End? "The Strategy of Model Building in Population Biology" in its programmatic context

*Zvi Hasnes-Beninson*

*Tel Aviv University, Israel*

habezvi87@gmail.com

According to Google Scholar, the paper "The Strategy of Model Building in Population Biology" published by the ecologist and population geneticist Richard Levins in 1966 has been cited more than 2450 times. For a paper concerned with modeling approaches in population biology, a disproportionally large part of the attention The Strategy received is from history and philosophy of biology. While this philosophical attention began shortly after the publication of The Strategy, it became prevalent in the late 1980s (culminating in a special issue of Biology and Philosophy from 2006), with the lion's share of that attention coming from philosophical accounts of models and model formulation.

Here, I attempt to contextualize The Strategy in its original context; The Strategy was an attempt to address certain issues in ecology, and was intended for researchers working in the field. Based on a research in Levins' personal archive, I will argue that from an early stage, Levins considered the separation between theory and practice as a false dichotomy in ecology. The aspect of ecology where theory and practice most strongly intertwine concerns environmental degradation. The approach that emerged as the dominant research program to deal with those challenges starting from the late 1950s was ecosystem ecology, which applied principles of systems analysis to the study of population-level phenomena. While Levins was heavily influenced by Rachel Carson's book Silent Spring, and deeply committed to environment preservation, he rejected ecosystem ecology on the grounds that its modeling approach was inapt to ecological context.

However, there is another contextual dimension that my paper exposes. The Strategy is an unusual paper – it presents neither new data nor a new formal model; at times it reads like a manifesto for some modeling approach, without specifying which broader program that approach intends to support. Moreover, when considering Levins' marginal position at the time The Strategy was published (having finished his doctoral studies shortly before), and the fact that he wrote the paper by himself, the paper strikes one as peculiar.

When these peculiarities of The Strategy are even mentioned, the philosophical literature tends to explain them away by invoking Levins' Marxist commitments. In contrast, I argue that those peculiarities can be explained by examining the programmatic purpose of the paper – The Strategy called for a methodological approach meant to be used in a broader research program that Levins was trying to establish. That program meant to account for the relations between fitness and environment in different terms than the prevalent lock-and-key view. Levins finalized that program in 1974 with his work on loop analysis and time averaging, but he had worked on certain aspects of it already in his doctoral work. My paper brings that program back to the discussion of Levins' 1966 paper, explains its relation to competing approaches and presents the role Levins ascribed to The Strategy within the controversies of the day.

Reading Levins' by now classical paper in its immediate context helps understand the peculiarities of the paper while also allowing me to elucidate anew Levins' "philosophy of modeling" and frequent themes in contemporary philosophy of science.

## The Limitations of Biological Examples in Evolutionary Explanations

*Caleb Hazelwood*
*Duke University, United States*
caleb.hazelwood@duke.edu

In his recent monograph, The Causal Structure of Natural Selection, Charles Pence gives an updated account of the debate between "causalists" and "statisticalists." Causalists argue that natural selection is a bona fide causal process that acts on populations, whereas statisticalists argue that natural selection is not a genuine cause, but a statistical summary of the lives and deaths of individual organisms. In defense of causalism, several philosophers of science have appealed to case studies from biological practice to illustrate the causal power of selection. Pence, however, is skeptical of this approach.

In this talk, I develop and defend Pence's claim that an appeal to biological practice is necessary but insufficient to resolve the causalism/ statisticalism debate. This is because the rhetorical force of any case study from evolutionary biology will turn on two features: the empirical details of the example and the conceptual commitments of the interpreter. In cases where competing interpretations seem to equally comport with the empirical details, philosophers of science must deploy other kinds of resources. At best, I will argue, an appeal to biological practice equips us with a means of constraining the possible explanations of the case study in question.

For example, Pence acknowledges the similarities between Jaegwon Kim's "causal exclusion arguments" in the philosophy of mind and difficulties that arise when distinguishing population-level causes from individual-level causes in evolutionary biology. Kim's argument rules out downward causation—the possibility of macro-level entities causing changes in micro-level entities. An analogous claim can be attributed to statisticalism, namely, that natural selection is not causally responsible for changes at the individual level. All the relevant causal details can be cashed out in terms of the births and deaths of individual organisms. Natural selection, therefore, is epiphenomenal—a statistical shadow of more fundamental processes.

Evaluating this argument by analogy demonstrates both the promises and the limitations of an appeal to biological practice in the causalism/statisticalism debate. On one hand, the possibility of downward causation in biology is, at least in part, an empirical matter. For example, in the case of frequency-dependent selection, changes in the properties of populations are indeed correlated with changes in the fitnesses of individuals. Therefore, biological practice may leave downward causation on the table. On the other hand, the supervenience of populations on individuals leaves us without recourse to empirical tests for strictly population-level causes. This is because we cannot manipulate a population-level property without manipulating the underlying properties of its constituent individuals. Therefore, whether one concludes that selection is epiphenomenal may depend on whether one believes that causal powers at the micro-level are transmitted to the macro-level.

In sum, progress in the causalism/statisticalism debate will demand that we look to biological practice to inform our philosophical theories. We should not, however, expect that sensitivity to practice will settle the matter entirely. By carefully distinguishing places where accounts of causation are constrained by empirical biology from those where accounts of causation elude it, we are positioned to gain conceptual and methodological clarity in an otherwise vexed debate.

---

## On "collective knowledge" in the fixation of a scientific data set

*Götz Hoeppe*
*University of Waterloo, Canada*
ghoeppe@uwaterloo.ca

What could astronomers possibly mean when they say (as some do) that "a catalogue" – a table of the measured properties of celestial objects – "encodes the collective knowledge of its makers"? What could we learn from a detailed ethnographic account of astronomers' practical reasoning as they achieve an agreement on such a form of data? And how could this possibly contribute to philosophical discussions of "collective knowledge"?

These are questions that puzzle me as I reflect on the two years of my ethnographic study of the MUWAGS (Multi-Wavelength Galaxy Survey; pseudo-acronym) collaboration, a team of ca. 30 astronomers from Europe, North America, China and India. When eventually published, their catalogue was central to their data release: a large table (90,000 rows and 200 columns) of measurements of objects in a certain part of the sky, including their celestial coordinates, classification (e.g. 'star' or 'galaxy') and physical parameters such as brightness, colours, distance, and mass. It was intended both for the team's own future work as well as for uses by other researchers.

Typically generated by large teams, large data sets like the one of MUWAGS usually include processed, higher-level data, such as measurements, that are useful for diverse scientific studies. From John Hardwig

(1985) onwards, the collective authorships of large teams appears to have been of specific philosophical interest. Recent philosophical discussions of collective knowledge by Margaret Gilbert, Brad Wray and others have been largely concerned with propositional accounts of knowledge. But this captures only in part what scientists – who are more committed to shared practices than to shared beliefs (Rouse 2003, Chang 2017) – are after.

The fixation of the MUWAGS catalogue was a negotiation, resulting in what was acceptable to team members and coherent with the diverse data uses pertinent to their completed work. It was through preparing their catalogue as an 'instructing data object' that this team seeked to encode its members' knowledge of how the data were processed and to make it consequential for users by devising methodical ways to structure anticipated uses. These methods included introducing redundancies that would help users to self-correct mistaken uses, selectively deleting data, and deflecting accountability through making notational choices. These methods dwell on an understanding of knowledge not as exclusively propositional, but as embedded in witnessable activities and the products of these activities.

My paper develops three distinct moves from this account. The first is to take the materiality and mediality of writing into account and point out how it matters for the fixation of data sets that are irreducible to the work of individual team members. The second is to follow Gilbert Ryle (1949) and move from propositional notions of knowledge to conceive of knowing as a 'capacity'. The third is an invitation to engage the kinship of Ryle's work with praxeological approaches like ethnomethodology with the aim to make them fruitful for philosophical studies of science in practice.

References

Chang, Hasok (2017). Operational Coherence as the Source of Truth. Proceedings of the Aristotelian Society, vol. 67, part 2, pp. 103 – 122.

Hardwig, John (1985). Epistemic Dependence. Journal of Philosophy 82 (7), pp. 335-349

Rouse, Joseph (2003). Kuhn's Philosophy of Scientific Practice. In: Nickles, Thomas (ed.) Thomas Kuhn. Cambridge: Cambridge University Press, pp. 101–121.

Ryle, Gilbert (1949). The Concept of Mind. London: Hutchinson.

# Using X-ray Computed Tomography to study phenomena in biomedical practice: Insights for the philosophical debate on phenomena and data

*Linda Holland*

*Vrije Universteit Amsterdam, Netherlands*

h.a.holland@vu.nl

In philosophy of science it has been assumed that scientific theories are aimed at explaining and predicting the data (i.e., public records) that are produced in experiments. However, Bogen and Woodward (1988) have argued that theories are aimed at explaining and predicting phenomena (i.e., stable, repeatable effects or processes) rather than data. We have to analyze the produced data (e.g., with statistical methods) before we can conclude something about the phenomenon that the data provide evidence for.

Bogen and Woodward assume that phenomena are given in nature. Data-to-phenomena reasoning is justified when the probability that one accepts a claim about the phenomenon based on the data when and only when that claim is true is high (Woodward 2000). Massimi (2011) has objected that data-to-phenomena reasoning cannot be justified using this criterion. Scientists need to have some causal knowledge about the mechanism that generates the data to justify their inferences from data to phenomena, since phenomena are underdetermined by data.

On her own account, phenomena are not only the result of input from nature in the form of data, but also the result of conceptual construction by humans. According to her, data-to-phenomena reasoning starts with the produced data, but in addition it requires a causal concept that maximizes the probability of the produced data.

In this presentation, I will examine this debate and apply it to the context of biomedical imaging. More specifically, I will investigate the practice of studying phenomena with the often-used imaging technology X-ray Computed Tomography (CT).

I will demonstrate that this case study confirms Bogen and Woodward's argument that scientific inquiry starts with questions about a phenomenon of interest. This is in contrast with Massimi's account in which scientific inquiry starts with the produced data. Still, Bogen and Woodward miss one important component that is required to justify data-to-phenomena reasoning: causal knowledge about the mechanism generating the data, as pointed out by Massimi. This causal knowledge can be provided by scientific theories and scientific concepts.

Moreover, I will demonstrate that in the context of CT not only causal knowledge about the connection between the data and the phenomenon is required for data-to-phenomena reasoning – as has been argued by Massimi –, but in addition, causal knowledge about the connection between the data and noise (i.e., data due to other causal factors than the phenomenon).

Based on the case study, I will conclude that phenomena are partially the result of input from nature in the form of data, and partially constructed using both causal knowledge about the connection between the produced data and the phenomenon of interest and about the connection between the data and noise.

An important problem that has been discussed in the debate on phenomena and data is the underdetermination of phenomena by data. By requiring causal knowledge about the connection of the produced data to the phenomenon of interest and to noise in data-to-phenomena reasoning, the proposed account brings us closer to solving this problem than the discussed accounts of Bogen & Woodward and Massimi.

References

Bogen & Woodward (1988). The Philosophical Review 97(3): 303-352.

Massimi (2011). Synthese 182: 101-116. Woodward (2000). Philosophy of Science 67: S163-S179.

---

## Where the Wild Things are Classified: feminist values and classification practices in cases of human-wildlife conflict and ecological crises

*Denise Hossom*

*University of California, Davis, United States*

drhossom@ucdavis.edu

A folk conception of the “wild” existed long before the biological sciences had means to quantify, qualify, or choose it as the focus of environmental conservation efforts. Yet classification practices for “wild” nonhuman animals are becoming increasingly complex. Movement of “wild” nonhuman animals across the planet under diverse scientific and social contexts and aims, alongside shifting classification practices, can shape how particular cases of human-wildlife conflict (HWC) and ecological crises (EC) are understood. My larger project examines classification of nonhuman animals in the biological sciences under four interrelated sets of concept categories ; 1) wild - domestic, 2) feral - tame, 3) free-roaming - captive, 4) vermin/varmint/pest - livestock/pet.

This paper gives an account of two felid conflicts; endangered 'wild" snow leopard (Panthera uncia) HWC in Nepal, and "feral" cats in the US, New Zealand, and Australia (Felis catus) as EC. Snow leopards in Nepal encounter HWC through surplus killing of domestic livestock; herders may lose 40 to 100 animals in a depredation event by a single cat, which is often met with retaliatory killing of snow leopards. Domestic livestock in mountainous regions are a main economic resource to impoverished rural communities, and livestock compete for grazing with wild prey species for snow leopards, promoting a "wild versus domestic' framing. Contrastingly "feral" cat EC conflicts involve the "domestic" cat (F. catus), and exhibit a wider range of classification practices, and relations between both humans, F. catus, and their broader ecological communities.

As HWC and EC conflicts include both social and scientific stakeholders, I examine how conflict discourses appeal to constitutive and contextual values (Longino 1990) to shape the framing of particular conflicts. I report on how narratives of "the wild", as a relational concept, capture which human stakeholders (social or scientific) are given authority to speak on behalf of which nonhuman animals. I argue that framing "the wild" with appeal to solely traditional scientific values, increases the likelihood that power relations of domination appear in stakeholder group dynamics. Reframing conflict with appeals to feminist values, such as "diffusion of power" and "mutuality of interaction" (Longino 1995, 1996) allows novel interests to emerge from both scientific and social stakeholder groups. The role of 'scientist as advocate' (Odenbaugh 2003) comes into question, and raises issues related to the social constitution of practicing scientific communities engaged in HWC/EC conflicts.

An implication of framing cat conflicts through diffusion of power and mutuality of interaction is that it compels a stronger inclusion of indigenous perspectives within the scientific community in environmental and ecological sciences dealing with HWC and EC (Whyte 2018, Salmón 2000, Reo & Ogden 2018). Diffusion of power critically enforces Contextual Empiricism's criterion of equality of intellectual authority (Longino 1995, 384-389) in areas of the biological sciences that interact with marginalized communities concerned with histories of biotic colonialism (Lean 2021) and ecological imperialism (Guha 1989). Additionally, these feminist values can support the application of standpoint theory to include indigenous perspectives as a matter of epistemic importance to understanding HWC and EC.

## Commercial provision of evidence in information security

*Phyllis Illari and Jonathan Spring*
*University College London, United Kingdom*
phyllis.illari@ucl.ac.uk

Philosophical work on evidence recently extended to institutional processes, such as the authoritative role of the National Institute for Care and Health Excellence (NICE) in assessing medical evidence in the UK and internationally, partly based on its transparency (Parkkinen et al., 2018). Yet many other institutions produce authoritative evidence.

Information security (infosec) concerns prevention of unauthorised actions on information, and involves millions of practitioners internationally, across sectors, across private and public organisations, and across scales from 50-person to 400k-person organisations. Evidence is often gathered by commercial providers, using tools such as machine learning. This raises questions about how evidence gathered at large scale, by commercial providers, relying on machine learning, is shared, demonstrated, generalised and trusted by so many different kinds of institutions, without the level of transparency that institutions such as NICE offer.

We will explore the systems and institutional decision-making behind the automatic security updates your computer makes. We will describe the recent history of the complex architecture discovering vulnerabilities in software through the Exploit Prediction Scoring System (EPSS) Special Interest Group (SIG) (Romanosky and Jacobs, 2022), organised under FIRST. EPSS tries to predict the time between a vulnerability becoming publicly known and that vulnerability being exploited by attackers. The history of how the SIG comes to ask its questions and provide evidence demonstrates a case of supranational provision of authoritative evidence. Results are made public. However, there are various ways that organisations speed up affiliated processes for a fee, to prioritise security patches and protections.

We will examine how EPSS estimates two key things: i) what exploit code matches what vulnerability (a complex task), ii) observations of attempts to use such exploit code. Data for these estimates come exclusively from the company Kenna Security under a non-disclosure agreement to the SIG. These private data complement existing public data from other sources.

This information (via EPSS's predictions) is used to prioritise both fixes and detection strategies. We will identify two important features of this. First, automatic intrusion detection systems (IDS) rely on static signatures with a high probability of uniquely being an exploit (from well-studied systems or vulnerabilities), due to their need for very low false positive rates (Axelsson, 2000). Second, we show that the evidence production loops: EPSS's predictions contribute to deciding which signatures are included in the detection list, while in turn EPSS's predictions are based on alerts from these signatures from the network security provider (Kenna), using machine learning to associate those alerts with features of the vulnerabilities.

We will show how authoritative evidence is nevertheless provided. Notable features include important processes of standardisation, such as allocating vulnerabilities a Common Vulnerability Enumeration Identifier (CVE-ID) and putting exploit code in databases (ExploitDB and Metasploit). Significant challenges include how predictions made from Kenna's customers' network traffic are shared and used for infosec provision on very different kinds of systems. The evidence production process copes, at various levels, with three challenges in infosec: changeable software, deceptive adversaries, and well-motivated secrecy (Spring and Illari, 2019).

References

Stephan Axelsson. (2000). The base-rate fallacy and the difficulty of intrusion detection. ACM Transactions on Information and System Security (TISSEC), 3(3), 186-205.

Veli-Pekka Parkkinen, Christian Wallmann, Michael Wilde, Brendan Clarke, Phyllis Illari, Michael P Kelly, Charles Norell, Federica Russo, Beth Shaw, Jon Williamson (2018): Evaluating evidence of mechanisms in medicine: principles and procedures, Springer.

Sasha Romanosky and Jay Jacob (chairs) (2022). Exploit Prediction Scoring System (EPSS). https://www.first.org/epss/. Forum of Incident Response and Security Teams.

Jonathan M Spring, Phyllis Illari (2019). Building general knowledge of mechanisms in information security. Philosophy & Technology, 32(4), 627-659.

**Incompatible Models, Modality and Realism**

*Aditya Jha*
*University of Canterbury, New Zealand*
aditya.jha@pg.canterbury.ac.nz

The Problem of Incompatible Models (PIM) refers to the existence of multiple contradictory models of a target system T, which puts up a challenge to the realist reading of these models (Morrison 2011; Weisberg 2007). Perspectival Modelling (Massimi 2018; Rice 2019) aims to solve the PIM by rejecting the 'representation-as-mapping' notion that models must represent/capture the structural features of T accurately in order to explain the relevant features of T. The departure from representation-as-mapping is justified by appeal to the modal features of perspectival models which map "the space of what is objectively possible" (Massimi 2018, 350) by "capturing (modal) patterns of behaviour that are universal across classes of real, possible, and model systems" (Rice 2019, 96).

This paper shows that the appeal to modal features of T and universality class arguments fails to distinguish between degrees of modality offered by these perspectival models because the conditionals to these modal inferences fail to circumscribe the antecedents correctly and systematically. With the help of a case study of modal explanations of pendulum systems ($n$-tuple pendulums), the paper shows how some seemingly robust modal explanations break down under various perturbations spelling trouble for the perspectivalist. This is because (1) it is simply not the case that a system falling in a universality class with certain antecedents (related to perturbations) will fail to be a part of the same universality class if the antecedents change, and (2) nothing in the modal model alone or in the universality class allows us to ascertain the antecedents that make an explanation work, thereby affecting its degree of modality and applicability.

The case study discussed concerns with the prediction of the minimum number of equilibrium points of pendulum systems using universality classes of topological manifolds. In most cases, an n-tuple pendulum has at least $2^n$ equilibrium positions, which can be explained by modelling the potential energy function of the system over its configuration space and then counting the number of critical points of the resulting manifold using Morse theory (generalizing the format of explanation from a special case of double pendulums discussed by Lange 2016, p. 27-28, which is a putative case of modal explanation). The shape of the manifold restricts the number of critical points that can be admitted over it and thus, allegedly, modally restricts the number of equilibrium positions of the pendulum system as well thereby showing that the phenomena falls with the universality classes of topological manifolds. However, the paper shows that these explanations break down, importantly, when we introduce perturbations in the length of the pendulum rods because they open a pandora's box of a variety of degenerate potential energy functions that cannot be accommodated in the simple topological framework of Morse theory thereby breaking the modal force of the argument. The perspectivalist cannot account for these explanatory failures because unless one works out these explanations on a case-to-case causal basis (which requires an approximately correct structural mapping to T), one cannot show what antecedents lead to the failure of the modal conditional. The arguments in the paper generalise for any dynamical system with a purported modal explanation analogous to the one above.

---

## The Science of Antitrust: Market Definition as Mechanism Identification

*Jennifer S. Jhun*

*Duke University, United States*

jennifersjhun@gmail.com

In this paper, we introduce the interested philosopher to some basic concepts in antitrust analysis, in particular on market definition. How antitrust cases play out in the courtroom, and the role of economic reasoning in the process, is a continuously evolving dialogue even today. Antitrust analysis and regulation constitute an arena where concerns regarding measurement, classification, theory, modeling, and scientific expertise all meet. Moreover, it has serious implications for legal and policy interventions; it is here that the economics qua science engages with the law, where industrial organization meets competition policy. However, it has received no attention from philosophers of science. I believe it could be of interest, especially those inclined towards practice-oriented approaches, as it is a juncture where they can be potential contributors to the discussion. This paper is a step towards addressing this lacuna, and does double duty as a partial introduction for the interested philosopher to a few basic but fundamental concepts from antitrust analysis and items from the economist's toolkit.

The question the economist faces is: does a particular firm have market power or not? Is it likely to exercise that power? If a firm has market power, it is able to profitably raise and maintain prices above competitive levels. The risks are that this could possibly stifle innovation, lower product quality or quantity, and cut into consumer welfare. Determining these answers is just as much an art as it is a science. There is yet no completely algorithmic, surefire strategy for addressing the question of market power in any particular instance, even setting entirely to the side the practical matter of courtroom litigation.

We suggest that the activity of market definition is, in the parlance of philosophy of science, that of identifying the contours of a mechanism. However, these considerations lead us to diagnose the mechanistic framework with a shortcoming: without further supplementation it does not provide clear identity conditions for mechanisms, namely when two mechanisms are of the same kind. This is problematic because in antitrust analysis, it is crucial to determine whether a market mechanism is of one sort or another. In particular, antitrust analysis constantly compares the market at hand with a counterfactual, so it is crucial to be able to answer whether (1) these two are the same kind of market, and (2) what the relevant counterfactual is. We provide a framework for answering both questions, appealing to Woodward's (2003) interventionist conception of causation for the former and Norton's (2021) material theory of induction for the latter.

---

## Against alignment: why experts should use their own values in responding to epistemic risk

*Stephen John*
*University of Cambridge, United Kingdom*
sdj22@cam.ac.uk

In recent work on the proper role of non-epistemic values in policy-oriented science, various authors have suggested that scientists should be responsive to, and guided by, the values an audience does (or should) hold. Drawing on a case-study of the advice given by the UK's Joint Committee on Vaccination and Immunisation (JCVI) to the government during the Covid-19 pandemic, this paper argues against this claim. Rather, there are good ethical and epistemic reasons to think that experts ought to be guided by "professional" values, irrespective of audiences' concerns.

The first part of the presentation sets out arguments for the claim that any non-epistemic values which structure research or its communication should be consistent with or guided by the values of their audiences. I argue that, despite superficial differences, authors such as Alexandrova, John and Schroeder all seem agree on the value of alignment.

The second part turns to the JCVI's deliberations over whether or not to recommend routine Covid-19 vaccination for 13-16 year-olds. A distinctive feature of these deliberations is that the committee did not offer "value-free" advice, but, rather, was at pains to stress that its advice was guided by - and solely by - "medical values". Epidemiologists, by contrast, offered contrary advice, which was explicitly framed in terms of the value of "population health". Ultimately, in this case, the Government sided with the second group of experts, over the first.

The third section takes up the obvious questions: in failing to align to the Government's values, did the JCVI do anything "wrong"? I suggest not. Rather, in virtue of their training and background, members were only qualified to speak on a narrow range of inter-related ethical and epistemic topics. Demanding they align advice to a broader range of values would require ethical and expertise possess they did not possess. Furthermore, I suggest that familiar claims about ethical pluralism and epistemic pluralism imply that such expertise cannot be possessed; the best that any experts can do is to give us insight into part of the policy puzzle.

The final section considers the implications of my view for notions of trust and trustworthiness. My work poses serious challenges to recent moves to make "alignment" a condition for what Irzik and Kurtulmus call "enhanced epistemic trust". What is required for trust is that people do their job well, not that they share "our" values. I finish by noting some more general implications of my remarks for how we think about the proper role of science in policy.

---

## Perspectivism and multi-species epistemology: the case of coral reefs

*Elis Jones*

*University of Exeter, United Kingdom*

es744@outlook.com

In this paper I examine the philosophical underpinnings of a particular part of coral reef science: coral reef bioacoustics, the study of how reef organisms generate and respond to sounds. In doing so, I flesh out an extension of current perspectivist accounts of science, by including perspectives of non-human organisms, rather than just novel human perspectives. In coral bioacoustics, scientists seem to have access to the perspectives of non-human organisms, for example when exploring the way in which certain sounds imprint on reef organisms, causing them to preferentially swim towards these sounds and to populate reefs which produce them (Simpson et al., 2010). These practices are fruitful: they provide ecologically relevant techniques for assessing reef health, and may also be used to improve it, for example by bringing fish larvae to degraded reefs, boosting the chances of that ecosystem regenerating (Gordon et al., 2019).

These scientific practices rely on, at least in part, occupying the perspectives of other organisms. And yet this is exactly what is famously declared impossible by Thomas Nagel in his paper 'What is it like to be a bat?', where he argues that we can't know what it is like to be another organism (Nagel, 1974). This would seem to imply that we cannot occupy or integrate their perspectives into ours. How can this be squared with the success of coral bioacoustic practices? I argue that a combination of three existing theories in philosophy of science can help here: Joseph Rouse's new naturalism, whereby science is a form of niche construction, with scientists shaping their environments to make them more conducive to understanding (Rouse, 2016); Adam Toon's cyborg empiricism, which treats scientific equipment as part of the extended physiology of the scientist (Toon, 2014); and Michela Massimi's perspectivism, whereby scientists multiply the perspectives available to them when observing phenomena, again with the goal of improved understanding (Massimi, 2017).

Combined, these theories explain what it is that coral bioacousticians are doing, and why it is successful: they use instrumentation to deliberately reconstruct the cognitive niches of reef organisms, so as to understand how they perceive the world, and then build the perspectives of these organisms into our overall understanding of the reef. They also show a way past Nagel's arguments: each theory emphasises the extended and ecological nature of perception, and thereby pushes against the organism/environment (or body/instrument) dichotomy which Nagel relies upon when arguing that perception is a private affair. If perception operates through extended (i.e. not just internal or bodily) physiology, it becomes a continuous (rather than discrete) matter whether we can occupy other perspectives. This is what coral scientists do when they listen to reefs, taking advantage of the cognitive and epistemic affordances of the environment in similar ways to other reef organisms. This view of scientific practice allows for recognition that epistemologies can incorporate facts and values from the perspectives of many organisms, that is, that scientific practice can produce multispecies epistemologies.

**Accountability of deep machine learning models: Accounting for adequacy for purpose**

*Koray Karaca*
*University of Twente, Netherlands*
karacak@gmail.com

An essential feature of what are called deep machine learning (DML) models is the opacity of their internal working that makes it impracticable for humans to understand how these models process big data sets and thereby provide predictions. In order to build public trust in the societal applications of DML models, it seems necessary that the decisions based on the results (or predictions) of these models be accountable. On the part of ML modelers, this entails the responsibility that they should provide the relevant stakeholders involved in societal applications of DML models with adequate explanations regarding the credibility of these models.

In this paper, I will suggest that the credibility of DML models and thus their accountability can only be claimed with respect to their intended purposes. Since deep ML models used in societal applications are predictive models, their intended purpose can be generically defined as providing accurate predictions about big data sets for the right reasons. I will argue that the accountability of DML models should be evaluated with respect to their adequacy for their intended purposes. To this end, I will draw upon Wendy Parker's recent account (Parker 2020) that characterizes the evaluation of a scientific model in terms of its adequacy for its intended purpose, rather than solely in terms of its representational accuracy that concerns its fit to the available empirical evidence. In Parker's account, in order for a model to fulfill its intended purpose, it must satisfy the constraints imposed by the following factors: target system (T), relevant background conditions (B), underlying scientific methodology (M), and prospective users (U).

I will argue that demonstrating accountability of DML models requires explanations that account for how these models provide their predictions. I shall point out that because of opacity, the required explanations can only be in the form of inferences that are drawn from the collection of the features of DML models that are interpretable by humans (Montavon et al. 2018). Based on Parker's account, I will also argue that demonstrating accountability of DML models also requires explanations as to whether the interpretable features of DML models, by virtue of which they provide accurate predictions, satisfy the constraints imposed by the features of T, B, W and U, showing if these models provide accurate predictions for the right reasons. This in turn requires further explanations that are different from those required to account for the predictions of DML models, suggesting that the explainability of the predictions of DML models is a precondition for the accountability of these models. In order to illustrate the foregoing claims, I shall consider the case of DML models constructed to predict hospital readmission of patients (e.g. Huang et al. 2019).

References

Huang, K., Altosaar, J., and Ranganath, R. (2019). ClinicalBERT: Modeling clinical notes and predicting hospital readmission. arXiv: 1904. 05342.

Montavon, G., Samek, W., and Müller K. R. (2018). Methods for interpreting and understanding deep neural networks. Digital Signal Processing, 73:1-15.

Parker, W. S. (2020). Model Evaluation: An Adequacy-for-Purpose View. Philosophy of Science, 87: 457–477.

---

## From plant-based lubricants to food grade oil: What standardization and platform thinking in bioengineering can tell us about knowledge integration

*Catherine Kendig*

*Michigan State University, United States*

kendig@msu.edu

Biological engineering applies engineering techniques and computational models to understand, modify, and construct biological pathways and products. It also relies on a suite of engineering aims and principles. Perhaps the most influential of these is standardization. Standardization is required for the development of what are called ‘platform technologies’, suites of tools and concepts that are useable over a range of applications. What makes platform technologies workable across a number of different applications is that the suite of techniques, underlying concepts, and normative constraints are either transferable or translatable from one field to another. As such, the development of a platform technology requires the practical integration of a nexus of epistemic, technological, and normative goals that shape the sorts of bioengineering activities utilized, the choice of products produced, and the methods by which the products can be measured and evaluated.

To illustrate this practical integration process, I rely on the transformation of the Canadian rapeseed plant (Brassica rapa and Brassica napus) into double-zero rapeseed oil (canola). In North America, the rapeseed plant was primarily used in the Second World War to produce plant-based lubricants rather than as a food grade oil due to its high levels of erucic acid and glucosinolates (Busch & Juska 1997). The purpose of reengineering rapeseed was to reduce the content of these two potentially toxic compounds to make it desirable as an edible oil. But prior to any attempt to breed low erucic acid rape, an instrument that could measure the content of the erucic acid in a seed was needed. Although techniques such as gas-liquid chromatography could be used to measure gasified substances, the device was not useable for analysis of fats like those from oilseed rape. Because of this, the possibility of developing double-zero rapeseed oil containing 0.4% erucic acid and 15 micromoles of glucosinolates per gram relied on first developing a means to measure it (Juska et al. 1997: 18). The transformation of oilseed rape to canola not only relied on the development of instruments that could measure chemical content, but also on tools for grading, and universal quality standards.

The transformation of oilseed rape from inedible erucic acid rich to edible rapeseed relied on platform thinking that included both the normative assessment of rapeseed as suitable edible oil as well as the development of universal standards by which to judge, measure and make uniform the quality of oilseed rape. The goal of making the highly variable rapeseed edible effectively generated a new ontological entity through standardization. In virtue of this standardization—it became an internationally traded commodity. Following discussion of this case, I explore the role of standardization and discuss how the pursuit of platform technologies and platform talk in bioengineering research affords a means of understanding knowledge integration for philosophy of science by providing alternative ways of understanding how (not just knowing that) knowledge integration is possible.

---

## Managing Performative Models: Methods from Social Science and Proposals from Philosophy

*Donal Khosrowi*

*Leibniz University Hannover, Germany*

donal.khosrowi@philos.uni-hannover.de

Scientific models can be performative: in addition to serving various epistemic purposes, they can also causally affect phenomena, such as when agents' behaviors change in response to model predictions. In recent years, philosophers have made important progress in delineating different forms of performativity and characterizing the problems they can pose, such as when the predictions issued by models are self-defeating and compromise models' epistemic functioning.

The existing literature offers two broad types of response to performativity. First, to maintain models' predictive performance in the face of performativity one can attempt to endogenize agents' behavioral response in the model. This approach has been pursued by social scientists as early as the 1950s and currently enjoys renewed interest. A second approach was recently outlined by philosophers in the context of epidemiological models informing policy response to the SARS-CoV-2 pandemic. They emphasise that performativity can sometimes be understood as a desirable model attribute, e.g. when model predictions, such as that critical care demand will exceed capacity, steer the public's behavior in desirable ways. This approach hence embraces (some forms of) performativity.

In this paper, I argue that neither approach is fully compelling. The philosophers' approach recognizes that performative models may have desirable performative features, but says too little on how to adjudicate models' epistemic and performative roles when they are in tension. Specifically, while it might seem plausible to appraise models post-hoc for having made important performative contributions (e.g. helping agents manage their response to a new wave of SARS-CoV-2 infections), constructing models specifically to meddle with agents' behaviors would threaten the epistemic integrity of models.

The social scientists' approach, by contrast, disregards important value-related considerations. Specifically, by 'endogenizing away' agents' behavioral response to better align predictions with actual behaviors, it neglects the potential real-world pragmatic benefits that performative models can harbor. And since endogenizing can prevent such benefits from obtaining, endogenization is a choice that itself involves substantive value-judgments.

With neither option fully compelling, I offer some constructive proposals for managing performativity, i.e. realizing models' performative potentials, while ensuring that their epistemic integrity remains uncompromised. I especially focus on carving out a clearer division of labor between model builders and model users to help keep value-influences from illegitimately meddling with the production and use of models to inform policy. On this view, various important decision points concerning model construction and use must be kept independent of researchers' and decision-makers' expectations and hopes regarding potential performative effects. What is more, while decision-makers may legitimately make value-laden choices about how to interpret model outputs, how to use them in decision-making, and how to communicate their decisions to the public (including how these are justified by model outputs), they must refrain from suggesting that their decisions follow straightforwardly from model outputs (e.g. claiming that they merely 'follow the science'). This is to ensure that models don't carry excessive justificatory burden in grounding value-laden choices.

---

**Materiality and material modeling in earth-scientific experiments**

*Maarten G. Kleinhans and Henk W. de Regt*
*Descartes Institute & Earth Simulation Laboratory, Universiteit Utrecht, Netherlands*
M.G.Kleinhans@uu.nl

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

Morgan (2003) has advanced a typology of experiments ranging from 'ideal lab experiments' to 'mathematical model experiments', with special attention for the role of materiality. She argues that in between these two extremes hybrid experiments exist: 'virtually' and 'virtual' experiments. While this may clarify experimental practices in physics and chemistry, where employed materials often are the same as targeted in the world, we submit that her account does not fit earth-scientific experimentation, for two reasons. First, its targets involve much longer timescales and larger spatial scales than can possibly be implemented in real-world experiments. Second, the employed materials often differ from those relevant to the target phenomena, but are chosen because they enhance similarity of relevant dynamics of the target system. Scale experiments differ from ideal lab and virtual/virtually experiments in Morgan's sense, because the similarity is not in the material itself. This raises the questions on the basis of what considerations experimenters select materials, and how these relate to the target system.

We review the literature of experimental tectonics, geomorphology and civil engineering, which operate on spatio-temporal scales of 1:1,000,000, 1:1,000 and 1:10 respectively. We find that material choices are made on the basis of (at least) three scaling considerations. The first is pragmatic: the material must be available, affordable and safe to use. The second and third are that experimental scaling can be done geometrically and dynamically. Which of these prevails depends on the explanatory target, the scale, and tradition in the various subfields of the earth sciences. In particular, civil engineering follows scaling rules to model systems with the geometry and dynamics of a specific location, whereas tectonics employs analog models for much larger time- and spatial scales wherein only a more select set of variables can be dynamically scaled. As a result, only a select set of processes and properties are represented in manipulable material models. Our analysis adds a dimension of materiality, perpendicular to Morgan's typology of experiments: similarity of materials in the target system to that in the model reduces with increasing scale of experiments, in order to maintain the dynamics that scientists deem most important in the target system.

References

Morgan, Mary S. (2003). Experiments without material intervention: model experiments, virtual experiments, and virtually experiments. In: H. Radder (ed.), The Philosophy of Scientific Experimentation (University of Pittsburgh Press), 216-235.

---

## Empirical Evidence for the Explanatory Language in the Neurosciences

*Daniel Kostic*[a] *and Willem Haiffman*[b]

[a,b]*Institute for Science in Society (ISiS) at Radboud University, Netherlands*

[a]daniel.kostic@gmail.com; [b]w.halffman@science.ru.nl

The literature on scientific explanation in the philosophy of science has been dominated by the idea of mechanisms (Carl F. Craver 2007; Bechtel and Richardson 2010; Glennan 2017). The basic idea can best be captured by the following definition of a minimal mechanism (Glennan 2017, 17):

> A mechanism for a phenomenon consists of entities (or parts) whose activities and interactions are organized so as to be responsible for the phenomenon.

The new mechanist philosophers often claim that all explanations in life sciences are mechanistic in the above sense, or at the very least that they conform to various degrees of completeness of this definition, e.g. there could be full-fledged mechanisms, partial mechanisms or mechanistic sketches (Piccinini and Craver 2011). Furthermore, anything that doesn't fit this definition, or a degree of completeness thereof, is not an explanation at all (Craver 2016). We call this set of claims "explanatory imperialism". But such extraordinary claims require extraordinary evidence, which so far wasn't forthcoming. The importance of empirical evidence about pervasiveness and uses of "mechanisms" in life sciences is particularly needed because examples and case studies that are used to illustrate new mechanists' claims cannot represent a statistically relevant sample, even if taken all together. Furthermore, given that they are admittedly handpicked, a robust quantitative and qualitative bibliometric study of the large body of relevant literature that we conduct in this paper will put such claims into perspective by showing:

1) To what extent exactly do explanatory language patterns in neuroscience corpora conform to the accepted definitions of mechanisms and mechanistic explanation in the philosophy of science?

2) What is the pragmatics of uses of these notions that do not conform to the accepted definitions of mechanisms and mechanistic explanation in the philosophy of science literature?

In conducting this study, we will employ the following methodology. In the first step, we define search strings for identifying explanatory language patterns, and then use these strings to search through the large neuroscience corpus downloaded from the bioRxiv repository. In the second step we qualitatively analyze detected patterns in the corpus. The purpose of the qualitative analysis is to classify these uses. Finally, we argue that the proposed methodology will provide comprehensive and empirically grounded insights into the debate on explanatory imperialism.

References

Bechtel, William, and Robert C. Richardson. 2010. Discovering Complexity: Decomposition and Localization as Strategies in Scientific Research. MIT Press ed. Cambridge, Mass: MIT Press.

Carl F. Craver. 2007. Explaining the Brain: Mechanisms and the Mosaic Unity of Neuroscience. Oxford: New York: Oxford University Press: Clarendon Press.

Craver, Carl F. 2016. "The Explanatory Power of Network Models." Philosophy of Science 83 (5): 698–709.

Glennan, Stuart. 2017. The New Mechanical Philosophy. First edition. Oxford: Oxford University Press.

Piccinini, Gualtiero, and Carl Craver. 2011. "Integrating Psychology and Neuroscience: Functional Analyses as Mechanism Sketches." Synthese 183 (3): 283–311.

**How can Critical Contextual Empiricism accommodate criticism of epistemic communities' goals?**

*Teemu Lari*
*University of Helsinki, Finland*
teemu.lari@helsinki.fi

Helen Longino's Critical Contextual Empiricism (CCE) is an influential normative account of science. However, Longino's exposition of CCE in Science as Social Knowledge and The Fate of Knowledge contains an unexplored tension, even an apparent inconsistency, that threatens the practical applicability of CCE to the evaluation of knowledge production by epistemic communities. In this paper, I explicate the tension and suggest a resolution.

This is the dilemma: On the one hand, According to Longino, the cognitive goals of epistemic communities should be open to criticism. The requirement of an ongoing process of "transformative criticism" applies not only to assumptions involved in research but also to questions about what kind of knowledge the community should aim to produce. On the other hand, the contextualism of CCE entails that all appropriate criticism of the commitments of an epistemic community must be "relevant to their cognitive and practical aims", so a community need not be responsive to criticism that does not "affect the satisfaction of its goals". But clearly, a criticism to the effect that a particular goal should be abandoned is anything but helpful in the pursuit of that very goal. Thus it seems that according to CCE, criticism of cognitive goals of epistemic communities is required but difficult to present in an acceptable way.

The practical relevance of this dilemma is manifest in the criticisms that feminist economists have voiced against mainstream economics. Some feminist economists argue that economics needs qualitative methods in the study of inequality and thus should count among its cognitive goals the pursuit of the kind of understanding provided by those methods. This criticism has evoked little by way of response from mainstream economists. But such criticism is arguably not relevant for mainstream economists' pursuit of what they see as the goals of economics, so the neglect seems to be justified by CCE – a result certainly not intended by Longino.

To resolve the dilemma, I argue that criticism of a community's goals that appeals to ethical and societal values should count as relevant and thus should require a response regardless of whether the criticism advances the community's current goals. This result illuminates an issue that Longino explicitly postpones for further study in The Fate of Knowledge: She wonders whether the "public standards of argumentation" norm included in CCE should be understood differently when the aims of research are debated, compared to when the debate concerns facts that the research tries to uncover. The answer is: yes, there is a difference. A strict reading of the requirement that all criticism invoke some standards of argumentation that are conducive to the community's goals entails that the room for acceptable criticism of the goals themselves is drastically reduced. This would be in stark contrast to the spirit of CCE – that the value commitments involved in scientific research should be uncovered and tried in an inclusive discussion.

---

### Model descriptions and imagination

*Aki Lehtinen*
*Nankai University, China*
aki.lehtinen@helsinki.fi

A prominent fictional account of scientific modelling posits that model descriptions are to be interpreted as props that prescribe imagining fictional truths about the model. This account is usually taken to be motivated by the scientific 'face-value practice': modellers often talk as if their models were concrete entities even though they are clearly describing systems that do not exist in the real world. Although some recent versions define the model as model descriptions and their content (Salis 2019; Salis, Frigg & Nguyen 2020), they continue to distinguish between the model and the model descriptions because the model lies in the modeller's imagination. Model descriptions merely provide the props that govern how we are to imagine the model in a game of make-believe.

This paper provides a set of criticisms of the fictional view of modelling, and proposes an account of modelling in which one does not distinguish between models and model descriptions. The proponents of this view seem to be making a case that imagination has an epistemic role in modelling, and the very idea of appealing to fiction is in part motivated by this epistemic role. In their account of the process of modelling the modeller first writes down the 'original' model descriptions, and then generates implied fictional truths about the model via the 'principles of generation'. According to the account, the principles of generation thus play an important epistemic role in modelling because they enable deriving results from models.

As acknowledged by some fictional accounts, Walton's (1990) principles of generation (the reality principle and the mutual belief principle) cannot be applied in scientific modelling because they would lead to unacceptable results. Hence, the fictionalists have proposed that in science such principles are replaced by discipline-specific laws or mathematical principles. They then face the following dilemma: if such laws and principles, together with the model descriptions, allow different modellers to come up with different fictional truths, one ends up with the problem of intersubjective disagreement (Weisberg 2013). If, on the other hand, different modellers must come up with exactly the same results and interpretations - and I think that this best describes their current view - then deriving the results from model descriptions has become epistemically irrelevant. It is just a matter of calculating what follows from the mathematical equations. It is misleading to talk about an original model description which requires some mathematical principles or laws for generating results. A model is simply not yet ready if it does not suffice for generating results without further formal or conceptual resources.

The root of the problem is that, because the model descriptions are to be interpreted as props for the imagination, it is supposedly employed only after the model descriptions have already been written down. But in scientific practice imagination is needed for coming up with the right kind of model descriptions in the first place, and this is where the relevant epistemic action is. This means that imagination is supposedly needed only when the model is ready and all the learning has already taken place. Employing the Waltonian account in scientific modelling is seriously flawed because the resulting account misplaces the epistemic role of imagination in modelling.

---

## Evidence in Evidence-based Management

*Bert Leuridan*

*University of Antwerp, Belgium*

Bert.Leuridan@UAntwerpen.be

The idea of evidence-based management (EBMgt) has been developed by Denise Rousseau in a series of articles (2006, 2018, 2020) and in the book Evidence-Based Management: How to Use Evidence to Make Better Organizational Decisions (2018, co-authored with Eric Barends). EBMgt is needed, they claim, because too many organizational decisions fail because of “managers who rush to judgement, impose their preferred solutions, fail to confront the politics behind decisions, ignore uncertainty, downplay risks, and discourage search for alternatives.” (Rousseau 2018, 136) The proposed remedy is to make management decisions “through the conscientious, explicit and judicious use of the best available evidence from multiple sources [...] to increase the likelihood of a favorable outcome.” (Barends & Rousseau 2018, 2) To that effect, they offer practical recommendations which managers could implement in their companies. They distinguish ‘evidence’ (defined as information supporting or contradicting hypothesis) from mere ‘data’ (numbers, words, figures, ...) and from mere ‘information’ (defined as data relating to something or someone and considered meaningful or useful) (Rousseau 2018, pp. 176-177). In their view, four sources of evidence are relevant for management decisions: ‘evidence from the scientific literature’, ‘evidence from the organization’, ‘evidence from practitioners’ and ‘evidence from stakeholders’.

Rousseau’s and Barends’ project is very useful and promising, but there is room for improvement, as their conception of evidence has a shortcoming: they characterize it a as a two-place relation between information on the one hand and a hypothesis on the other, while evidence should be seen as a three-place relation between a method, information and a hypothesis. Information can only support or contradict a claim, assumption or hypothesis if it was gathered using a method that minimizes bias. I will proceed as follows: 1. Briefly introduce Rousseau’s and Barends’ EBMgt-project. 2. Zoom in on their data-information-evidence tripartite and on their four sources of evidence. 3. Show that evidence characterized as a two-place relation between information and a hypothesis is problematic and a three-place characterization is needed (using one or two toy examples). 4. Briefly discuss different types of inductive reasoning in the special sciences (1° parameter estimation in the social sciences and 2° biomedical causal discovery) to show that the basic insights from 3. also apply to scientific practice; and offer a three-place characterization of evidence for parameter estimation and causal discovery (taking into account the types of bias that should be avoided or minimized in their respective contexts). 5. Distill a generic, overall approach to evidence as a three-place relation, dubbed the ‘logico-procedural approach to evidence in science’, which highlights both the need for an adequate logical, mathematical or statistical relation between information and hypothesis and the importance of using the right procedures for gathering information. 6. Review Barends and Rousseau’s book Evidence-Based Management: How to Use Evidence to Make Better Organizational Decisions (2018) and show, for each of the four sources of evidence they distinguish, that several of their practical recommendations are in fact bad advice; and offer improved recommendations instead.

In short, by offering a better, three-place account of evidence we endeavour to contribute to the EBMgt project.

References

Barends, Eric, Rousseau Denise (eds.), Evidence-Based Management : How to Use Evidence to Make Better Organizational Decisions, 2018, New York – London: Kogan Page Ltd, xxix, 351p.

Rousseau, Denise M. "Is There Such a Thing as "Evidence-Based Management"?" Academy of Management Review (2006) 31.

Rousseau, Denise M., "Making evidence-based organizational decisions in an uncertain world", Organizational Dynamics (2018) 47, 135-146.

Rousseau, Denise M., "The Realist Rationality of Evidence-Based Management", Academy of Management Learning & Education, 2020, vol. 19, No 3, 415-424,

---

## Reproducibility in animal-based research

*Simon Lohse*

*Universität zu Lübeck, Germany*

simon.lohse@uni-luebeck.de

Reproducibility issues in animal-based research are widely seen as a main driver for the translational crisis in biomedicine – where more than 90% of therapeutic agents that were successful in pre-clinical testing fail in early clinical trials. This has fueled critical debates on the legitimacy of animal experimentation in science and society. The observed low degree of reproducibility in biomedical research has also been discussed by philosophers of science interested in the reproducibility/replication crisis in science. The mainstream consensus concerning underlying causes for scientific irreproducibility is neatly summarised in the *Stanford Encyclopedia of Philosophy* article on the matter, which states that "[t]he causes of irreproducible results are largely the same across disciplines [. . . ]" (Fidler & Wilcox, 2018). This assessment resonates with recent meta-science work on animal-based research in biomedicine, which diagnoses research problems that seem mostly generic. Animal-based research is criticised for the high prevalence of publication bias, biased research designs and well-known questionable research practices, such as p-hacking (Ioannidis et al., 2014).

In this talk, I will scrutinise this view on reproducibility issues in animal-based research in biomedicine. I will claim that there are several aspects specific to animal-based research that are relevant to the (ir)reproducibility of results but have as of yet not been thoroughly analysed in philosophy of science. This talk aims to take first steps toward such an analysis. I will start by mapping out the discussion on the reproducibility of scientific results in animal-based research and review the most commonly mentioned (generic) causes for irreproducibility in this context. Next, I will discuss two challenges to reproducibility that are specific to animal-based research but have not received much attention in philosophical debates: First, I will sketch methodological challenges to standardisation practices in animal-based research. While the received view assumes the need for high standardisation in biomedical research to increase the validity and robustness of results, several animal researchers have argued that too much standardisation might be part of the problem. I will support this claim with examples from animal experimental practice, such as housing conditions in the laboratory, and relate it to recent discussions regarding the role of implicit knowledge in experimental research. Second, I will discuss what I call "ethico-epistemic trade-offs" in research practice. These trade-offs are a consequence of the controversial nature of animal experimentation and its strict regulation and manifest in situations where epistemic and non-epistemic values are – or appear to be – in conflict, such as when the reduction of the number of animals used in an experimental setup (according to the 3R principle: replace, reduce, and refine animal experiments) threatens the statistical power of the experiment. In the concluding part of my talk, I will argue that my analysis can contribute to a more pluralistic and nuanced picture of local challenges to scientific replication in philosophy of science debates, as would analyses of scientific irreproducibility in other (sub-)fields, and draw out normative implications of my discussion for animal-based research and its governance.

---

## Null Hypothesis Statistical Testing and Psychology – a Case of Bad Scientific Practice

*Matthew Lund*

*Rowan University, United States*

`lund@rowan.edu`

Much SPSP research works under the assumption that contemporary scientific practice embodies some of science's epistemic norms. As a result of this high esteem for practice, it is rare for a scientific field's practice to be judged as epistemologically defective. However, if practice is a significant component of science, and science can go wrong, practice could well be the culprit. This paper argues that psychology's practice is epistemically defective. While psychology uses many of the same statistical tools as other fields, it currently lacks the practical feedback loops to ensure that its tools and data are used responsibly. If practice is defined as "organized or regulated activities aimed at the achievement of certain goals", we can understand the practice of psychology as being centrally concerned with the goal of maximizing publication rates, eschewing thereby many traditional epistemic practices.

For much of the past decade, the field of psychology has been rocked by a replication crisis. (Pashler and Wagenmakers, 2012) Many published results that met the standards for significance, as defined by psychology's methods, have proved not to be reproducible. (Nuzzo, 2014) There have been two main theories of reform within psychology. The first blames most of the problematic features of the discipline on dishonest or questionable research practices (QRPs). For instance, there have been notable cases of individual fraud; also, there have been many cases of hiding data that does not lead to a certain p-value for a hypothesis (so-called "p-hacking"). Another line of critique calls into question the legitimacy of Null Hypothesis Significance Statistical Testing (NHST) itself. Such critiques explore the dubious historical origins of NHST and show how the p-value is not – contrary to the opinions of many researchers – the touchstone to replicability and other notable theoretical virtues.

While there certainly are many valuable lessons to be learned from both of these lines of inquiry, they do not take into sufficient account the practical environment of psychology. All scientific fields suffer, in varying degrees, from fraud and questionable research practices. Moreover, many other fields employ the statistical tools of NHST in much the same way as psychology. Nonetheless, such fields – with a few notable exceptions – have not been vitiated by abuses constituting a crisis.

Here is a list of practice defects endemic to contemporary psychology:

1) The majority of psychologists interpret central statistical variables incorrectly. For instance, the majority of psychologists surveyed committed to at least one of the following false beliefs: a. that a study having a p-value below 0.05 implies that the chance of replicability is 1-p, b. that a p-value below 0.05 proves the reality of an effect, or c. the probability of the alternative hypothesis (to the null hypothesis) being true is 1-p. (Gigerenzer, 2018)

2) The collection and evaluation of data is ordinarily done by the same (interested) parties. This is in contrast, for example, to NIH sponsored clinical trials, which require evaluation by an independent statistician as part of the research process.

3) Having a p-value below 0.05 is generally an essential condition for publication, and other aspects of the experimental situation are therefore

not considered important.

4) As a field, psychology values the publication of research most highly. Psychology is not particularly unified, and new studies are largely independent of other work in the field. Thus, should a study reporting false results be published, it will be unlikely to conflict with extant studies, and hence will not be detected through consistency testing.

This paper advocates that psychology borrow some "best practices" from other fields to alleviate its crisis. For instance, p-values ought not to be the sole determinant of whether a study is publishable, statistical evaluation should be done by independent statisticians, psychologists should be better trained in statistical methods, full data sets of studies ought to be published, and preprints of studies should be encouraged.

References

Gigerenzer, Gerd. 2018. Statistical Rituals: The Replication Delusion and How We Got There. Advances in Methods and Practices in Psychological Science. 1 (2): 198-218.

Nuzzo, Regina. 2014. Scientific Method: Statistical Errors. Nature. 506: 150-152.

Pashler H, and EJ Wagenmakers. 2012. Editors' Introduction to the Special Section on Replicability in Psychological Science: A Crisis of Confidence? Perspectives on Psychological Science: a Journal of the Association for Psychological Science. 7 (6): 528-30.

---

## Participatory modeling: Does it solve the problems of uncertainty and values in sustainability science?

*Miles MacLeod and Michiru Nagatsu*

*University of Twente, Netherlands*

milesfromanywhere@gmail.com

Modern modeling methods in the environmental sciences and sustainability science – such as climate science – attempt to provide accurate predictions of future climate and resource availability and supply based on current stocks and projections of future behaviors in order for policy makers and the public to make informed decisions on how best to manage human behavior and resources. Philosophers, STS researchers and others give two related criticisms – amongst others - of these "hard" uses of modeling. Firstly reliable predictions (and reliable measurements of uncertainty) from complex models are hard to obtain, and the capacity for models to play these kinds of roles for complex environmental systems is severely limited. Secondly, as philosophers of science have pointed out, building these models requires unavoidable often implicit value-laden decisions in design and interpretation. These decisions are not necessarily well understood by modelers, nor well represented to stakeholders, leading to criticisms particularly in the context of environmental and sustainability sciences, that the selection of modeling platforms are not neutral with respect to ethical questions. Deeply embedded traditional approaches, such as those arguably used in integrated assessment modeling reinforce particular ethical frameworks. However in these sciences – particularly sustainability science - there are both high stakes and deep disagreement over the right ethical frameworks to apply.

In this paper we argue that many modeling practices exist in contemporary environmental and sustainability science that can be understood as a reaction to both these two concerns; namely a concern with predictiveness and accuracy, and with value-ladenness. These fall under various headings, in particular "participatory modeling". Participatory models involve stakeholders deeply in the model development process including the selection for instance of relevant variables, the modeling goals etc. Stakeholders then run the model with scientists and explore outcomes. Since stakeholders participate in the design process in theory and have control over which values and goals are represented - and also gain knowledge of how the model works and by virtue how their systems work - they should be more willing to be bound by the outcomes. This stands in contrast to a more traditional approach in which scientists use a more established framework – such as an economic optimization – and measure the relevant values from stakeholders they need. We critically examine the extent to which participatory modeling can be a solution to both these problems mentioned above using reported cases of participatory modeling. One particular concern, which has yet to be addressed in research literature, is that there is no evidence participatory modeling results are robust generally. Running a participatory process again with say different organizers and participants (and different modeling choices) will not always produce a similar outcome. This raises an essential ethical problems. Given participants may indeed feel more bound by such a process, is it reasonable to rely on non-robust outcomes? In fact trying to avoid modeling biases in these cases may in fact may just generate more severe ethical problems.

**Exploring the usage of epistemic terms in the scientific literature: A computational text-mining approach**

*Christophe Malaterre*[a] *and Martin Léonard*[b]
[a,b] *Université du Québec à Montréal, Canada*
[a]malaterre.christophe@uqam.ca; [b]leonard.martin@courrier.uqam.ca

Much research has been devoted in the philosophy of science to explicating highly-prized concepts such as "explanation", "theory", "law" or "model" among many others, resulting in a plurality of nuanced philosophical accounts (such as the DN account, the causal account, the unification account or the mechanistic account of explanation). The rationale for carrying out such conceptual analyses is to be found in the central epistemic roles that such concepts are taken to play in science: science, as we all know or have been told, is about finding the universal laws that govern nature, elaborating theories and models that capture the phenomena we encounter, or explaining the world we inhabit. Yet, do these central concepts of philosophy of science actually play such significant roles in the very practice of science? The objective of this contribution is to investigate the actual roles that some such epistemic concepts do have in science. To this aim, we propose to use computational text-mining approaches to analyze terminological occurrence patterns in scientific publications. For the sake of feasibility, we narrow down our study to six major epistemic concepts: "theory", "model", "mechanism", "explanation", "understanding" and "prediction". We measure actual terminological usage and relationships in a corpus of over 75000 full-text scientific articles of the biological and medical sciences (BioMed database). In particular, we measure term frequencies and reveal the semantic context of terminological usage by means of co-occurrence analyses. We examine additional contextual variations by analyzing how terminological usage varies depending on topical variations between disciplinary clusters of articles. The resulting terminological cartographies partly validate select philosophical intuitions but also suggest notable differences between philosophical reconstructions and the actual roles that concepts appear to be playing in the scientific discourse. For instance, while some usage of "mechanism" and "understanding" do indicate that mechanisms help us understand phenomena, the two terms appear to be much more often associated to stress that mechanisms are the things to be understood (and not the things that provide understanding); also, "mechanism" is rarely seen in the vicinity of "explanation", the latter being more frequently used in connection with modal markers such as "possible" (indicating a hypothetical status) and causal terms such as "effect". The term "model" noticeably appears in statistical and mathematical contexts, with frequent connections to "prediction"; absence of "explanation" and "understanding" in its vicinity seem to indicate that models have little explanatory power. Disciplinary contexts reveal additional variations in terminological: "theory" for instance is relatively often used in disciplines such as psychology and the health sciences, though much less in molecular biology. Overall, the text-mining approaches we use make it possible to sketch a conceptual map linking the six selected epistemic concepts to a broader set of terms that reveal the actual usage of these terms in the practice of the biomedical sciences.

---

## Scientific Understanding without Veritism

*Mariano Martín Villuendas*
*University of Salamanca, Spain*
marianomv@usal.es

Scientific understanding has been the subject of intense debate within studies devoted to the philosophy of science.

Several authors have addressed this problem by employing the conceptual tools belonging to traditional epistemology, i.e., making use of the epistemological notions of truth and knowledge (Frigg & Nguyen, 2019; Kahlifa, 2017; Strevens, 2008). These authors have concluded that, since true knowledge is obtained by means of explanations and because explanations are the necessary element for understanding, the latter concept must be evaluated in terms of truth. This approach has been subject to several criticisms, especially since the advent of philosophical studies devoted to scientific modeling (Potochnik, 2017; Suárez, 2009).

Given these latter difficulties, several authors (de Regt, 2017; Elgin, 2017) have developed an alternative approach to scientific understanding, adopting a pragmatic and contextual stance. The aim of the communication is to contribute to the latter approach by proposing a novel notion of scientific understanding (Author, 2021): someone is said to have understanding when (1) the condition of possibility exists to be able to (2) exercise a certain capacity or capacities, (3) grounded on the standards of acceptance of a given epistemic subcommunity of agents, with the explicit aim of satisfying, adequately, (4) certain cognitive interests/goals in an (5) efficient and coherent manner.

I will argue that scientific understanding always takes place within a given subcommunity of cognitive agents and thereby depends on very specific material and epistemic conditions. Its emergence depends on a very complex research environment that includes a variety of epistemic and non-epistemic elements that establish what should be considered valid (condition of possibility), significant (condition of exercise) and feasible (condition of modality). The condition of possibility states that in order to have the possibility of acquiring understanding, cognitive agents must be trained and able to respond to the epistemic and material elements that structure the corresponding space of reasons of a given subcommunity (Brandom, 1994). The condition of exercise refers to the meaningful goals set by a given subcommunity in which the corresponding epistemic agents work dialogically to articulate the intelligibility of the domain of inquiry, goals ranging from the grasping of explanations to the exploration of spaces of possibility. Lastly, the condition of modality serves to point out the dynamic and perfectible character of scientific understanding.

From the latter analyses, I will stress to what extent the account proposed departs from veritist and recent pragmatic analysis. First, against the grain of veritist accounts, that scientific understanding is structured by certain context-dependent capacities –know-how– and not by the possession of true theoretical knowledge –know-that. This know-how requires that the cognitive agents be able to know how to locate a given body of beliefs, practices and material/experimental skills already instituted in the corresponding space of reasons (Sellars, 1956/1997). Second, that understanding does not depend on grasping an explanation. A traditional assumption held by both veritists and pragmatists (de Regt, 2017) is that explanation is the only cognitively relevant goal. I argue that these two concepts are independent of each other.

---

## The epistemic status of atmospheric retrieval models in exoplanetary science

*Vera Matarese*

*University of Bern, Switzerland*

vera.matarese@gmail.com

Exoplanetary science provides an exciting frontier not only for astrophysics but also for philosophy of science. In particular, given that its modeling practices are still fluid, there is a genuine opportunity for philosophers of science to contribute to the discussion on their epistemic status and validity. This talk focuses on retrieval models, which are indispensable to overcome the lack of knowledge about exoplanet atmospheres (Madhusudhan 2018). Retrieval methods permit the inference of the composition of an exoplanet's atmosphere by exploring a wide range of possible atmosphere models and by evaluating which ones yield the best fit with the exoplanet's transmission spectrum. Typically, the first step is to make an initial guess of the physical and chemical state of the exoplanet's atmosphere, feed it into forward models derived from first principles, and obtain a synthetic spectrum. The second step consists in comparing the synthetic spectrum with the transmission spectrum and evaluating, with Bayesian statistics, whether the two spectra match. In case of a negative answer, the initial guess is modified and the resulting model is checked again through an iterative process. The epistemic status of retrieval models is problematic. First of all, there is some tension concerning their epistemic role. At times they are assigned a mere exploratory and heuristic role, at times they are considered epistemically significant, because of their explanatory power and representative role. Secondly, they suffer from the problem of incompatible models, as different retrieval methods may provide vastly different models for the same data set (Barstow et al. 2020). Arguably, the cause lies in their incompleteness and idealization, as retrieval models simplify atmosphere phenomena by omitting several sources of opacities and assuming the homogeneity of molecular abundances. In light of this, questions on their epistemic status are at stake. Are these models truth-conducive or are they mere heuristic tools? Can they be taken as sources of justified knowledge about the exoplanet atmospheres? I propose that these questions should be answered by regarding retrieval models as perspectival models (Massimi 2018). First of all, they play an exploratory function by exploring and carving out the space of possible combinations of atmospheric conditions that could generate the transmission spectrum under consideration. This exploratory role is tightly correlated to their modal function. Indeed, by informing us of the possible atmospheric compositions of an exoplanet, they deliver a modal knowledge about the target system. Moreover, they should be regarded as complementary rather than contradictory, given that each model provides a partial and incomplete representation of the target system due to their high level of idealization. However, a difference with other perspectival models is that retrieval models are not mere heuristic tools. Thanks to the fact that they operate with forward models and with an iterative process, they rely on a correct reasoning practice that can be truth-conducive, exactly in the same way eliminative reasoning is. The conclusion is that retrieval models are not merely heuristically acceptable but can be considered legitimate sources of epistemic beliefs.

## Can Local Knowledge Contribute to Our Understanding of Climate Change?

*Ryan McCoy*
*University of Kentucky, United States*
Ryan.McCoy@uky.edu

Tension between expert and local knowledge has taken a central role in recent debates concerning the observability of climate and climate change. Given that climate is understood in its scientific sense as an average of variables typically over a 30 year period and developed by rather complicated modeling, some scholars have argued in favor of invisibilism, or the view that climate change is undetectable from the standpoint of local observation (Swim, et al. 2009; Hulme 2009). Moreover, work in experimental psychology has indicated that local observations of climate change have tended to be biased, and there is concern that giving credence to lay observations of climate and climate change will result in evaluations that are as mercurial as the weather (Rudiak-Gould 2013). In contrast, visibilist scholars have argued that the effects of climate change can in fact be locally observed. The rationale for this position has been that stakeholders in indigenous and local communities with prolonged engagement with their environment possess the knowledge and acumen to detect climatic changes at a local level.

In this paper I argue for a qualified version of visibilism that addresses invisibilist concerns, and conclude that local knowledge can importantly contribute to our understanding of climate and climate change. As evidence for this claim, I draw on recent work undertaken by climate researchers in Spain at the Local Indicators of Climate Change Impacts (LICCI) project (Reyes-García, et al. 2020), as well as researchers working with indigenous communities in Australia and New Zealand (Green, et al. 2010, King, et al. 2008). I argue that this research shows that local knowledge can significantly contribute to our understanding of climate and climate change given that (1) local knowledge can provide data that is much more fine grain than is capable for regional climate modeling, (2) this data can be used to track and address impacts on biophysical systems, and (3) local knowledge can fill spatial and temporal gaps in instrumental climatic data.

Lastly, in my conclusion I explore the non-epistemic consequences of including local knowledge within our understanding of climate and climate change. I argue that citizen science initiatives like those undertaken at the LICCI importantly engage non-experts in the production of scientific knowledge. A consequence of this engagement is that it can both facilitate public trust in science, as well as improve our scientific understanding.

## Replication in Action: First findings

*Stephanie Meirmans, Jonna Brenninkmeijer, Maarten Derksen and Jeannette Pols*

*Amsterdam UMC/ University of Amsterdam, Netherlands*

s.meirmans@amsterdamumc.nl

In the last decade, concerns have been raised in several fields of research about the reproducibility of research findings. In psychology, for example, a large-scale effort to replicate 100 experiments could only reproduce about 40% of the original results. A systematic review of the medical literature showed that the reproducibility of pre-clinical research findings was even lower. Researchers declared a replication crisis in psychology and in medicine, and questions are increasingly being raised in other fields as well.

However, many philosophers and others have highlighted that the problem might not be as straightforward and that indeed many more issues are at stake. While reproducibility of results is generally considered an important touchstone of the validity of scientific claims, there are a number of complexities associated with replication and reproducibility that need to be taken into consideration. For example the problem that a replication study is never identical to the original, if only because it was done at a different time. Furthermore, could we learn from error? There is also discussion about the different kinds of replication and their respective value in different situations and disciplines. Instead of closely replicating the procedures of the original study in so-called 'direct replications', some researchers argue that it would make more sense to test the theory about these mechanisms in novel ways: so-called 'conceptual replications'. A more fundamental question is whether replication is a worthwhile goal in all fields of research.

In our work, we aim to better understand the practical and epistemological complexities of replication studies by ethnographically studying how replication research is conducted in practice and how their results are discussed in the research teams who perform them (and beyond). For this, we make use of a unique research funder experiment in which money was awarded specifically to do replication studies. From 2017 to 2019, in total 24 PI's had received money from the Dutch research funding organization NWO to perform replication research, and that across three different research fields: 1) the social sciences, 2) the medical sciences, and 3) the humanities. 21 PI's have agreed to work with us, and we added three extra humanities studies.

We now study the complexities of replication 'in action' and their epistemological underpinnings with an empirical ethics approach. This entails that we will explicitly (via interviews/ observations) investigate what researchers themselves think would be the right thing to do in a replication. What does each of them understand by 'good' or 'bad' replication studies and practices, and why? Does what is seen as a good replication result or practice vary across the fields, studies, materials or methods used? We analyse notions of 'good' or 'bad' replications empirically, in each unique situation.

We will here present some first findings on how replication studies are practically conducted, how they vary across fields, and what is typically at stake. A comparison of the unique study settings and circumstances of replication studies 'in action' will help to understand the epistemological claims that come with replication efforts across research settings.

---

## Vibrating Strings, Naturalism and Johann I Bernoulli's Reduction of Physics to Pure Mathematics

*Iulia Mihai*

*Ghent University, Belgium*

iulmihai@gmail.com

A large part of eighteenth-century vibration theory was published as journal articles. Inventories of chronologically ordered attempts and advancements have been made, and the truth of claims and the soundness of proofs have been assessed. This paper proposes one way of philosophically reappraising (some of) the mathematization practices underlying this material. Here, I focus on two journal articles by Brook Taylor and Johann I Bernoulli, which are published in the Philosophical Transactions (1713) and the Commentaries of the Petersburg Academy (1732), respectively. These contain a peculiar conception of musical strings in vibration, according to which the shape and the law of motion of the string are epistemically independent. This way of thinking about the string in motion was salient during the three decades before advanced differential techniques were introduced at the end of the 1740s. My argument is that this conception is not inert, but changes diachronically through Bernoulli's rearticulation in a different mathematical language. It is only by paying close attention to elements of the two mathematical practices (Bernoulli's and Taylor's) that the evolution of this conception can be delineated, analyzed and used for further historical assessment.

My analysis is indebted to Madeline Muntersbjorn's (1999) naturalistic proposal for integrating the history and philosophy of mathematics on the basis of an ontology of mathematical objects which evolve over time. On her account, evolutions can be assessed through changes in notation and accompanying "shifting ontological commitments". As I show, the curve describing the shape of the string in Taylor and Bernoulli's work is one such evolving object; in Taylor's notation it has existence, but no independent expression, whereas Bernoulli's notational practice gives it autonomy. But Muntersbjorn's naturalism, which methodologically emphasizes the inquiry into notational practices, can be pushed even further to illuminate mathematization practices. In connection to the string's law of motion when used to express the time, it becomes apparent that Bernoulli's extensive use of algebraic symbolism enables innovative notational interventions which result in a more robust handling of both physical and geometrical quantities. This way, Bernoulli steers clear of physical analogies in investigating the string's properties, whereas Taylor approaches the string (also) by drawing analogies with other mechanical objects on the basis of shared (physical and geometrical) properties.

An evolving conception of strings in vibration gives a glimpse into how Bernoulli performs as a reader of Taylor's text. Bernoulli's stance can be understood as reductive towards Taylor's physical (or natural philosophical) arguments, in favor of what the Bernoulli brothers called 'pure mathematics'. This is more subtle and philosophically relevant than Bernoulli simply transposing Taylor's occasional fluxions into the differential calculus.

---

## Stem Cells and the Microenvironment: Reciprocity with Asymmetry in Regenerative Medicine

*Guglielmo Militello and Marta Bertolaso*

*Universidad del Pais Vasco, Spain*

gugli.militello@gmail.com

Much of the current research in regenerative medicine concentrates on stem-cell therapy that exploits the regenerative capacities of stem cells when injected into different types of human tissues. Although new therapeutic paths have been opened up by induced pluripotent cells and human mesenchymal cells, the rate of success is still low and mainly due to the difficulties of managing cell proliferation and differentiation, giving rise to non-controlled stem cell differentiation that ultimately leads to cancer.

The limitations and the risks of stem cell therapy can be understood in the light of the components and architecture of tissue microenvironment into which stem cells are inserted. The constituents and the topological features of tissue microenvironment (e.g., cues, morphogenetic fields, and the architecture of the tissue extracellular matrix) play a role in stem cell fate, thus being essential to the success or the failure of stem cell therapies (Votteler et al. 2010; Wilems et al. 2019). However, the nature of the causal relationship between tissue microenvironment and stem cells has not been studied in detail. We address the causal relationship between the tissue microenvironment and stem cells in two case-studies: the cardiovascular regenerative medicine and the neuro-regenerative medicine. We focus on these case-studies because they represent the most important (and promising) current applications of stem-cell therapy, and because they highlight the conceptual relevance of tissue microenvironment for the success or failure of stem cell therapy.

We argue that tissue extracellular matrix and stem cells have a causal reciprocal relationship in that the 3D organization and composition of the extracellular matrix establish a spatial, temporal, and mechanical control over the fate of stem cells, which enable them to interact and control (as well as controlled by) the cellular components and soluble factors of microenvironment. At the same time, the reciprocal constraining action of stem cells and microenvironment is asymmetrical due to the asymmetrical causal relationship between ECM and stem cells. Thus, the very sense of stemness lies in the network of mutual constraints between stem cells and their microenvironment that ultimately explains (i) how and why stem cells can correctly proliferate, differentiate, and migrate in a specific niche and (ii) why scientists usually refer to the microenvironment role in terms of a modulatory capacity rather than a direct causal (i.e., mechanistic) influence.

These kinds of evidences open the way to a more dynamic analysis of the topological features that characterize the causal relevance of the context -or microenvironment- in space and time. In this sense, a relational account of stem cells implies a systemic one, where ‘system’ is the spatio-temporal coupling between stem cells and their microenvironment. We suggest that the current research programs in regenerative medicine can benefit from the theoretical tools provided by mechanobiology not only for understanding the mechanical properties and forces of the microenvironment, but also, and most importantly, for developing tissue engineering techniques for restoring, or at least improving, the functional organization and mechanical properties of the tissue extracellular matrix and hence stem cell fate.

## Calibrating Cosmic Dark Age Instruments

*Nora Mills Boyd*
*Siena College, United States*
nboyd@siena.edu

Calibration has a special role in the epistemology of experiment. Yet, when calibration is very difficult, some scientists suggest adopting a Bayesian approach that marginalizes over calibration parameters together with other unknowns, effectively erasing the distinctive epistemic role of calibration. This suggestion has arisen in connection with the calibration problems that researchers face in 21 cm cosmology. Drawing on lessons from the Hydrogen Epoch of Reionization Array, I will argue against adopting this Bayesian approach in 21 cm cosmology for the ambitious upcoming Square Kilometer Array. I claim that this argument clarifies the epistemic role of calibration in cases where calibrating the instrument is very challenging.

The special epistemic role of calibration can be clearly appreciated in light of the threat that the experimenters' regress poses to the epistemology of science (Collins 1992/1985). Especially when investigating new phenomena, how do researchers tell that their instruments are working properly? Often, experimentalists can find or devise surrogates for the signals ultimately of interest to them, and use those surrogates to calibrate their instruments. When a same-type surrogate is not available it can sometimes be possible to use surrogates that nevertheless possess the relevant features to effectively accomplish calibration. Signal injection in gravitational wave interferometry is an example of this strategy (Franklin 1997). Thus, calibration by suitable surrogate signals is what severs the experimenters' regress. While calibration is not sufficient to justify taking results an instrument produces seriously, it is often a necessary step.

21 cm cosmology uses radio arrays to detect the very faint signal of a hyperfine transition in cosmic neutral hydrogen. The aim is to use this signal to fill in an important gap in our understanding of the evolution of the universe. We have empirical access to the early universe thanks to the cosmic microwave background, and to the later universe via light from stars and galaxies. However, the cosmic dark ages after the universe cooled enough to become transparent but before structure formation had advanced sufficiently to produce luminous bodies, remains uncharted. 21 cm cosmology aims to map the signal from neutral hydrogen during that period of the universe's history. Calibration of a 21 cm cosmology instrument involves determining the complex frequency-dependent gain factors that transform the 'true' signal to the 'received' signal (Liu and Shaw 2020). Since for an array of $N$ antennas, one wants to determine both $N(N-1)/2$ 'true'

visibilities as well as $N$ gain factors from only $N(N-1)/2$ measurements, there are more unknowns than constraints. I will discuss approaches that have been proposed for addressing this problem, including fitting to a sky model, redundant calibration, and hybrid techniques (ibid.). I will argue that hybrid calibration techniques are more suitable than a Bayesian approach that treats gain factors together with other unknowns for justifying the epistemic significance of the results of 21 cm research because the hybrid techniques retain the crucial epistemic role of surrogate signals in a way that the Bayesian approach does not.

References

Collins, Harry. 1992/1985. Changing Order: Replication and Induction in Scientific Practice. University of Chicago Press.

Franklin, Allan. Calibration. Perspectives in Science. 5(1): 31-80.

Liu, Adrian and J. Richard Shaw. 2002. Data Analysis for Precision 21 cm Cosmology. Publications of the Astronomical Society of the Pacific 132:062001

---

## Internalizing Research Integrity: A Practice Based Project to Enhance Research Ethics Education

*Barton Moffatt*

*Mississippi State University, United States*

brm157@msstate.edu

Research integrity is central to successful epistemic practice in science. One consequence of the increase in research ethics regulation and the corresponding increase of research ethics bureaucracy is the unintended impression by some that research integrity is external to the practice of science. It is not unusual to hear researchers in certain disciplines refer to the bureaucracies of research ethics as the ethics police. This impression is unfortunate because people tend to defer ethical judgments if they perceive it to be someone else's responsibility. Some research in economics suggests that external interventions may undermine intrinsic motivations in a crowding out effect (Frey & Jegen 2001). The more researchers perceive research ethics to be a domain external to scientific practice and adversarial in nature the less they will view ethics as necessary for their practice. Additionally, they will be less likely to embrace the mantle of teaching research ethics as an integral part of science.

This paper argues that there is a need to redirect this dynamic by educating graduate students in a way that highlights the centrality of research ethics to scientific practice. One way to do this is to introduce a moral exemplar project in research laboratories that makes students learn more about their broader research communities and identify scientists who have built a reputation for particularly moral behavior. Ideally, this will lead to a discussion within the laboratory that identifies why the behavior was important to the community. Of course, not all graduate students are well situated in active research communities; these students would have the option of taking a historical approach and researching scientific moral exemplars in the history of their fields. I argue that this type of project will enhance research ethics education by emphasizing the centrality of research ethics to epistemic communities and blunt the force of externalizing bureaucracies.

References

Frey, Bruno & Jegen, Reto. 2001. Motivation Crowding Theory: A Survey of Empirical Evidence. J Econ Surv. 15(5).

---

## The Predictive Reframing of Machine Learning Applications: Good Predictions and Bad Measurements

*Alexander Mussgnug*

*University of Edinburgh, United Kingdom*

alexander@mussgnug.de

In the past decade, machine learning (ML) has evolved from a predominantly exploratory field to an increasingly established and more broadly used instrument of inquiry. Today, ML models are applied to tasks as diverse as the automatic segmentation of plant images, unemployment rate forecasting, or better modeling retinal sensory processing. Coextensive with the increasing use of ML models is the near-universal interpretation of their outcomes as statistical predictions. This finds expression, for instance, in Agrawal et al.'s seminal book Prediction Machines: "Because it [ML] is becoming cheaper it is being used for problems that were not traditionally prediction problems. Kathryn Howe, of Integrate.ai, calls the ability to see a problem and reframe it as a prediction problem 'AI Insight,' and, today, engineers all over the world are acquiring it." (2018, p. 23).

What Agrawal et al. commend as "AI Insight," I take as motivation to critically analyze how exactly ML developers reframe problems as prediction tasks. Focusing on the case of ML-enabled poverty inference, I explore how reframing a socioeconomic measurement problem as a statistical prediction alters the primary epistemic aim of the application. In poverty measurement, one seeks to quantify the poverty of a particular region. However, in the reframed predictive tasks, researchers aim to predict the hypothetical value of a given poverty metric.

I argue that this predictive reframing of machine learning applications to poverty measurement is neither epistemically nor ethically neutral, as it allows developers to externalize concerns critical to the epistemic validity and ethical implications of their model's inferences. This includes, but is not limited to, the question of which measurement of poverty is the right measurement for the given purpose. Instead of critically contextualizing and evaluating the ML model outcomes, for instance, through construct validation techniques common to measurement in the social sciences, the evaluation of supervised ML models often proceeds solely based on statistical correlation with a given measurement. However, I will show that just because a supervised ML model might display a high correlation with a given poverty metric, i.e., reliably solve the predictive task, it does not necessarily follow that the application also adequately addresses the initial measurement problem the model is marketed as solving. In other words, a supervised ML model might provide good predictions but bad measurements.

I further hold that the predictive reframing is not a necessary feature of supervised ML by offering an alternative conception of machine learning models as measurement models. An interpretation of supervised ML applications to measurement tasks as measurements internalizes questions of construct validity and ethical desirability critical to the original problem these applications are intended to and presented as solving. In doing so, this paper introduces an initial framework for further exploring epistemic and normative issues at the intersection of measurement and machine learning.

References

Agrawal, A., Gans, J., & Goldfarb, A. (2018). Prediction Machines: The Simple Economics of Artificial Intelligence. Harvard Business Press.

**Where memory resides: Is there a rivalry between molecular and synaptic models of memory?**

*Jonathan Najenson*[a] *and David Colaço*[b]
[a]*The Hebrew University of Jerusalem, Israel;* [b]*LMU Munich, Germany*
[a]`jonathan.najenson@gmail.com`; [b]`davidjcolaco@gmail.com`

In recent years, scientists (Langille and Gallistel 2020; Levin 2021; Gold and Glanzman 2021; Gershman et al. 2021) have argued that the substrate of memory is molecular. Some proponents of this molecular model take it to challenge the dominance of a synaptic model of memory, according to which the synapse is the substrate of memory. This rivalry might appear odd, even trivial, to philosophers. Why would there be a rivalry between molecular and synaptic models? Those familiar with reductionist and mechanistic explanatory accounts would be surprised to learn that these models are considered incompatible.

Supporters of the molecular model defend their position with two arguments. The first is Evolutionary: memory phenomena occur, via molecular mechanisms, in evolutionarily ancient organisms like single-cells and slime molds (Levin 2021). As such, the molecular mechanisms historically precede synapses, are wider in their explanatory scope, and are evolutionarily conserved in more complex organisms like humans. The second is Computational: synaptic models cannot adequately model the computational properties required from a memory mechanism, such as the ability to retain information for long periods of time (Gallistel and King 2009; Najenson 2021). Only a molecular substrate can account for these properties, so molecular models alone can explain complex memory phenomena.

In this talk, we address these arguments and what they tell us about the rivalry between the molecular and synaptic models. First, we distinguish the rhetorical and substantive aspects of this debate. Second, we discuss the possibility of integrating these models, highlighting how each argument relates to commitments regarding proximate vs. ultimate explanation, reductionism, and multi-level mechanistic frameworks.

In the second part of the talk, we address the targets and aims of each model. The molecular model has different targets and aims from a synaptic model. For instance, the molecular model addresses memory in systems that lack synapses (Colaço Forthcoming). Further, the aims of the evolutionary and computational arguments are distinct. The evolutionary-motivated aim of the molecular model is to account for memory phenomena that are not exclusive to complex organisms like humans, while its computational motivations are driven by commitments to classical computation that are unlike the commitments to a connectionist architecture akin to the synaptic model. These differences raise doubts as to whether these models share target systems.

Our analysis elucidates the rivalry between molecular and synaptic models of memory. Specifically, it highlights how the substantive disagreement between what these models aim to account for is a consequence both of how memory is conceptualized and where it is expected to occur.

References

Colaço, David. Forthcoming. "What counts as a memory? Definitions, hypotheses, and 'kinding in progress'.' To appear in Philosophy of Science.

Gallistel, C. R., & King, A. P. Memory and the computational brain: Why cognitive science will transform neuroscience. (2011) John Wiley & Sons.

Gershman, Samuel J., Petra EM Balbi, Charles R. Gallistel, and Jeremy Gunawardena. 2021. "Reconsidering the evidence for learning in single cells." Elife 10: e61907.

Gold, Adam R., and David L. Glanzman. 2021. "The central importance of nuclear mechanisms in the storage of memory." Biochemical and Biophysical Research Communications 564: 103-113.

Langille, Jesse J., and Charles R. Gallistel. 2020. "Locating the engram: Should we look for plastic synapses or information-storing molecules?." Neurobiology of Learning and Memory 169: 107164.

Levin, Michael. "Life, death, and self: fundamental questions of primitive cognition viewed through the lens of body plasticity and synthetic organisms." Biochemical and Biophysical Research Communications 564 (2021): 114-133.

Najenson, Jonathan (2021). "What have we learned about the engram?" Synthese 199 (3-4):9581-9601.

---

## Qualitative methodologies for philosophical research.

*Milena Padilla Sierra*

*Universidad Nacional Autónoma de México*

milenasierra18@hotmail.com

Despite the wide thematic that motivates reflections in the contemporary philosophy of science, the methodology to approach both traditional and current topics is usually solely philosophical, e.g., exhaustive theoretical reviews, evaluation of arguments and justifications, logic and rhetoric. Undoubtedly, these methodologies are extremely valuable. However, they can be enriched by incorporating methodologies to obtain information about the social world in a direct, controlled and intentional way to guide philosophical concerns (Villanueva, 2021). Thus, expanding the possibilities of philosophical research by asking questions that require, in addition to the appropriate analytical work, a direct and intentional approach to the social or everyday world. This work exposes the methodological design of my ongoing doctoral research in Philosophy of Science, aimed to answer the following question: what are the epistemic reasons involved in the epistemic dependence and its rupture possibilities during women's transition from hormonal contraceptives to other natural methods? The challenge posed by this primarily philosophical question is to take into consideration its social dimension and, therefore, design an adequate methodological approach. My research is divided in two parts that maintain a strong connection: fieldwork and conceptual grounding. For the former, it was necessary to collect and interpret the experiences of women who have switched from using hormonal contraceptives to other natural methods. To achieve this objective, I developed a methodological design drawing from qualitative research methodology (Vargas, 2012; Flick, 2015) that consists in the triangulation (Flick, 2012) of semi-structured interviews (Flick, 2007; Doody & Noonan, 2013), episodic interviews (Flick, 2007) and a feminist research approach (Harding, 1988; Acker, 2003). The fieldwork is supported by a robust theoretical framework that defines epistemic reasons as those that justify the beliefs of an agent A, either in a proposition or in an agent B (Hardwig, 1985; Sylvan , 2016; Broncano & Vega, 2020) in order to identify and analyze women's reasons to switch from hormonal to natural methods. By using the Atlas.ti software, the analysis will be directed to find the corpus for linguistic markers of the epistemic modality (Palmer, 1986; Sanmartín, 2009), as well as evidential markers (González, 2006; Sanmartin, 2009) and the linearity of the episodic events of the transition and the relationship that these have with the markers of the epistemic modality. Studying the epistemic reasons exhibited by the members of a social group regarding an episodic event in their life experience allows us to understand and outline the rational route that guides them to different epistemic positions (of others and of themselves at various moments of their lives) and, even, those discordant with those hold by the hegemonic epistemic authority.

## When to Stop Building Trust: Percieved Epistemic Injustice and the Limited Obligation of Institutions to Cultivate Trust in Science

*Tyler Paetkau*
*McGill University, Canada*
tyler.paetkau@mail.mcgill.ca

The ongoing Covid-19 pandemic has brought the issue of trust in science to the fore. Despite the demonstrated safety and efficacy of vaccinations against Covid-19 and access to vaccines, a significant portion of those in Canada and the United States resist vaccination. While traditional approaches have sought to ameliorate vaccine hesitancy through public education, recent scholarship has argued that the root of vaccine hesitancy is a lack of trust in vaccine-promoting institutions. Specifically, vaccine-promoting institutions are seen as being motivated by financial gain or racist ideologies rather than the public good. However, while trust-focused approaches offer legitimate strategies for increasing vaccine uptake, these approaches often directly conflict with public health measures imposed during the pandemic. In particular, policies such as vaccine passports, vaccine mandates for employment, and special taxes for the unvaccinated have all come under fire for supposedly contributing to distrust in vaccines and vaccine-promoting institutions.

This tension between competing public health concerns reveals a topic largely overlooked by trust-based approaches. That is, how far should public institutions go in their efforts to develop trust in science among the general public. Drawing on theories of procedural justice and epistemic injustice, I argue that public institutions have a normative obligation to cultivate trust in policy-relevant science. In particular, institutions have an obligation to be trustworthy, appear trustworthy, and to build trust through developing respectful relationships with the public. However, these obligations have limits. One such limit is instances of distrust in science rooted in perceived epistemic injustice. While instances of epistemic injustice are generally composed of both substantive and perceived epistemic injustice, I argue that these two elements can occur independently. As such, when distrust is rooted in an instance of epistemic injustice that lacks the substantive element, the institution has no obligation to cultivate trust.

# Technology, Representation, and Understanding

*Myron A. Penner[a] and Amanda J. Nichols[b]*
*[a]Trinity Western University, Canada; [b]Oklahoma Christian University, United States*
[a]myron.penner@twu.ca; [b]amanda.nichols@oc.edu

This paper presents a detailed comparative analysis between two precise, technologically driven pathways to representation and understanding in scientific practice: functional magnetic resonance imaging (fMRI) and electron microscopy (EM). As technologies of representation, both fMRI and EM contribute to understanding in their respective domains. However, we argue that EM does a better job of contributing to understanding material composition than fMRI does for understanding cognitive neuroscience. Moreover, we explain why this is the case by demonstrating that while both technologies follow a similar developmental pathway toward understanding, EM is more "sure footed" at each step along the path.

For both representative technologies, the pathway toward understanding can be charted in six distinct steps. The pathways begin with theoretical assumptions which provide the framework for developing the respective technologies. This is followed by an experimental intervention which yields measurement of data. Measured data are then subjected to interpretation which then yields a particular representation. Representations are then interpreted in a way that brings about a level of understanding. We appeal to this developmental framework at different points in arguing for the following two claims.

First, we argue that EM yields greater understanding than fMRI. Appealing to de Regt's construal of scientific understanding in terms of "intelligibility," we demonstrate that EM renders the phenomena it represents more intelligible than does fMRI. Second, we explain why this is the case by demonstrating that for the first three steps on the developmental pathway charted above (assumptions, intervention, and measurement), EM is advantaged in ways that fMRI is not. Compared to fMRI, EM's assumptions have more empirical support, EM's intervention's are simpler and more controlled, and EM's measurements are more precise. As a result, when compared to fMRI, there are fewer ways for interpretations of EM to be misconstrued, leading to more accurate representations via EM and, ultimately, greater understanding.

We conclude with some applications of this research to fMRI and future technologies of representation for understanding cognitive neuroscience. Use of existing fMRI technology can be enhanced with greater clarity and empirical support for the assumptions driving the technology. Moreover, greater emphasis on precise interventions relative to control groups will improve the quality of data measurement. This would assist subsequent interpretations of data in ways that enhance the quality of representations of neural networks and, ultimately, our understanding of them.

---

**How-possibly/how-actually and mathematical modelling: a change of perspective informed by the epistemology of mathematics in practice**

*José Antonio Pérez Escobar*
*ETH Zurich, Switzerland*
jose.perez@gess.ethz.ch

Recently the use of modal explanations (how-possibly explanations, or explanations that suggest that something is necessary or impossible) in science has been subject to increased attention by philosophers of science. Usually this kind of explanations is contrasted with how-actually explanations, which are merely concerned with non-modal descriptions of known facts (but some, focusing on slightly different nuances, some prefer to contrast how-possibly to why-necessarily or why-actually instead). More importantly, there are two main positions in the literature: one that considers that these two types of explanations are of a different kind (eg. Dray, 1957; Dray, 1968; Forber, 2010), and another that sees them as points in a continuum of certainty and empirical support (eg. Hempel, 1965; Brandon, 1990; Bokulich, 2014).

Recent philosophy of science has been concerned with scientific models which provide how-possibly explanations (Grüne-Yanoff and Verreault-Julien, 2021). In a similar line, some have noted the use of "minimal" models in the scientific practice (Grüne-Yanoff, 2009; Fumagalli, 2016; Batterman and Rice 2014). Moreover, mathematics is more and more used in models not just in physics, but also in economics and biology (eg. Pérez-Escobar, 2020). Therefore, it seems sensible to carefully study modelling and mathematical practices in order to shed light on the how-possibly/how-actually distinction.

This work is concerned with two main aims: 1) to challenge the how possibly/how actually division a la Dray-Forber in the context of mathematical modelling, and 2) to offer an alternative to the Hempel-Brandon-Bokulich continuum view. In order to achieve this, I will rely on a focus on the scientific practice and analyze the character of mathematical models within the framework of the later Wittgenstein's philosophy of mathematics.

Bearing a certain resemblance to Quine's holistic view of knowledge, Wittgenstein's late philosophy of mathematics understands mathematical models as rules of description and the meaning of mathematics as its use. Critically, certainty is not the only factor at stake. For instance, Euler's conjecture for polyhedra (the number of vertices V, faces F, and edges E in a polyhedron satisfy V + F - E = 2) is often not discarded in the advent of counterexamples, but can be used as a rule for what counts as a face or a vertex (Pérez-Escobar, 2022). I will argue that mathematical models are situated in a resilience continuum, which in essence is a how possibly/how actually continuum different from an information completeness or certainty continuum a la Hempel-Brandon-Bokulich. Last, I will make the case that mathematical models are not strictly "how-possibly" or "how-actually" models and their symbol arrays do not represent modality, but there are "how-possibly" and "how-actually" uses of those models and they can be used modally depending on different epistemic needs, in a practice and context-dependent manner. I will show this aided by a description of mathematical modelling practices in cognitive neuroscience, paying special attention to the development and use of mathematical models for the "brain compass".

References

Batterman, R. W., & Rice, C. C. (2014). Minimal model explanations. Philosophy of Science, 81(3), 349-376.

Bokulich, A. (2014). How the tiger bush got its stripes: 'how possibly' vs. 'how actually'model explanations. The Monist, 97(3), 321-338.

Brandon, R. N. (2014). Adaptation and environment. Princeton University Press.

Dray, William (1957). Laws and Explanation in History. Oxford University Press.

Dray, W. H. (1968). On explaining how-possibly. The Monist, 52(3), 390-407.

Forber, P. (2010). Confirmation and explaining how possible. Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences, 41(1), 32-40.

Fumagalli, R. (2016). Why we cannot learn from minimal models. Erkenntnis, 81(3), 433–455.

Grüne-Yanoff, T. (2009). Learning from minimal economic models. Erkenntnis, 70(1), 81–99.

Grüne-Yanoff, T., & Verreault-Julien, P. (2021). How-possibly explanations in economics: anything goes?. Journal of Economic Methodology, 28(1), 114-123.

Hempel, C. G. (1965). Aspects of scientific explanation (Vol. 3). New York: Free Press.

Pérez-Escobar, J. A. (2020). Mathematical Modelling and Teleology in Biology. In Research in History and Philosophy of Mathematics (pp. 69-82). Birkhäuser, Cham.

Pérez-Escobar, J. A. (2022). Showing Mathematical Flies the Way Out of Foundational Bottles: The Later Wittgenstein as a Forerunner of Lakatos and the Philosophy of Mathematical Practice. KRITERION–Journal of Philosophy.

---

## Ethicists into the lab – once again? How to conceive of a sound practice turn for bioethics

*Anja Pichl*

*University of Tübingen, Institute for Ethics and History of Medicine, Germany*

anja.pichl@gmail.com

Many contributors to the ethical debate on organoids call for a closer integration of ethics into scientific practice, whether in the form of ethics in the laboratory, "engineering ethics" (Hyun 2017) or "real-time ethics engagement" (Sugarman/Bredenoord 2020). This paper investigates the proposed methods for closer engagement with scientists brought forward. It discusses their possible implications for ethical theory and practice and asks for the conditions of successfully engaging with scientific practice as ethicists. It argues that closer engagement with science needs to be complemented by approaches and findings of social studies of science and philosophy of science. Both provide relevant resources for making sense of science as social practice in contrast to a still lingering idea of science as being largely separate from society and of being of ethical concern mostly with regard to its applications and downstream effects (Hilgartner et al. 2017). Whereas social science research opens up the blackbox of the manifold societal conditions and dimensions of scientific research, where norms and values are inextricably linked to specific practices, institutions, thought styles etc., philosophy of science provides tools and insights for understanding its dynamics and theoretical underpinnings, such as the role of models or cell concepts in organoid research (Fagan 2020) or the situatedness of knowledge more generally (Haraway 1988). These theoretical and societal aspects tend to be blanked out by bioethicists and scientific practitioners. However, they are crucial for identifying ethical issues and for understanding current transformation processes of which science, society and ethics are intricately intertwined parts. The case study of organoid research and its ethical debate is supposed to substantiate these claims. It serves furthermore to ask how the practice turn proposed by organoid ethicists relates to the practice turn in philosophy of science. Are there any relevant similarities and differences, any lessons learned by philosophers of science of possible interest to ethicists? What does the notion of "practice" encompass in philosophy of science, how are societal dimensions of scientific research taken into

References

Fagan, M.B. (2020): Organoide. Ein wesentliches Element in einem generativen Modellgefüge. In: Bartfeld et al. (Hrsg.): Organoide. Ihre Bedeutung für Forschung, Medizin und Gesellschaft. Baden-Baden: Nomos.

Haraway, D. (1988): Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective. In: Feminist Studies, Vol. 14 (3), 575-599.

Hilgartner, S., Prainsack, B. and Hurlbut, J.B. (2017): Ethics as Governance in Genomics and Beyond. In: Felt et al. (Hrsg.): The Handbook of Science and Technology Studies. 823-852.

Hyun, I. (2017): Engineering Ethics and Self-Organizing Models of Human Development: Opportunities and Challenges. In: Cell Stem Cell 2017 Dec 7;21(6):718-720.

Sugarman, J., Bredenoord, A.L. (2020): Real-time ethics engagement in biomedical research. Ethics from bench to bedside. In: EMBO reports 21: e49919.

---

## Integrating representational, exploratory, and design modeling

*Christopher Pincock*[a] *and Michael Poznic*[b]

[a]*Ohio State University, United States;* [b]*Karlsruhe Institute of Technology, Germany*

[a]`pincock.1@osu.edu`; [b]`michael.poznic@kit.edu`

Ongoing work in the philosophy of science has shown how a variety of different kinds of models are used to arrive at different outcomes such as well-confirmed scientific theories, successful experiments, and technological applications. This attention to practice has generated a proliferation of kinds of models, and one might argue that there is no point in trying to regiment these kinds into any organized system. However, we maintain that clarifying how scientists and engineers are able to reliably arrive at this or that sort of success is an urgent philosophical task. In addition, one good way to achieve this clarification is by classifying models according to how they are best integrated to achieve this or that outcome.

To start, we argue that models may be classified based on how they are evaluated by practitioners. Some models are praised or criticized based on how accurately they represent a target phenomenon (Frigg & Nguyen 2020). These representational models play a central role in testing and confirming theoretical claims. In addition, there are so-called exploratory models that are evaluated quite differently (Gelfert 2016, Fisher et al. 2021). Some are evaluated based on their heuristic value in illustrating a concept or highlighting an aspect of a phenomenon that may be otherwise obscure. We further argue that some non-representational models are evaluated differently than exploratory models. More specifically, we show how a third kind of "design model" is evaluated in a distinctive way. These design models are praised or criticized based on how well they afford the construction of things (Eckert & Hillerbrand 2018, Poznic 2021).

To show the value of this approach to classifying models we consider a case of the use of scientific theories in the technological development of a new product. The creation of a product is often more involved than simply examining the representational models associated with relevant theories. For example, a new sort of electric car could be built in various ways, employing different sorts of batteries, structural materials, and control mechanisms. Engineers devising such a new product should certainly draw on many representational and exploratory models, but this collection of models is not sufficient to specify how such a product should be built. Here, we see an important place for a series of design models. A design model should be formulated and assessed in its own way to help guide the production of the new artifact. Clearly, the design process will go better when these design models are appropriately integrated with the best available range of representational and exploratory models.

To conclude, we argue that there are at least three kinds of models that are relevant for scientific and other research: design models, exploratory models, and representational models. Against the prevalent focus of many contributions in the literature on representational models, we want to stress that design models constitute an important kind of models that has not been discussed thoroughly enough. Furthermore, we suggest a first step in this direction by analyzing the integration of design models, explanatory models, and representational models with a case from technology.

References

Eckert, C. & Hillerbrand, R. (2018). Models in Engineering Design: Generative and Epistemic Function of Product Models. In P. E. Vermaas & S. Vial (Eds.), Advancements in the Philosophy of Design. Springer, pp. 219-242.

Fisher, G., A. Gelfert & F. Steinle (eds.) (2021). Special Issue on

Exploratory Models and Modeling in Science. Perspectives on Science 29: 355-557.

Fouke, D. (2003). Pascal's Physics. In N. Hammond (ed.), Cambridge Companion to Pascal. Cambridge University Press, pp. 75-101.

Frigg, R., & Nguyen, J. (2020). Modelling Nature: An Opinionated Introduction to Scientific Representation. Springer.

Gelfert, A. (2016). How to Do Science with Models: A Philosophical Primer. Springer.

Poznic, M. (2021). Models in Engineering and Design: Modeling Relations and Directions of Fit. In Michelfelder, Diane P. & Doorn, Neelke (Eds.), The Routledge Handbook of the Philosophy of Engineering, pp. 383–393.

Steinle, F. (2016). Exploratory Experiments: Ampère, Faraday, and the Origins of Electrodynamics. A. Levine (trans.). University of Pittsburgh Press.

---

## The Role of Narratives in De-idealising Models in Economics

*Alexandra Quack*

*University of Zurich, Switzerland*

alexandra.quack@uzh.ch

How do economists de-idealise their models? In line with widespread views that scientists need to reverse a model's idealisations to de-idealise a model (cf. Knuuttila and Morgan 2019), it has been argued that economists de-idealise their models by replacing less realistic assumptions with more realistic ones (cf. Peruzzi and Cevolani 2021). In this paper, rather than treating de-idealisation as an issue of replacing assumptions to make models more realistic, I will consider de-idealisation as an active and constructive part of modelling where the model's idealisations cannot easily be reversed (cf. Knuuttila and Morgan 2019). I will focus on the role that narratives play in these processes of de-idealisation.

Based on an empirical case study analysis, I examine the role of narratives in economic modelling to discuss how a mathematical model from contemporary macroeconomics is de-idealised by the authors as they use the model to explain recent income inequality trends (cf. Autor et al 2020). In my analysis, I will compare the role of narratives as they appear in the published article with the insights about narrative's role which I obtained from an interview with one of the co-authors.

In this paper, I will argue that in working with the mathematical model, the economists construct narratives which play a key role in de-idealising the model. Employing a framework introduced by Knuuttila and Morgan (2019) to analyse the complex processes of de-idealisation, I will argue that narratives, by 'recomposing' and 'reformulating,' contribute to the de-idealisation of the mathematical model in this case. More specifically, the narratives on the one hand 'recompose' the model because it is through the narratives that factors outside of the model (especially ceteris absentibus factors) are re-considered. On the other hand, narratives 'reformulate' because they are the means through which the model results are translated into statements about the real world. The analysis of this case provides a detailed study of the actual processes of de-idealisation and can thus contribute to a more explicit treatment of model de-idealisation in science.

References

Autor, David; Dorn, David; Katz, Lawrence; Patterson, Christina; and Van Reenen, John. 2020. "The Fall of the Labor Share and the Rise of Superstar Firms." The Quartely Journal of Economics 135 (2): 645-709.

Knuuttila, Tarja and Morgan, Mary. 2019. "Deidealization – No Easy Reversals." Philosophy of Science 86 (4): 641-661.

Peruzzi, Edoardo and Cevolani, Gustavo. 2021. "Defending De-idealization in Economic Modelling: A Case Study." Philosophy of the Social Sciences.

---

## From Chemistry to Biology: Experimental Strategies in Origins-of-Life Research

*Franziska Reinhard*

*University of Vienna, Austria*

franziska.reinhard@univie.ac.at

How, where, and why life first emerged are still open scientific questions. And origins-of-life researchers who try to answer them are in a particularly challenging epistemic situation. By our current best estimates, life began more than 3.5 billion years ago. Evidence for origins-of-life events is extremely scarce. Researchers cannot expect to find substantial traces of the formation of first life forms and their knowledge of environmental conditions on the early Earth is uncertain. And yet, origins-of-life research is an active field. In this contribution, I will focus on experimental strategies used by origins-of-life researchers to counteract their difficult epistemic starting point. In particular, I will highlight how methods and techniques from synthetic organic chemistry are applied to find out about processes at the origins-of-life. I will argue that this shows how chemical synthesis serves as an important – but often neglected – experimental strategy for studying phenomena that are otherwise difficult to access.

Contemporary origins-of-life research is a vast interdisciplinary endeavour drawing on a large number of research strategies. I will focus on so-called 'prebiotic chemistry' – a subfield of origins-of-life research that draws on organic chemistry. Prebiotic chemists seek to understand how biomolecules (from amino acids and simple carbohydrates to nucleic, acids, proteins, and lipids) formed from simple precursors; how they self-assembled and ultimately gave rise to biological functions such as replication or metabolism. Research in prebiotic chemistry is largely experimental – famously starting with the Miller-Urey experiment in 1953. Researchers in prebiotic chemistry approach the origins-of-life as a chemical problem, which motivates their experimental strategy: they focus is on synthesizing - rather than analysing – relevant biomolecules in the laboratory under conditions that are consistent with our knowledge of the early Earth environment. The rationale behind this approach is summarized by one origins-of-life researcher as follows: "... biogenesis, as a problem of science, is lastly going to be a problem of synthesis. The origin of life cannot be 'discovered', it has to be 're-invented'" (Eschenmoser, 2007).

In my contribution, I will analyse two case studies that develop synthetic routes for nucleotides – the constituents of RNA and DNA – under plausible early Earth conditions. In many ways, this is an exercise in typical synthetic organic chemistry, taking into account things like reactivity and stability of the reactants, the reaction mechanism, and a suitable laboratory set-up. However, the reactions and laboratory set-ups are also supposed to 'stand in' for prebiotic conditions and processes. Hence, as I want to highlight, synthesis in prebiotic chemistry is not primarily a means for producing a certain molecule of interest, it is an experimental strategy that makes it possible to study phenomena that are difficult to access or in contexts of uncertain or fragmentary background knowledge. In addition, I will spell out the consequences of using synthesis as an experimental strategy for establishing validity of experimental results and drawing inferences to the actual phenomenon of interest.

## The Import of Conceptual Engineering of ‘Disability’

*Joel Reynolds*
*Georgetown University, United States*
joel.reynolds@georgetown.edu

Concepts of or related to disability are omnipresent in the life sciences. Indeed, how ‘disability’ is understood impacts a number of central debates in philosophy of science as well as the newer fields of philosophy of technology and philosophy of medicine. In this paper, I argue for a pragmatist approach to conceptual engineering concerning disability that goes beyond the stale medical vs. social model distinction and offers a novel pathway for researchers (Burgess, Cappelen, and Plunkett 2020; Chalmers 2020). I do so by focusing upon recent scholarship in philosophy of disability and showing how it applies to a far wider range of concerns for scientific practice.

Philosophical theories of disability are typically understood as theories of what it is to be disabled, to be impaired, or to be both. These descriptive projects are also typically offered in terms of the extension of ordinary language concepts, refinement of biologically-based concepts used in the life sciences, or expansion of legal concepts deployed in everything from national anti-discrimination laws to international treaties and conventions. Over the last decade, however, disability theory took a normative turn. In the wake of Elizabeth Barnes's scholarship in particular, multiple theories of disability have been developed that are both descriptive and prescriptive in nature (Barnes 2016; Begon 2020; Jenkins and Kim Webster 2021; Nadelhoffer Forthcoming; Timpe Forthcoming; Campbell and Stramondo 2017). Put otherwise, the metaphysics of disability moved from a project of arguing for “what feature(s) of the world – if any – unify or explain disability” to a project of arguing “what feature(s) of the world – if any – both do and should unify or explain disability.” Since nearly all of these projects understand the prescriptive force in question to be in the service of a more equitable and just society for disabled people, I will call these justice-first theories (cf. Haslanger 2012).

I argue that extant justice-first theories of disability fail in two ways: (1) as general theories of disability and (2) as theories that can model the sorts of specific concerns relating to disability in the life sciences. Such concerns apply, to take just a few examples, to issues in genetic counseling (and translational genomics more generally), to clinical diagnostics, and to models used across biology that involve assumptions about “capacity,” among a host of other domains. I show how justice-first theories do not pick out an appropriately wide range of paradigmatic cases of disability and how they problematically fail to do so in at least three ways: with respect to the principles used to determine cases, the flexibility to capture cases across differences of historical, cultural, political, and other contexts, and the breadth to explain how cases of disability that are paradigmatic, yet prima facie different in kind, are accounted for (for example: Deafness, major depression, Autism, vitiligo, chronic pain, and degenerative Multiple Sclerosis). The issues this presents for understanding human animal life are no less serious than the issues this presents for understanding non-human animal life. I conclude by offering a framework for addressing the issues of principles, flexibility, and breadth with respect to 'disability' and discuss how it might impact scientific practice and theory more generally.

References

Barnes, Elizabeth. 2016. The Minority Body. New York, NY: Oxford University Press.

Begon, Jessica. 2020. "Disability: A Justice-Based Account." Philosophical Studies.

Burgess, Alex, Herman Cappelen, and David Plunkett, eds. 2020. Conceptual Engineering and Conceptual Ethics. Oxford, New York: Oxford University Press.

Campbell, Stephen M., and Joseph A. Stramondo. 2017. "The Complicated Relationship of Disability and Well-Being." Kennedy Institute of Ethics Journal 27 (2): 151–84.

Chalmers, David J. 2020. "What Is Conceptual Engineering and What Should It Be?" Inquiry, September, 1–18.

Haslanger, Sally. 2012. Resisting Reality: Social Construction and Social Critique. Oxford: Oxford University Press.

Jenkins, Katharine, and Aness Kim Webster. 2021. "Disability, Impairment, and Marginalised Functioning." Australasian Journal of Philosophy 99 (4): 730–47.

Nadelhoffer, Thomas. Forthcoming. "Chronic Pain, Bad-Differences, and Disability Variantism." The Journal of Philosophy of Disability.

Timpe, Kevin. Forthcoming. "Denying a Unified Concept of Disability." Journal of Philosophy and Medicine.

## Understanding the Notion of Chemical Substance as a Dynamic Aggregate

*Marabel Riesmeier*
*University of Cambridge, United Kingdom*
mcr62@cam.ac.uk

Chemists interact with chemical substances in a multitude of ways. They produce and manipulate them in the laboratory, measure their properties with elaborate instruments, simulate them on computers and use a wide range of diagrams to represent, describe and reason about them. But what lies at the centre of all these activities? What is a substance?

In this paper, I argue that there is no universal way of delineating chemical substances. Rather, contextual notions of substances arise from different types of interactions. These are aggregated to form broader, dynamic notions of substance.

I begin by showing that there are different, contextual notions of chemical substance, depending on the mode of interaction between scientist and substance. For this purpose, I distinguish two rough but foundational types of interactions with substances - practical and conceptual ones. Practical interactions involve handling chemicals, e.g. mixing powders and solutions, whereas conceptual interactions involve abstract reasoning, e.g. manipulating Lewis formulas or approximating electronic wave functions. On the side of practical interactions, substances are distinguished based on observation, such as spectroscopic measurements, and transformation, such as phase changes and chemical reactions. On the conceptual side, substances are delineated by abstracted structures. The actual picture of possible interactions with substance is, of course, much more fine-grained, multiplying the potential for contextual notions of substance.

In the next step, I show that no contextual notion of substance arising from just one of these types of interactions can be generalised without significant sacrifices. Delineating substances based purely on some combination of practical interactions and observations, such as Schummer's (1998) criterion of purification, leaves us with some taxonomy of observable substance. However, it does not allow for distinguishing and reasoning with potential substances, which only exist conceptually but play an important role in bringing about new substances. Delineating substances based on conceptual concerns, as attempted in Hendry's (2006) defence of the microstructural view, gives primacy to theoretical constructs even at the cost of failing to integrate some empirical evidence. This is undesirable in science and cannot account for historical changes in theoretical constructs that have been informed by practical interactions.

The alternative to taking one such contextual notion of substance as a starting point for generalisation is the combination of several. Given the incongruencies between what these accounts cover and at times contradicting taxonomic consequences, it would be ill-advised to elevate one such combination as universal. Nevertheless, broad connections can be made, that account for the delineation of most substances at any given time. As a dynamic aggregate, my proposal can account for the persistence of most substances even over several dramatic theoretical and experimental shifts in chemistry over the last 200 years and the continuing relevance of the concept to this day.

References

Schummer, Joachim. 1998. "The Chemical Core of Chemistry I: A Conceptual Approach." Hyle 4 (2): 129–62.

Hendry, Robin Findlay. 2006. "Elements, Compounds, and Other Chemical Kinds." Philosophy of Science 73 (5): 864–75.

---

## Thinking of epigenetic function, inheritance and bio-medical practices through Agential Realism

*Tom Rolef Ben-Shahar*

*BIU, Israel*

tomrbs1@gmail.com

Over the last 20 years, Environmental Epigenetics and related fields such as Social Epigenetics and Developmental Origin of Health and Disease (DOHaD), have become important approaches in biological and biomedical research. Studies tend to focus on tracing the effects of diverse, and in particular difficult or harmful socio-material environmental conditions on personal and lineal health, and their mediation by epigenetic mechanisms. As environmental exposures, i.e., life circumstances, are strongly related to socio-economic distribution, these research fields are inherently political. As epigenetic mechanisms are understood not only as reacting to environmental changes but also as transmitting the memory of exposure via 'epigenetic inheritance', these fields are significant in understanding and addressing the perpetuation of health and overall inequality.

Initially, the epigenetic promise to extricate biological research from the confining determinism of the genetic code, was celebrated by scholars from both the biological and social sciences as well as the humanities. However, over the past decade, Science and Technology Studies (STS) and other social sciences scholars, have been critically demonstrating the complexity and existing pitfalls in navigating between socio-political concerns, epigenetic bio-medical research and its application as policy and interventions. The scientific practice and theory and their translation have been criticized for reducing the much-anticipated plurality of epigenetics, incorporating it into the existing biomedical framework of binary and mechanistic determinism, and potentially leading to new forms of social discrimination.

Karen Barad's Agential Realism (AR) is a powerful theoretical framework, congruent with and grounded in scientific thinking, that both emphasizes the indeterministic nature of the world, and offers a rigorous way to develop scientific practices that engage with it. In this work, I hope to demonstrate some avenues through which AR is particularly relevant to epigenetics, and holds innovative potential for formulating research practices that address the challenges, possibilities and response-abilities epigenetics brings.

Barad's ontology of entanglement and indeterminacy, may be read through the determinism-indeterminacy tension found not only in epigenetic research practices, as critically analyzed by others, but also as it manifests in epigenetic plasticity. Indeed, both 'indeterminacy' and 'entanglement' are key to epigenetics' theoretical conceptualization, and may be further developed through AR. Other concepts which are thoroughly rearticulated by Barad, have surfaced as key to the scientific and theoretical development of epigenetics, including: materiality, temporality and memory. Epigenetic mechanisms are first and foremost material phenomena. Increasingly, epigenetic function is being understood beyond the localized impact of various epigenetic marks, through the intricate, dynamic and complex orchestration of chromatin structure and nuclear organization. Read through Barad's understanding of mater as agentive and meaningful, the significance is not simply 'conceptual' but concrete (as concepts are themselves material), in analyzing epigenetic temporality and the function of epigenetic inheritance. For this, Barad's particular understanding of memory and history can be read through epigenetic materiality, offering an extension to current understanding of epigenetic function as bearing memory or history. Some of the ethical, ontological and epistemic (ethico-onto-epistemological) implications that this material reading carries to bio-medical research and epigenetic interventions will be explored.

# The Meta-Philosophical Import of Naturalized Philosophy of Science (in Practice)

*Joseph Rouse*
*Wesleyan University, United States*
jrouse@wesleyan.edu

Anglophone philosophy mostly turned toward meta-philosophical naturalism beginning mid-20th Century. Quine's (1969) rejection of "first philosophy" and Sellarsian (1963) primacy of the Scientific Image prominently abdicated philosophical authority over science. Naturalists instead situate philosophical work within a scientific conception of the world. Even their critics now only exempt some philosophical domains from accountability to the sciences while acknowledging scientific claims as adjudicated scientifically.

Philosophy of science thereby became more isolated within the discipline. Philosophers working closely with particular sciences no longer seemed to explicate rationality generally, with seemingly less direct relevance to other philosophical topics. Philosophy of science took its own naturalistic turn decades later (Giere 1985): concern with "science as we know it" (Cartwright 1989, 1) replaces rational reconstructions; philosophers attend to scientific practice as locus of scientific understanding; and philosophy of science mostly accommodates historical, sociological, and other empirical studies of the sciences.

Both orthodox and "liberal" (de Caro and Macarthur 2010) meta-philosophical naturalists typically posit a "scientific conception of the world" uninformed by naturalistic accounts of scientific understanding in practice. They dispute whether and how to relate "normative facts" (in ethics, semantics or psychology, or aesthetics) to an anormative scientific image as the domain of law or causality, while "oppos[ing] ... the view that normative facts hold wholly independently of human practices" (de Caro and Macarthur 2010, 3).

I advance three theses about how naturalized philosophy of science relates to orthodox or liberal versions of meta-philosophical naturalism. First, meta-philosophical naturalism ought to defer to naturalized philosophy of science concerning a "scientific conception of the world." Doing otherwise implicitly imposes a "first philosophical" account of scientific knowledge onto scientific understanding in practice. Second, naturalized philosophies of science challenge both orthodox and liberal distinctions between normative facts and scientific understanding of nature. Non-cognitivist accounts of normativity and "liberal" ascriptions of autonomy to normative facts instituted by human practices, falsely contrast scientific facts to "normative facts." Scientific factual determinations answer to normative concerns in scientific practice.

Finally, this challenge to recent meta-philosophical naturalists extends philosophy of science's contribution to the original naturalistic turn. Goodman (1954), Sellars (1957, 1997), and Hempel (1965) rejected empiricist scruples against causal or nomological necessity as falsely contrasting empirical facts to the modality of causes or non-Humean laws. The sciences do not describe the world non-modally; counterfactual and subjunctive projectibility of scientific concepts is indispensable to conceptual content, experimental design, empirical confirmation, and explanation. Similarly, naturalized accounts of scientific understanding in practice show that scientific understanding is not anormative, but institutes norms in scientific practice. That recognition then re-opens questions of how best to account for relations between the normativity of scientific and other practices.

References

Cartwright, Nancy 1989. Nature's Capacities and their Measurement.

De Caro, Mario and Macarthur, David 2010. Naturalism and Normativity.

Giere, Ronald 1985. Philosophy of Science Naturalized.

Goodman, Nelson 1954. Fact, Fiction, and Forecast.

Hempel, Carl 1965. Aspects of Scientific Explanation.

Quine, W.v.O. 1969. Ontological Relativity

Sellars, Wilfrid 1963. Philosophy and the Scientific Image of Man.

---

## Science, Responsibility, and the Philosophical Imagination

*Matthew Sample*

*Centre for Ethics and Law in the Life Sciences, Leibniz Universität Hannover, Germany*

matthew.sample@cells.uni-hannover.de

Despite the "demise of the demarcation problem" (Laudan, 1983), philosophers still talk about "Science" or a science or "the sciences." Sometimes even without a clear definition, we charge full force into discussions of "well-ordered" science or the value-free ideal. Untethered thinking may seem like a quintessentially philosophical privilege, but the capacity to imagine and re-imagine science functions to coordinate activities as diverse as presidents' claims of scientific leadership in global crisis and novel collaborations between engineers, investors, and scientists. Nevertheless, this dual feature of science (i.e. as both real and imagined) receives only indirect attention in the recent philosophical literature on science, disconnected from adjacent sociological literatures that could explain it. Accordingly, I propose that we must renew and refine our attention to collective imagination to do two things.

First, it reveals for the philosophical observer of scientific practice a key mechanism by which science becomes situated in society, eschewing the easy Kuhnian assumption that science is a social but self-contained epistemic practice. More specifically, attending to the role of collective imagination helps philosophers of science to better understand how scientific reasoning and experimental techniques are made simultaneously intelligible and possible through particular institutional arrangements, values, and visions of desirable futures. Philosophers can then engage with these normatively-laden features in a given context, inquiring into their origins and asking if that normative content is genuinely worthy of our assent. Philosophy of science thus becomes continuous with research in applied ethics, political philosophy, and science and technology studies (STS).

Second, but just as importantly, the framework of imaginaries can also be turned back onto philosophical practice itself as a form of self-reflection. By identifying the content of philosophy's own internal imaginaries of science, whether idealizations or heuristic definitions, we can compare our discourse with the sociotechnical imaginaries (Jasanoff and Kim 2015) that actually organize scientific practices in society. This critical comparison does more than simply reveal inaccuracies or limitations in our own disciplinary habits. Realizing the dependence of philosophy of science on the philosopher's imagination reinforces the profound and urgent need to create an inclusive, equitable, and representative community within our discipline.

To support these sweeping promises of utility and self-correction, I construct an argument in three parts. I begin with the high-level insight that institutions and imagination are closely linked in society, as documented in foundational and more recent social theory on imagination and institutions. Working from this assumption – imagination matters – I then move to analyze the imaginary of science implicit in two case studies: one from philosophy of science (Heather Douglas' work on values in science) and one from STS ("technoscience" as a distributed network), demonstrating their implications for the distribution of responsibility in society. Comparing these two cases, I conclude, exhibits the idiosyncrasy of philosophical imaginaries of science and pushes us to make collective imagination a more explicit object of inquiry and self-reflection. Our work on science will be better when we actively question the implicit sociotechnical arrangements and desirable futures that underpin philosophical thinking.

## What is a plesiosaur?

*Judyth Sassoon*
*University of Exeter and University of Bristol, United Kingdom*
js7892@bristol.ac.uk

Philosophy of paleontology is a new discipline exploring how palaeontologists gain knowledge of the past from fossils. To date it has mainly focused on epistemic questions, such as the incompleteness of the fossil record and the scientific strategies that mitigate those limitations. What has not been discussed in sufficient detail is the actual process of fossil interpretation, especially the influence of ideas brought to fossil specimens by the palaeontologists themselves. Since the beginning of palaeontology as a science, palaeontologists have worked within generally accepted conceptual frameworks that provided the backdrop for their interpretation of specimens. T.D. Johnston (2021) called these frameworks “theories of relatedness”. A theory of relatedness makes certain assumptions and influences the way comparative data is interpreted. At the same time, empirical comparisons themselves can also shape theories of relatedness. The meanings assigned to fossils arise out of this reciprocity. Here, I will show how such dynamics have modified the conceptualisation and meaning of one group of extinct animals: the plesiosaurs.

Plesiosaurs were marine reptiles that evolved from a terrestrial ancestor in the early Mesozoic Era. Fossils show that plesiosaurs had a unique four-finned body “type” which has not been repeated by any animal group following their extinction. Since their ‘rebirth’ in 1821 as scientific concepts and palaeontological icons, plesiosaur fossils have been studied and interpreted against a background of slightly different “theories of relatedness”. The interpretations of plesiosaur specimens emerged from the referencing of comparative observations to such theories, and it is also possible to find examples of how data from plesiosaur fossils shaped the overarching theories of relatedness themselves. Here I will present several conceptualisations of the meaning of “plesiosaur”, as it emerged from the reciprocity between empirically derived comparative observations and theoretical backdrops. I discuss three theories of relatedness within which the idea of the plesiosaur has been conceptualised in slightly different ways: (1) natural theology, (2) vertebrate archetype (3) phylogenetic.

In the 19th Century, the first detailed plesiosaur description was made by William Conybeare who coined the Greek name pleios sauros (“near reptile”). This name itself reflected the way plesiosaurs were conceptualised within a natural theological theory of relatedness. At the same time, Richard Owen was working on his own theory of relatedness, which emerged as the vertebrate archetype. Empirical date from a plesiosaur specimen helped Owen to form his ideas in 1840. Owen's theory was a peculiar mixture of natural theology, continental Naturphilosophie and the comparative anatomist's need for a general, experimentally derived reference "schema". The Darwinian revolution of 1859 opened a new vista in palaeontology, enabling fossil workers to situate specimens into ancestral lineages. It brought with it new possibilities to explain the two broad morphologies adopted by the plesiosaur body "type": plesiosauromorphs (small heads and long necks) and pliosauromorphs (large heads and short necks).

These examples will be used here to highlight the reciprocal dynamic between comparative observations and the overarching theories of relatedness in the process by which palaeontologists make sense of fossils.

---

## Atomistic Simulations and Scientific Explanation: The Puzzle of Aquaporin Proton Exclusion

*Julie Schweer and Marcus Elstner*
*Karlsruhe Institute of Technology, Germany*
julie.schweer@posteo.de

Explanation is usually seen as a central goal of science. Given that computer simulations have become an indispensable scientific tool in various areas, it is of no surprise that their role in obtaining scientific explanations has increasingly attracted philosophical attention. However, philosophical studies providing a detailed examination of the explanatory role of simulations in concrete contexts of application are still rather rare. In this paper, we focus on atomistic simulations and discuss a case study from molecular biology in order to shed light on their explanatory role.

So-called aquaporins are channel proteins located at human cell membranes. While they enable a rapid transmembrane transport of water molecules, they do at the same time block the passage of protons. This proton exclusion is pivotal for various electrochemical processes but also surprising since protons are otherwise known to be easily transferred in bulk water (de Groot and Grubmüller 2005). To better explain the remarkable selectivity of aquaporins, several simulation studies using atomistic approaches have been conducted in recent years (e.g. de Groot et al. 2003; Burykin and Warshel 2004).

Aquaporin selectivity makes a particularly exciting study case regarding the explanatory role of simulations. Not only are atomistic simulations relevant for a multitude of scientific fields; their explanatory role is, moreover, not trivial to determine. Consider that atomistic simulations using so-called molecular dynamics methods commonly start with a description of different kinds of atomic interactions and proceed by numerically solving Newton's equation of motion for the atoms of a given system, thereby computing trajectories that display the dynamic evolution of the system in time. At first glance, it may seem promising to assume that such simulations explain in a DN fashion by tracing how a given system behavior is derivable from certain governing equations and particular initial conditions.

However, we argue that in the case of aquaporin research, overly focusing on the derivational character of the involved simulation approaches might be misleading when trying to understand their explanatory contribution. Albeit starting with a description of atomic motion, the explanatory power of simulations here mainly stems from the employment of methods that allow to abstract from individual atomic trajectories and learn about the thermodynamic properties of a system. More precisely, simulations are explanatorily relevant in the case of aquaporin because they provide means to analyze as to how different structural regions of the protein contribute (or fail to contribute) to the emergence of an energetic barrier that prevents protons from passing.

Put briefly, in the investigated case, simulations explain not so much by showing how the explanandum follows from governing equations and specific conditions but rather by allowing scientists to systematically abstract from vast amounts of micro-level trajectories and to assess how different parts of the system (in our case: structural regions of the protein) give rise to its overall behavior. Against this backdrop, we suggest to fruitfully address their explanatory role by referring to mechanistic models explanation as ongoingly discussed in the field of philosophy of biology.

References

Burykin, Anton, and Arieh Warshel. 2004. 'On the Origin of the Electrostatic Barrier for Proton Transport in Aquaporin'. FEBS Letters 570(1):41–46.

de Groot, Bert L., Tomaso Frigato, Volkhard Helms, and Helmut Grubmüller. 2003. 'The Mechanism of Proton Exclusion in the Aquaporin-1 Water Channel'. Journal of Molecular Biology 333(2):279–93.

de Groot, Bert L., and Helmut Grubmüller. 2005. 'The Dynamics and Energetics of Water Permeation and Proton Exclusion in Aquaporins'. Current Opinion in Structural Biology 15(2):176–83.

## Construct validation in psychology: a bridge between measurement and theory?

*Maria Serban*
*University of East Anglia, United Kingdom*
`m.serban@uea.ac.uk`

One condition for building sound psychological theories is to have valid measures of the key (theoretical) constructs being put forward. While construct validity is treated as a golden methodological standard, the notion itself is ambiguously used to the point of being misleading or useless. The lack of clear standards for the validity of theoretical constructs leads to a 'validation crisis' in psychological sciences. I argue that philosophers of science can contribute to current debates about what construct validity is, and what conditions must be met for a psychological test measure to have validity. Conceiving construct validation as an iterative epistemic process allows us to better identify the main challenges that face linking theory to measurements in paychology. To illustrate these, I unpack two case studies which track the interplay between measurement and theory in the study of general intelligence and of working memory. The epistemic iteration perspective implies, on the one hand that some tests can be said to have validity even if the target theoretical construct is not completely clearly delineated. The case of IQ tests and general inteligence illustrate this point, while the case of memory span tests points to the normative and interpretative role that local theories play in the validation of measurements.I argue that the process of construct validation and the methodological developments that it demands should be seen as part of the broader project of developing more precise causal theories of psychological processes and capacities.

References

Alexandrova, Anna & Haybron, Daniel M. (2016). Is Construct Validation Valid? Philosophy of Science 83 (5):1098-1109.

Cronbach, Lee J. & Meehl, P. E. (1956). Construct validity in psychological tests. In Herbert Feigl & Michael Scriven (eds.), Minnesota Studies in the Philosophy of Science. , Vol. pp. 1–174.

Feest, Uljana (2020). Construct validity in psychological tests – the case of implicit social cognition. European Journal for Philosophy of Science 10 (1):1-24.

Nelson, Katherine (2015). Quantitative and Qualitative Research in Psychological Science. Biological Theory 10 (3):263-272.

Vessonen, Elina (2017). Psychometrics versus Representational Theory of Measurement. Philosophy of the Social Sciences 47 (4-5):330-350.

## The Hodgkin-Huxley Model, Explanatory Force and Explanatory Aims

*Kaamesh Singam*
*Indian Institute of Technology Kanpur, India*
kaameshsingam@gmail.com

The Hodgkin-Huxley model is a hugely influential mathematical model in the field of neuroscience and electrophysiology. The precise nature of the model and its explanatory status have been discussed by Carl Craver and Arnon Levy in their publications ((Craver, 2006), (Craver, 2008), (Levy, 2013)). While Craver argues that the model is a mechanism sketch and hence is a deficient explanation, Levy argues that the model is an aggregative abstraction and hence is not explanatorily deficient. I compare their arguments and then I show that both their philosophical accounts of explanation are based on what scientists take to be adequate explanations in their practice. I then explore the question of what carries the explanatory force of both their accounts of adequate explanation. Then I suggest that an answer could be found when we turn towards talk of explanatory aims of scientists.

## Extraordinarily corrupt or statistically commonplace? Reproducibility crises may stem from a lack of understanding of outcome probabilities

*Caetano Souto Maior*
*Basque Center for Applied Mathematics, Spain*
caetanosoutomaior@protonmail.com

Failure to consistently reproduce experimental results, i.e. failure to reliably identify or quantify an effect — often dubbed a 'reproducibility crisis' when referring to a large number of studies in a given field — has become a serious concern in many communities and is widely believed to be caused by (i) lack of systematic methodological description, poor experimental practice, or outright fraud. On the other hand, it is common knowledge of the scientific practice that (ii) replicate experiments — even when performed in the same lab, by the same experimenter — will rarely show complete quantitative agreement between them. The presence of the widely believed

(i) and commonplace (ii) are not mutually exclusive, but they are incompatible as justifications for irreproducibility. Invoking the former implies an anomaly, a crisis, while the latter is statistically expected and therefore amenable to quantification.

Interpreting two or more studies as conflicting is often a reduction to a mechanicist view where a ground truth exists that must be observed with every properly performed experiment, a slightly less naive view (at best) is a frequentist view where statistical tests must confidently identify a true effect (i.e. a single parameter value) as significant almost always (i.e. an arbitrary proportion of 95% of times). A broader view, however, may consider that the effect can only be observed as a probability distribution; individual experiments are, therefore, not expected to differ only by sampling and power to identify a significant effect, but by variation at the level of the parameter value itself — i.e. it is accepted that there are sources of variation that cannot be controlled with infinite precision, for instance in the environment and from the experimenter, or it is acknowledged that there may be unknown, uncontrolled factors that will introduce biases.

Quantitatively, that perspective is consistent with a Bayesian hierarchical formulation, where the effect (commonly called the group-level) parameters are under a hyperprior and above individual experiment parameters. Put another way, the Bayesian hierarchical view allows reconciliation between seemingly discordant results by interpreting each experiment as a sample itself of a (group-level/effect) distribution, which in turn sets the range and probability of expected outcomes for new individual experiments. As a corollary, a large number of replicates will increase the confidence not only in the expected value but also in the deviation for it. Thus, "validating" an experiment does not mean getting the same number every time, but establishing the range and likelihood of well-performed experiments. Conversely, once an experiment has been extensively replicated, the effect distribution is informative of how much each repetition deviates from expectation, whether they are actually extreme — and potentially contain anomalies or misconduct — or if they are probabilistically not surprising. This formulation has profound consequences for assessments and claims on reproducibility.

**Uncertainty in Medicine: A Pragmatic Definition**

*Erman Sozudogru*
*UCL, United Kingdom*
erman.sozudogru@ucl.ac.uk

Uncertainty is endemic in medicine despite the desire for certainty in clinical decision making. Uncertainties arise when providing a diagnosis, deciding on the best course of treatment, evaluating clinical trial results, and determining public health interventions, among other situations. The drive for certainty in medicine can lead medical practitioners to overlook uncertainty instead of understanding the sources and its nature. Overlooking uncertainties can have grave consequences in medicine, giving clinicians a false sense of security and undermining the complexities of the decision-making process. These concerns have been raised in several articles by medical professionals and scientists, calling practitioners to develop better ways of dealing with uncertainty (Simpkin & Schwartzstein, 2016, Hatch 2017).

This paper presents a pragmatic definition of uncertainty, focusing on the source and nature of the uncertainties in medical practice. This definition moves away from the common notion that equates uncertainty with the gaps in our knowledge. Looking at broader medical and philosophical literature, uncertainty is commonly thought of as the gap between our current state of knowledge and the perfect state of knowledge. In what follows, I present a pluralist argument against the idea that there is a perfect state of knowledge that can be captured in a single, comprehensive account. Instead of treating uncertainty as a gap in our knowledge, we must understand it as the subjective experience of our ignorance. This position is built on Paul Han's recent work that defines uncertainty as a metacognitive process where a person or a group actively reflect on their lack of knowledge or understanding. Herein, I introduce another important feature of uncertainty that is that uncertainty arises when our actions are underdetermined by our existing state of knowledge. In other words, an inquirer will experience uncertainty when they cannot determine the best course of action in light of what they know in a specific context.

This pragmatic definition serves as a helpful starting point for philosophers, medical practitioners and researchers in their reflections on the source of uncertainty and the complexities of the judgements they must make about how to act in different situations. The pragmatic definition also highlights the complexities involved in managing uncertainty, shifting the focus to different types of judgements involved in this process. Here, I will explore several case studies demonstrating how clinicians will experience uncertainty when they cannot determine the best course of action for their patients. Similarly, look at how public health officials deal with uncertainty when the present state of knowledge underdetermines the appropriate forms of action. In particular, I will explore how different national vaccine programmes reacted in light of such uncertainty that following reports of rare blood clots linked to the AstraZeneca vaccine Vaxzevria. To address the worries expressed in the medical literature with regards to acknowledging and dealing with uncertainties, we first need to acknowledge the subjective source of uncertainty and recognise the complexities in judgements we need to make to resolve them.

---

## Scientists are Internalists about the Epistemology of Imagination: A Case Study from Space Science

*Michael Stuart*
*National Yang Ming Chiao Tung University, Taiwan*
mike.stuart.post@gmail.com

The past 5 years have seen an influx of work on scientific imagination: what kinds are there, when is it permissible to use, who gets to use it, and how are uses of imagination evaluated and justified? This paper presents a case study from space instrumentation science – the field of study that designs and builds technology to perform extraterrestrial experiments. Scientists working in this field know that their instruments (optimistically) have launch windows that are several years to decades in the future. And every instrument they build (and many of the parts of those instruments) are often one-of-a-kind and the first-of-their-kind. Accordingly, many problems that arise in space instrumentation require a great deal of imagination to solve. But that imagination is tightly constrained by time, budgets, political and public interest, as well as the limits of current technology. This paper looks at a specific episode of problem solving during the development of ProSPA, which is an instrument built to search for water on the lunar surface and to perform in-situ resource utilization which will create water from lunar regolith. The question is, how do scientists recognize a use of imagination as a good one? That is, when are scientists willing to say that an imagining confers some justification in favour of a potential problem solution? Two options for answering this question can be taken from mainstream epistemology: internalism and externalism. Most writing on scientific epistemology portrays science as an externalist reliabilist project. For example, when scientists want to know if something is correct, they (very naturally) check the reliability of the instruments. And most writing in mainstream epistemology of imagination assumes an externalist viewpoint. For example, we can trust that the imagination will provide the correct answer to the question of whether we can hop over a particular stream, because evolution has programmed us to have a reliable imagination for answering that sort of question. Yet, in the case at hand, I will show that scientists prefer an internalist approach. Rather than justifying a use of imagination by finding out if the person who produced the idea has a reliable imagination, scientists, when faced with an imaginative problem solution, simulate the idea for themselves to see if the idea is rational and responsible given their background knowledge and given what they themselves find in their own imaginations. To go further, a sketch of an internalist epistemology of imagination is developed according to which an imagining is good depending on how well its output balances the need to satisfy as many epistemic constraints on good reasoning as possible (allowing that some can be violated), given the problem-solving context. In summary, some consequences are considered for how this instance of internalism might impact the growing literature on the epistemology of scientific imagination, and the perception that science is, in general, externalist in its epistemology.

---

### Evidence-Based Toxicology: An Epistemological Prognosis

*Richard D. Sung*

*KAIST Graduate School of Science and Technology Policy, South Korea*

rcdsung@gmail.com

Due to ongoing contestations surrounding the contemporary regime of toxicological risk assessment, there have been programmatic efforts to augment the scientific basis of toxicology. A noteworthy case is the "evidence-based toxicology" (EBT) movement, whose aim is to radically alter the paradigm of toxicology by modelling it after evidence-based medicine (EBM). EBT's epistemological pursuits primarily concern causal inference in toxicological risk assessment (Guzelian et al., 2005; 2009) and the biostatistical basis of toxicity testing (Hoffmann and Hartung, 2006). In particular, EBT has drawn much from Bayesian approaches to clinical diagnosis in order to critique the soundness of risk assessments based on in vivo data (ibid.) and develop an alternative approach to toxicity testing (Jaworska and Hoffmann, 2010). Furthermore, EBT is characterized by its advocacy of alternatives to animal testing as well as its skepticism regarding the epistemic role of expert judgement and precautionary approaches to risk assessment. The

EBT movement is currently spearheaded by the Evidence-Based Toxicology Collaboration (EBTC) of Johns Hopkins Bloomberg School of Public Health.

Motivated by José Luis Luján and Oliver Todt's call for a naturalistic approach to appraising evidentiary regimes or "epistemic policies" in regulatory science (Luján and Todt, 2020; 2021), I present a philosophical critique of EBT's approach to validating methods of toxicity testing. While agreeing with Luján and Todt's argument that epistemic policies should be assessed according to their real-life performances, I note that their argument does not provide the means for appraising emerging epistemic policies advanced against the background of conflicts of value and interest. In response, I argue that there are at least two a priori conditions that all adequate epistemic policies should satisfy: Coherence (i.e., a clear relation between the policy goals and proposed methods) and Immunity (i.e., not being designed to open up avenues for circumvention or exploitation of existing policy issues). In adopting a naturalistic view of epistemic policy appraisal, I stress that the two a priori conditions are necessary but not sufficient.

With these preconditions in mind, I assess the potential adequacy of EBT as an emerging epistemic policy for toxicological risk assessment. In relation to coherence, I examine whether there is a genuine epistemological connection between EBT and EBM. In relation to coherence and immunity, I examine whether the biostatistical basis of EBT would deliver on its promise of consistency, objectivity, and transparency. Based on 1) critiques of the Bayesian approach to confirmation and scientific evidence (Biddle, 2013; Mayo, 2018; Mayo and Morey, 2017), 2) my doubts regarding EBT's analogy between toxicity testing and diagnosis in medicine, and 3) Sven Ove Hansson and Christina Rudén's philosophical critique of a controversial attempt at advancing an EBT approach to risk assessment (Rudén and Hansson, 2008), I advance a skeptical position with regard to EBT's self-proclaimed title "evidence-based". To be specific, my examination shows that EBT lacks both the coherence and immunity necessary for augmenting the epistemic basis of risk assessment.

References

Biddle, J., 2013. State of the field: Transient underdetermination and values in science. Studies in History and Philosophy of Science Part A, 44(1), pp.124-133.

Guzelian, P.S., Victoroff, M.S., Halmes, N.C., James, R.C. & Guzelian, C.P., 2005. Evidence-based toxicology: A comprehensive framework for causation. Human & experimental toxicology, 24(4), pp.161-201.

Guzelian, P.S., Victoroff, M.S., Halmes, C. and James, R.C., 2009.

Clear path: Towards an evidence-based toxicology (EBT).

Hoffmann, S. and Hartung, T., 2006. Toward an evidence-based toxicology. Human & experimental toxicology, 25(9), pp.497-513.

Jaworska, J. and Hoffmann, S., 2010. Integrated testing Strategy (ItS) – Opportunities to better use existing data and guide future testing in toxicology. ALTEX-Alternatives to animal experimentation, 27(4), pp.231-242.

Luján, J.L. and Todt, O., 2021. Evidence based methodology: A naturalistic analysis of epistemic policies in regulatory science. European Journal for Philosophy of Science, 11(1), pp.1-19.

Luján, J.L. and Todt, O., 2020. Standards of evidence and causality in regulatory science: Risk and benefit assessment. Studies in History and Philosophy of Science Part A, 80, pp.82-89.

Mayo, D., 2018. Statistical inference as severe testing: How to get beyond the statistics wars, Cambridge University Press.

Mayo, D. and Morey, R.D., 2017. A poor prognosis for the diagnostic screening critique of statistical tests (No. ps38b). Center for Open Science.

Rudén, C. and Hansson, S.O., 2008. Evidence-based toxicology: "Sound science" in new disguise. International journal of occupational and environmental health, 14(4), pp.299-306.

---

## Fitness for purpose in psychometrics

*Eran Tal*[a] *and Sebastian Rodriguez Duque*[b]

[a,b]*McGill University, Canada*

[a]`eran.tal@mcgill.ca`; [b]`sebastian.rodriguezduque@mail.mcgill.ca`

Much recent philosophical attention has been given to the concept of validity in psychometrics (Alexandrova, 2017; Angner, 2013; McClimans, 2010, 2013). By contrast, the question of whether and when a psychometric instrument is fit for its intended purpose has been largely neglected. Here we argue that fitness for purpose is a distinct feature of a psychometric measure that does not automatically follow from its validity, and is established by distinct sources of evidence. We focus on applications of psychometrics in healthcare, and specifically on the use of patient-reported outcome measures (PROMs) in mental healthcare.

PROMs such as the Patient Health Questionnaire (PHQ-9) and the Kessler Psychological Distress Scale (K-10) are routinely used by mental health service providers for various purposes, including screening patients, assisting with diagnosis, recommending treatment plans, tracking patient progress, and assessing overall quality of care. Health outcomes researchers acknowledge that a PROM designed and validated for one purpose and population, such as screening in adults, may not be fit to serve another, such as tracking patient progress in youth. This context-sensitivity is partially due to differences in patient characteristics, and to the fact that different clinical decisions can require different kinds of evidence. Health outcomes researchers typically deal with this context-sensitivity by 're-validating' PROMs against 'gold standards' of evidence, e.g., by adjusting the severity thresholds of a screening tool against the outcomes of clinical interviews in new settings (Beard et al., 2016; Seo & Park, 2015; Urtasun et al., 2019; van Steenbergen-Weijenburg et al., 2010).

This paper argues that 're-validation' techniques are inadequate for establishing fitness-for-purpose across contexts, because they are based on an overly narrow concept of fitness-for-purpose. Fitness-for-purpose in psychometrics is not only an epistemic criterion, but also an ethical criterion, namely, the condition of fit between the meanings and uses of a measure and the values and aims of stakeholders. Consequently, evaluating fitness-for-purpose requires a thorough examination of the ethics of measurement. We substantiate our claims with the results of a recent project in which we collaborated with psychometricians, clinicians, and young people. As part of this collaboration, philosophers of science helped develop a training in measurement for clinicians working at Foundry, a network of integrated mental health clinics for people aged 12-24 in British Columbia. Our research revealed a gap between psychometric evaluation techniques, which focus on statistical properties, and the need of clinicians and patients to identify measures that promote ethical and social values, such as inclusiveness, empowerment and collaboration.

Our analysis highlights the need for a normative theory of measurement as a foundation for measure evaluation in psychometrics. Although some validation theorists have paid close attention to the ethics of measurement (Kane, 2013; Messick, 1995), they overemphasized the importance of avoiding negative social consequences. Building on McClimans (2010), we show that fitness-for-purpose is a stronger requirement than Messick's 'consequential validity', and involves using measurement as a tool for genuine dialogue between clinician and patient.

References

Alexandrova, A. (2017). A philosophy for the science of well-being. Oxford University Press.

Angner, E. (2013). Is it possible to measure happiness?: The argument from measurability. European Journal for Philosophy of Science, 3(2), 221–240.

Beard, C., Hsu, K. J., Rifkin, L. S., Busch, A. B., & Björgvinsson, T. (2016). Validation of the PHQ-9 in a psychiatric sample. Journal of Affective Disorders, 193, 267–273.

Kane, M. T. (2013). Validating the Interpretations and Uses of Test Scores. Journal of Educational Measurement, 50(1), 1–73.

McClimans, L. (2010). A theoretical framework for patient-reported outcome measures. Theoretical Medicine and Bioethics, 31(3), 225–240.

McClimans, L. (2013). The Role of Measurement in Establishing Evidence. Journal of Medicine and Philosophy, 38(5), 520–538.

Messick, S. (1995). Validity of psychological assessment: Validation of inferences from persons' responses and performances as scientific inquiry into score meaning. American Psychologist, 50(9), 741.

Seo, J.-G., & Park, S.-P. (2015). Validation of the Patient Health Questionnaire - 9 (PHQ-9) and PHQ-2 in patients with migraine. The Journal of Headache and Pain, 16(1), 65.

Urtasun, M., Daray, F. M., Teti, G. L., Coppolillo, F., Herlax, G., Saba, G., Rubinstein, A., Araya, R., & Irazola, V. (2019). Validation and calibration of the patient health questionnaire (PHQ-9) in Argentina. BMC Psychiatry, 19(1), 291.

van Steenbergen-Weijenburg, K. M., de Vroege, L., Ploeger, R. R., Brals, J. W., Vloedbeld, M. G., Veneman, T. F., Hakkaart-van Roijen, L., Rutten, F. F., Beekman, A. T., & van der Feltz-Cornelis, C. M. (2010). Validation of the PHQ-9 as a screening instrument for depression in diabetes patients in specialized outpatient clinics. BMC Health Services Research, 10(1), 235.

---

## Bridging epistemic divides: An epistemic system for interdisciplinarity?

*Merel Talbi*[a] *and Roosmarijn van Woerden*[b]

[a]*VU Amsterdam (Vrije Universiteit Amsterdam), Netherlands;* [b]*Utrecht University, Netherlands*

[a]`m.m.talbi@vu.nl`; [b]`r.vanwoerden@uu.nl`

In interdisciplinary research, academics from multiple disciplinary backgrounds work together to integrate their knowledge in order to deal with questions that are too complex to be dealt with by a single academic discipline. In undertaking these interdisciplinary projects, different disciplinary epistemic systems are introduced to the project, potentially leading to conflict. This leaves the question what an epistemic system for interdisciplinarity requires. Scholars of interdisciplinary studies have brought forward four key elements of interdisciplinary work that can be translated into requirements for an apt epistemic system for interdisciplinarity: accommodating the study of complex problems, normative pluralism, integration and what we call meta-pluralism. Here, meta-pluralism can be understood as a pluralism that can incorporate both pluralist as well as monist epistemic systems.

There seems to be a fundamental tension at the heart of thinking about an epistemic system for interdisciplinarity. Interdisciplinary projects inherently have a form of normative pluralism, which leads to projects in which different disciplines with their different epistemic systems are combined. However, these epistemic systems may be fundamentally different, which can lead to great obstacles for integration. The demand for integration leaves little room for epistemic systems to merely exist side by side. If epistemic systems cannot merely exist side-by-side because of the demand for integration, it means that somehow these systems, even if they are fundamentally different, should be brought in accordance with each other. This taps in to ongoing philosophical discussions about a possible synthesis between fundamentally different epistemic systems, which may dissolve the tension described above. These discussions may help develop our theorising about an epistemic system for interdisciplinarity. We explore these questions alongside a case of interdisciplinary failure: the Mill Town example, where a diverse team of researchers inadequately works together to produce an interdisciplinary course, which ends in misunderstandings and disagreement over what ‘proper science’ is.

In analysing different in-depth discussions of possible synthesis, the tension between integration and normative pluralism and the problem of accommodating meta-pluralism remains. We propose different possible directions for thought, which build on the difference between fundamental and non-fundamental epistemic goods. If the epistemic divide is based on disagreement over fundamental goods, true integration will be very hard to reach, and it may be more feasible to change to a multidisciplinary research design, or to value the interdisciplinary discussion and process as the product of the collaboration. If the disagreement is over non-fundamental epistemic goods, it may be very fruitful to consciously discuss differences in justification, reliability, evidence, reasons, theory, method, concepts, assumptions, values, outcomes and process to come to a joint epistemology for the project.

# The Elephant in the Room in 21st-Century Bio-Robotics: The Biomimetic Principle

*Marco Tamborini*

*TU Darmstadt, Germany*

marco.tamborini@tu-darmstadt.de

In recent years, bio-inspired robots have shaped numerous domains of technical and scientific production. Bio-inspired robots are now employed in all areas of industry, medicine, architecture, and even culture. The recent proliferation of robot construction has prompted philosophers, historians, and sociologists of science to reinterrogate the concept of the robot and organisms. In particular, several studies have examined the elements of continuity and rupture between bio-robotics and the use of automata in earlier centuries. While these studies are important for examining the knowledge claims of contemporary robotics, they have neglected important elements in their investigation of the concept of the robot. Particularly, what is missing is a philosophical investigation of the mimetic principle in use in bio-robotics. In fact, in contemporary biorobotics, the way in which robots mimic the form-structure of organisms influences, limits, and enables the potential interaction between these machines and other animals. In my presentation, I will ask a simple question: what is the role of biomimetic and bio-inspired processes in the different practices of biorobotics?

The argument I defend in my presentation is that despite the wealth of studies on (bio)robotics and embodied AI, the philosophical and theoretical presuppositions and claims of validity of the main theoretical pillar in these disciplines, i.e., the biomimetic principle, has always been taken for granted and therefore not addressed in-depth. In short, the biomimetic principle is the big elephant in the room of the philosophical and historical inquiry on bio-robotics which needs to be fully addressed to understand the epistemic aims and differences within the various practices of bio-robotics.

To develop a philosophical taxonomy of the biomimetic principle in use, I will examine several emblematic cases in which the biomimetic principle operates differently to produce (biological) knowledge. In the conclusion of my presentation, I will return to the elephant in the room and suggest how to address it further in a fruitful way.

## Causal Complexity and the Causal Ontology of Health-Related Quality of Life Model

*Hong-Ui Tenn*
*National Yang Ming Chiao Tung University, Taiwan*
hong5ui7.sh09@nycu.edu.tw

Patient-centered care (PCC) promotes the kind of healthcare that values patients' rights, perspectives, and autonomy. Clinical practitioners usually employ health-related quality of life (HRQL) measurement tools to help them assess how well they implement PCC. HRQL is a construct that consists of different dimensions of patients' health conditions, such as biomedical factors, functional status, general health perception, and overall quality of life (McClimans, 2019; Wilson and Cleary, 1995). HRQL measurement tools aim to develop ways of measuring HRQL. Developing an HRQL measurement tool needs a theoretical model. Wilson and Cleary (1995) developed the most widely-used theoretical model that informed the design or development of HRQL measurement tools (Bakas et al., 2012).

In this paper, I will point out that Wilson and Cleary's model implicitly instills a causal bias into the current HRQL measuring practice, even though they do not explicitly endorse any causal ontology in their model (1995, p. 60). Based on my literature analysis, most of the HRQL research guided by Wilson and Cleary's model has the same type of causal hypotheses, i.e., from biomedical factors to non-biomedical factors. Causal hypotheses regarding how non-biomedical factors cause biomedical factors are rarely investigated. This causal bias is an obstacle for implementing PCC because it implicitly directs researchers' attention away from how patients' values, preferences, and overall quality of life can causally affect their HRQL.

To rectify this implicit causal bias that impedes PCC implementation, I will propose a way to strengthen the causal ontology of Wilson and Cleary's model. I will employ Rocca and Anjum's (2020) notion of causal complexity to modify Wilson and Cleary's model. According to Rocca and Anjum, causal complexity means that variables from different dimensions of a patient can cause each other or co-cause an illness. I propose to change how Wilson and Cleary present causal connections in their diagram to represent their theoretical model. My proposed changes will provide clear guidance and motivation for clinical researchers to investigate how patients' values, preferences, and overall quality of life can causally affect their HRQL.

## Characterizing and Measuring Racial Discrimination in Public Health Research

*Morgan Thompson*
*Universität Bielefeld, Germany*
morganthompson28@gmail.com

Researchers in public health need to link a characterization of racial discrimination to their methods of measuring racial discrimination in order to explain the effects of racial discrimination on health. One important way to measure racial discrimination is to measure experiences of racial discrimination. By focusing on experiences of racial discrimination, researchers more clearly identify one potential causal pathway from experiencing racial discrimination to its negative psychological impacts to their negative mental and physical health outcomes. In this research, how are characterizations of racial discrimination linked to measurement tools, such as via operational definitions (Feest 2005)? And which characterizations and measurement tools are required to fulfill the concept's causal explanatory goal (Brigandt 2010)?

Many researchers treat racial discrimination as if one characterization and its operational definitions are sufficient for research on the causal explanatory pathway from racial discrimination to racial health disparities. I demonstrate this claim by analyzing the characterizations and operational definitions underlying Williams's Everyday Discrimination Scale (1997) and Krieger's Experiences of Discrimination scale (Krieger et al. 2005). In both cases, these scales require participants to first identify clear instances of discrimination (for Krieger's scale, specifically racial, ethnic, or color-based discrimination).

Yet, feminist and critical race theorists have identified two different features of experiences of racial discrimination, which I argue constitute challenges to this characterization of racial discrimination. First, discrimination may be experienced as intersectional in that the discriminatory experience is due to membership in multiple social groups (Harnois, Bastos, and Shariff-Marco 2020). While Williams's characterization and scale can be adapted to measure intersectional discirmination, Krieger's cannot. Second, some experiences of discrimination can be ambiguous to the victim whether the instance was a case of discrimination at all. This type of discriminatory experience is motivated by attributionally ambiguous microaggressions, which are everyday slights that might be attributable to the victim's belonging to particular social groups (see Rini 2021). This second type of discriminatory experience constitutes a more fundamental challenge to the characterization of racial discrimination employed by Williams and Krieger. In fact, the problem also extends to scales of microaggressions (e.g., the Racial Microaggressions Scale, Torres-Harding, Andrade, and Romero Diaz 2012).

My thesis is that we need different characterizations of racial discrimination (and different measurement tools) to satisfactorily elaborate the causal pathway from experiences of racial discrimination to racial health disparities. I sketch a view that connects the two characterizations of racial discrimination to different psychological effects, which in turn could lead to different negative health outcomes. Clearly attributable cases of discrimination (measured by Krieger's and Williams's scales) are more likely linked to negative health impacts via psychological moderators like anger or coping mechanisms like John Henryism; whereas ambiguously attributable cases of discrimination (not currently measured) are more likely to be linked to negative health impacts via psychological moderators like perseverative cognition, depressive rumination, and chronic stress. If my arguments are correct, public health researchers need multiple characterizations of racial discrimination (and new measurement tools) to satisfy their causal explanatory goals.

References

Brigandt, I. (2010). The epistemic goal of a concept: accounting for the rationality of semantic change and variation. Synthese, 177(1), 19-40.

Feest, U. (2020). Construct validity in psychological tests–the case of implicit social cognition. European Journal for Philosophy of Science, 10(1), 1-24.

Harnois, C. E., Bastos, J. L., & Shariff-Marco, S. (2020). Intersectionality, contextual specificity, and everyday discrimination: Assessing the difficulty associated with identifying a main reason for discrimination among racial/ethnic minority respondents. Sociological Methods & Research,
0049124120914929.

Krieger, N., Smith, K., Naishadham, D., Hartman, C., & Barbeau, E. M. (2005). Experiences of discrimination: validity and reliability of a self-report measure for population health research on racism and health. Social science & medicine, 61(7), 1576-1596.

Rini, R. (2021). The Ethics of Microaggression. Routledge. Sue, D. W., Capodilupo, C. M., Torino, G. C., Bucceri, J. M., Holder, A., Nadal, K. L., & Esquilin, M. (2007). Racial microaggressions in everyday life: implications for clinical practice. American psychologist, 62(4), 271-284.

Torres-Harding, S. R., Andrade Jr, A. L., & Romero Diaz, C. E. (2012). The Racial Microaggressions Scale (RMAS): a new scale to measure experiences of racial microaggressions in people of color. Cultural Diversity and

Ethnic Minority Psychology, 18(2), 153.

Williams, D.R., Yu, Y., Jackson, J.S., and Anderson, N.B. "Racial Differences in Physical and Mental Health: Socioeconomic Status, Stress, and Discrimination." Journal of Health Psychology. 1997; 2(3):335-351

---

## Fictionialism and Inquiry

*Adam Toon*

*University of Exeter, United Kingdom*

a.toon@exeter.ac.uk

My aim in this talk is to develop an approach to epistemology that can do justice to the importance of tools and social practices in inquiry. To do so, I will draw on a new account of the nature of the mind known as mental fictionalism (Toon, 2016, 2021).

According to the representational theory of mind (or representationalism), mental states are inner representations. In contrast, mental fictionalism claims that the idea of the mind as an inner world of representations is a useful fiction. We treat people as if they have representations inside their heads that express their beliefs or desires, but we don't take this too seriously. In fact, our conception of the mind is fundamentally metaphorical: we project the "outer world" of human culture (especially language) onto the "inner world" of the mind.

Many of our greatest thinkers about knowledge have been committed representationalists. Despite their disagreements, Descartes, Locke, Berkeley and Hume all accepted the basic vision of the mind as an inner realm of representations. Much of contemporary cognitive science shares this vision—although it adds, of course, its own technical sophistication and terms of art. Not surprisingly, representationalism shapes its proponents' approach to epistemology. On this view, the central questions of epistemology concern the nature of our inner world and its relationship to the world outside—if, indeed, there is a world outside.

Not all have accepted this approach, of course. Ryle (1949) famously rejects the idea that the mind is an inner world. He argues that epistemology should focus instead on "the structure of built theories" (or the "Grammar of Science"). It is here that the epistemologists' favoured terminology (e.g. "concepts", "ideas", "inferring") finds its proper home. The difficulty arises when these terms are misapplied to an inner world:

"the great epistemologists, Locke, Hume, and Kant, were in the main advancing the Grammar of Science, when they thought that they were discussing parts of the occult life-story of persons acquiring knowledge. They were discussing the credentials of certain sorts of theories, but they were doing this in para-mechanical allegories" (ibid., p. 299)

I will show that mental fictionalism provides a different, but importantly related, approach to epistemology. Like Ryle, I will suggest that many of our key epistemic terms find their home in print—or, more accurately, in the world of public representations, tools and social practices—from field guides to computer databases. And yet, if our application of these terms to an inner world is an allegory, it is an invaluable one. For it is an allegory that lies at the heart of our conception of the mind in inquiry.

References

Ryle, G. (1949). The Concept of Mind. London: Hutchinson.

Toon, A. (2016). Fictionalism and the Folk. The Monist, 99: 280-295.

Toon, A. (2021). Minds, materials and metaphors. Philosophy, 96: 181-203.

---

## Experimental control, validation, and replicability in omics research

*Dana Tulodziecki, David Marshall Porterfield and Colin P.S. Kruse*

*Purdue University (Philosophy), United States*

dtulodzi@purdue.edu

'Omics research' describes a collection of allied biotechnology approaches similar to or enabled by DNA sequencing technology developed for the human genome: genomics (DNA) follows into transcriptomics (RNA), proteomics (protein), and metabolomics (small metabolite molecules), mirroring the natural flow of biological information (genotype) into biological expression (phenotype). Omics research is fast becoming a standard approach for contemporary life sciences research, especially in medicine, where it forms the basis of biomedical technologies seeking to give rise to personalized and precision medicine.

Our goal in this paper is to argue that omics research is plagued with significant epistemological problems: when performing omics experiments, sample preparation, experimental error, and sample biological variability are all areas in which validation, experimental control, and replicability are lacking.

First, due to the complexity and expense of omics experiments, most omics experiments are not run in replicate. Under 'typical' (non-omics) laboratory conditions, during the course of an experiment, a specimen is sampled more than once and each individual sample processed separately in order to determine both (a) the reliability of the sampling process, and (b) the representativeness of the sample. In contrast, during the course of many omics experiments, usually each sample is analyzed a single time. In some even worse scenarios, multiple samples that should have been processed separately (such as samples from different plant specimens that are extremely valuable individually) are consolidated and processed together as one mixed sample. We will argue that therefore both (a) and (b) are called into question; as a result, it is unclear to what extent the data from such experiments is compromised. This has potentially far-reaching consequences, since such data might be used as the basis for policy decisions (specific health risk factors) and research investments (lead drug compounds, tax dollar research grants, etc.).

Second, even omics instrumentation standards themselves are not unproblematic. Some existing standards are supposed to ensure precision and accuracy by calibrating individual samples for analysis to their respective ribosomal RNA (an abundant and species independent type of gene). We will argue that these standards involve a significant underlying assumption, namely that ribosomal RNA is a consistent – and thus a universally appropriate – calibration reference for all organisms. As we will show, however, there is good reason to think that there are exceptions to this.

We will then illustrate the magnitude of these epistemological issues through an example of a serious biological mishap: the mutational drift in Henrietta Lacks's famous cells. We will argue that the adequate application of proper epistemological standards involved in omics analyses could have prevented inconsistencies and potentially erroneous findings in well over 500 peer-reviewed manuscripts.

We will end by categorizing the different problems outlined above and argue that, while some of them are practical and thus at least have a theoretical solution, there also remain more serious epistemological issues for which it is not easy to see how they could be resolved, even in principle. To wrap up, we discuss the implications of this for the results obtained through such research.

## Instruments mediating between theoretical conceptualization and practical engineering in 18th-century hydromechanics

*Jip van Besouw*
*Vrije Universiteit Brussel, Belgium*
jip.van.besouw@vub.be

Although no one would deny the indispensability of instruments to scientific practice, philosophical reflection on their precise roles is sparse. A conventional presumption is that any scientific instrument is built for a specific experimental setting and unproblematically fulfils a specific purpose in that setting. This presumption leaves little room for the idea that instruments may have a variety of functions and, like any technology, give shape to the actions of their users. In this talk, I will use a historical case study to provide support for this idea.

My case study comprises fountains used in eighteenth-century hydromechanics. The history of hydromechanics has often described this era as one in which 'hydraulic engineers who observed what could not be explained' were isolated from 'mathematicians who explained things that could not be observed'. Although hydromechanical theory was indeed far from providing direct solutions to engineers, I will show that significant dynamics nevertheless existed between the engineering and theoretical hydromechanical practices. These dynamics were produced via scientific instruments. Taking such dynamics into account will therefore help to broaden our view of what instruments were useful for.

My talk will focus particularly on fountains developed to measure the velocity of liquids flowing out of a reservoir. Beginning with water tanks with a single hole at the time of Torricelli, these fountains developed into sophisticated instruments in which friction and the height of the reservoir could be carefully controlled in the work of Daniel Bernoulli and Willem Jacob 's Gravesande, around 1740.

I will discuss how Bernoulli, 's Gravesande, and others used these fountains to bridge between theoretical and engineering practices. First, the instruments allowed them to observe and model controlled flows, which they compared with measurements on the flows of actual rivers, made by hydraulic experts. Second, the level of control achieved by the instrumental setup allowed them to link with conceptual frameworks available from the mechanics of solid bodies. Bernoulli and 's Gravesande adapted the concepts of force, pressure, and velocity—still open to discussion in post-Newton mechanics—such that they could be operationalized in the measurement of both controlled and real-world flows.

Quantification and visualization of hydromechanical concepts required engagement with both theoretical and practical issues. By getting progressively embedded and operationalized through the fountains, concepts such as velocity and force became useful for other purposes, too. As I will show, they were also transposed back to engineering situations, where attempts were made to predict the behavior of rivers in terms of these concepts. Force, as manifested through the fountains, also became a measure of the efficacy of various machines in raising water from inundated terrain. The fountains and water-raising machines, finally, were used in educational settings to visualize mathematical relations between forces, pressures, and velocity. Thus, my case study helps us to recognize the variety of roles scientific instruments may play—visualization, measurement, modelling, or the study of engineering apparatus—and to reconceive oversimplified distinctions between scientific theory and practice.

---

**Reassessing Bas van Fraassen's empiricist philosophy of science**

*Maarten Van Dyck*
*Ghent University, Belgium*
maarten.vandyck@ugent.be

Bas van Fraassen has occupied an absolutely central position in the philosophy of science following upon the publication of his The Scientific Image in 1980. Usually identified with the semantic conception of scientific theories and his anti-realist position, it is probably fair to say that the general impression of Van Fraassen's philosophy is that is primarily formally oriented and assumes an overtly idealized picture of scientific practice, even if he acknowledges pragmatist insights at different points in his epistemology. Okruhlik (2009) has argued, e.g., that even if his work after The Scientific Image has been characterized by a growing attention for the historical and pragmatic dimension of science, it remains wedded to a purist conception of epistemic considerations as belonging to a separate realm which also implies "that there is only one aim of science and one criterion against which to judge its success" (p. 691) (see also Okruhlik 2014). In my paper, I will show the hermeneutic dimension that lies that at the heart of Van Fraassen's empiricism and which cannot simply be brushed aside as not apt to deal with the very real diversity characterizing scientific research practices.

My paper proceeds in three steps. Firstly, I will highlight the relations between Van Fraassen's empiricism (a philosophical stance), his voluntarism (a position in epistemology), his empiricist structuralism (an analysis of scientific representation), and his constructive empiricism (a view on the aim of science). While each of these positions has been widely debated, the important relations between each has almost never been analyzed in any detail. I will show how placing Van Fraassen in a broadly speaking neo- or post-Kantian perspective helps us to bring these relations into focus. Secondly, it will emerge that the historicity of knowledge, which Alan Richardson had already identified as absolutely central to Van Fraassen's epistemology (Richardson 2011), is crucial for all aspects of his philosophical thinking. This will allow me to stress the often implicit but overall guiding importance that history of science has for Van Fraassen's analyses, which are usually more focused on logical and structural aspects of scientific theorizing. Thirdly, this will be used to reassess Van Fraassen's position within the debates on scientific realism. A surprisingly rich picture will be sketched about what it does mean to identify the aim of science for Van Fraassen. This cannot be simply read off from scientific practice, nor can it be straightforwardly established by an epistemological argument, but it requires a hermeneutic decision that is informed but not determined by empirical description of practices and epistemological argumentation. This hermeneutic dimension allows us to reconceive what is at stake within the debates on scientific realism for Van Fraassen, which will bring to light the thoroughly engaged character of his brand of empiricism: philosophical reflection on science is and cannot but be a historically situated practice.

References

Kathleen Okruhlik (2009). "Critical Notice. Review of Bas C. van Fraassen. Scientific Representation: Paradoxes of Perspective." Canadian Journal of Philosophy 39 (4), 671-694.

Alan Richardson (2011). "But what then am I, this inexhaustible, unfathomable historical self? Or, upon what grounds may one commit empiricism?" Synthese 178 (1), 143-154.

---

## Regimenting the explanatory potential of network models in psychopathology

*Dingmar van Eck*

*University of Amsterdam, Netherlands*

d.vaneck@uva.nl

In this contribution I aim to regiment the explanatory potential of network models in psychopathology (Borsboom 2017; Borsboom et al. 2019).

There is a new game in town in psychiatry and clinical psychology: the network approach to psychopathology. The network theory of mental disorders – the theoretical core of this burgeoning approach – conceptualizes mental disorders in terms of networks of causally connected symptoms. Mental disorders are understood as stable, dysfunctional states in which such networks can get locked. This conceptualization departs significantly from latent variable or common cause models of mental disorders in which symptoms are understood as arising from (and indicators of) underlying common causes, viz. common disease mechanisms or pathogenic pathways. According to advocates of the network approach, very few (if any) of such common disease mechanisms for mental disorders exist.

Network theorists thus assert that the explanation of mental disorders by reference to underlying common causes is going nowhere and propose an alternative explanatory strategy (Borsboom et al. 2019). Yet what precisely is meant by the term 'explanation' in the network approach is ambiguous (cf. de Boer et al. 2021). It is hence unclear what explanatory alternative to common cause explanations is exactly being offered by network theorists.

In this contribution I prize apart and elaborate different senses of the term 'explanation' as used in or afforded by the network approach to psychopathology. I unpack the explanatory potential of network models of mental disorders by relating the discussion to a recently developed (computational, dynamical) network model of panic disorder (Robinaugh et al. 2019) and a recently developed account of the explanatory power of dynamical models in cognitive science (van Eck 2018).

I regiment three types of explanation afforded by network models of mental disorders. The explanandum of the first type is the covariation between two or more symptoms. Such covariation can be explained by clarifying when a symptom is (and when it isn't) a difference maker for the occurrence of another symptom, relative to specific environmental constraints. I call this a "contextualized causal model" explanation (cf. van Eck 2018). The second type concerns mechanistic explanation, viz. clarifying how one symptom causes another by articulating the mechanism underlying the causal relation. The third type is also mechanistic, viz. the explanation of a mental disorder – conceptualized and described as a network of causally connected symptoms, locked in a stable, dysfunctional state – by describing the set of interrelated mechanisms that underlie the causal symptom-to-symptom relations specified in the network model. Such "extended" mechanistic explanations are currently not on offer but rather point towards a long term ideal.

I also clarify why the first and second type concern complementary explanatory projects rather than the first type being an elliptical version of the second.

This analysis offers network theorists a better informed explanatory alternative to common cause models and indicates that the network approach need not fall prey to the criticism that it merely offers descriptions rather than explanations of phenomena.

References

Borsboom, D. (2017). A network theory of mental disorders. World Psychiatry 16: 5-13.

Borsboom, D., Cramer, A.O.J., & Kalis, A. (2019). Brain disorders? Not really: why network structures block reductionism in psychopathology research. Behavioral and Brain Sciences 42: 1-63.

Robinaugh, D.J. et al. (2019). Advancing the network theory of mental disorders: a computational model of panic disorder. PsyArXiv preprint.

De Boer, N.S. et al. (2021). The network theory of psychiatric disorders: a critical assessment of the inclusion of environmental factors. Frontiers in Psychology 12: 1-13.

Van Eck, D. (2018). Rethinking the explanatory power of dynamical models in cognitive science. Philosophical Psychology 31: 1131-1161.

---

## On the heuristic role of economics models

*Melissa Vergara Fernández*
*Erasmus University Rotterdam, Netherlands*
melissa@mvergarafernandez.nl

For a while it has been standard in the philosophy of modelling in economics to distinguish between their having epistemic value and 'mere' heuristic value. The distinction seems to make reference to models' capacity to say something about the world—or from the model-user perspective, that it is possible to learn new empirical facts about the world through the use of a model. Only when a model has this capacity it is said to have epistemic value. Otherwise, it is heuristic, which, as Grüne-Yanoff (2013b) has suggested, would be akin to talking a walk or reading the newspaper; activities that scientists carry out in order to gain inspiration.

More recently, the distinction seems to have divided the profession: the sceptics regard theoretical economic models as 'merely heuristic'. They have suggested that models are either 'open-formulae', which only if they are 'filled out' with experimental data can become explanatory (Alexandrova, 2008); or conceptual explorations (Hausman, 1992, Chapter 5) or as yielding only the feeling of explanatoriness, but not the real deal (Alexandrova & Northcott, 2013; Northcott & Alexandrova, 2015). The optimists, by contrast, argue that theoretical models do have epistemic value, mostly by way of offering how-possibly explanations (Aydinonat, 2018; Grüne-Yanoff, 2013a, 2013b; Grüne-Yanoff & Verreault-Julien, 2021; Verreault-Julien, 2018).

But the optimists have somehow put themselves in an uncomfortable position. Somehow, they think that having mere heuristic value is insufficient to justify the trouble economists go through in building their models (Grüne-Yanoff, 2013b; Grüne-Yanoff & Verreault-Julien, 2021; Hindriks, 2008, 2013). In other words, if models in economics only have heuristic value, there is no way to justify the ubiquitousness of economic models across the discipline. So the optimists have claimed that how-possible explanations are epistemically valuable.

In this paper I shall argue that the distinction between heuristic and epistemic that has forced philosophers to take sides is not an apt one. There are two main reasons for this. First, if 'heuristic' is understood as "enabling discovery or problem-solving, especially through relatively unstructured methods such as experimentation, evaluation, trial and error, etc." (Oxford English Dictionary, n.d.), then there is hardly any model in economics that is not heuristic. Second, and more generally, the distinction has been historically contingent and relative to the role that theories have been thought to have. This brings us to evaluate the distinction between theories and models not just in economics, but also in philosophy of science. Neither field can claim to have a neat distinction between the two.

I will further suggest that the inaptness of holding to the epistemic-heuristic dichotomy has led the optimists to attribute epistemic value to certain theoretical models, often at the expense of mischaracterising the economic practice. The bottom line is that the dichotomy should be eschewed and the wide array of functions that models have in practice be finally recognised. I illustrate this with examples from economic geography and financial economics.

References

Alexandrova, A. (2008). Making Models Count. Philosophy of Science, 75(3), 383–404.

Alexandrova, A., & Northcott, R. (2013). It's just a feeling: Why economic models do not explain. Journal of Economic Methodology, 20(3), 262–267.

Aydinonat, N. E. (2018). The diversity of models as a means to better explanations in economics. Journal of Economic Methodology, 25(3), 237–251.

Grüne-Yanoff, T. (2013a). Genuineness resolved: A reply to Reiss' purported paradox. Journal of Economic Methodology, 20(3), 255–261.

Grüne-Yanoff, T. (2013b). Appraising Models Nonrepresentationally. Philosophy of Science, 80(5), 850–861.

Grüne-Yanoff, T., & Verreault-Julien, P. (2021). How-possibly explanations in economics: Anything goes? Journal of Economic Methodology, 28(1), 114–123.

Hausman, D. M. (1992). The Inexact and Separate Science of Economics. Cambridge University Press.

Hindriks, F. (2008). False Models as Explanatory Engines. Philosophy of the Social Sciences, 38(3), 334–360.

Hindriks, F. (2013). Explanation, understanding, and unrealistic models. Studies in History and Philosophy of Science Part A, 44(3), 523–531.

Northcott, R., & Alexandrova, A. (2015). Prisoner's Dilemma doesn't explain much. In M. Peterson (Ed.), The Prisoner's Dilemma (pp. 64–84). Cambridge University Press.

Oxford English Dictionary. (n.d.). 'heuristic, n. And adj.'. `https://www.oed.com/`

Verreault-Julien, P. (2018). How could models possibly provide how-possibly explanations? Studies in History and Philosophy of Science Part A, 73, 22-33.

---

## The Reference Class Problem of Variation

*Cristina Villegas*

*Konrad Lorenz Institute for Evolution and Cognition Research, Austria*

`crvilleg@ucm.es`

Evolutionary biology is famously undergoing philosophical discussions over the alleged need to extend, or even reconsider, some of its theoretical bases (the EES debate). This has led to a variety of accounts about it, some of which reflect there being a lack of common ground across evolutionary disciplines, in turn leading to a mutual lack of understanding. In this paper, I argue that a key aspect for understanding the challenges posed by these discussions is to consider the probabilistic nature of the models involved in evolutionary modeling and predictive practices. In particular, I argue that some developmentally-informed probabilistic models of evolution conceptualize their sample space differently to how classical evolutionary genetics models do.

Chance has been a matter of concern particularly for two aspects of the evolutionary process, namely for the spread, fixation and disappearance of variants in populations, responsible for changes in their composition and structure; and for the very origin of those variants, responsible for the possibility of those changes in the first place. The so-called "problem of variation" refers to the latter of these aspects—i.e. the origin of variation—, and it is the core of evo-devo vindications about the role of development in evolution, and one of the key sources of disagreement about the EES debate. It is a historical vindication of evo-devo that development biases variation in non-random ways, a statement in apparent contradiction with the classical idea that "chance reigns supreme" in variation. The fact that, in contemporary philosophy of biology, variation can be seen as a random phenomenon for some, while considered as not random in any defined expectation by others is the consequence of a discrepancy about variation and chance whose origin can be traced back at least to the very origin of evolutionary thought. However, understanding this discrepancy in contemporary biology requires, in addition, to consider current practices and models of evolution. While some recent works have pointed out that specifying the level of reference is crucial for understanding this mismatch, the connection of this fundamental idea with our understanding of predictions in evolutionary probabilistic models remains underdeveloped.

This paper bridges this gap by taking the concept of sample space to illustrate the significance of evo-devo vindications on the nonrandomness of variation, from the point of view of evolutionary models and probabilistic predictions. I first present how sample spaces are considered in classical evolutionary genetic models, and how this relates to empirical and conceptual discussions over the difference between selection and drift. Then, I show that these sample spaces cannot relate in the same way to the ideas about the randomness of variation. In particular, I argue that the modeling and predictive practices of classical evolutionary genetics models don't deal with the causal structure of variation the way they deal with ecological causes. Finally, I argue that, contrary to mere comparative approaches to evo-devo, the "developmental evolution", or devo-evo, agenda conceptualizes sample spaces in their models through variational tendencies, which can be in turn understood as variational probabilities.

# The Problem with Property Clusters

*Zina Ward*
*Florida State University, United States*
zina.b.ward@gmail.com

A long philosophical tradition conceives of natural kinds in terms of co-occurring properties (Broad 1920, Russell 1948). This tradition culminated in Boyd's (1989, 1991, 1999) influential homeostatic property cluster (HPC) theory, on which kinds correspond to families of properties held together by homeostatic mechanisms. Thinkers after Boyd loosened his requirements on natural kinds while preserving the basic insight that members of a kind cluster together (Chakravartty 2007, Slater 2015). Such theories of kinds are often characterized using a spatial metaphor: kinds are clusters in a high-dimensional property space.

In this paper, I discuss a pressing challenge for all clustering accounts of kinds. If we take kinds to be clusters of individuals in a property space, two questions arise: which properties comprise the space? And how are we to pick out individuals? Existing cluster theorists are overly sanguine about these issues. Many simply assume that individuals or objects can be identified prior to the identification of kinds (Franklin-Hall 2015). Others claim that the dimensions in the quality space are "natural properties," without adequately explicating that notion (Chakravartty 2007). I suggest that any clustering view of kinds requires adequate answers to these two questions, since the dimensions of the property space and the individuals plotted within it determine which clusters will be identified. The interdependence of kinds, properties, and individuals is further supported by recent research on biological individuality (Wilson and Barker 2019). There, the identification of biological individuals proceeds in tandem with the identification of biological kinds.

My proposed solution to these intertwined problems takes inspiration from William Whewell. In his Philosophy of the Inductive Sciences, Whewell points out that individuating objects and recognizing the relevant similarities between them is no more straightforward than delineating natural classes. He emphasizes that, "before we can attend to several things as like or unlike, we must be able to apprehend each of these by itself as one thing," a task that requires "mental operation as well as sensation" (Whewell 1840/1847, 466-7). Moreover, in addition to picking out objects, we also have to decide "what resemblances and differences" matter to their classification (ibid., 486). Hence, Whewell recognizes the challenge raised here. His solution is to argue that individuals, properties, and kinds should all be held to the same standard: all must be delimited so as to enable the formulation of "general, intelligible, and consistent assertions" (ibid., 473).

I argue that this proposal is broadly correct: individuals, properties, and kinds are all subject to constant revision in light of our continual search for general assertions. As Hacking (1994) puts it, "kind-making, classification, and generalization are...of a piece" (216). I suggest that ever-tighter clusters in property space facilitate prediction, explanation, and manipulation, since they allow for the formulation of more precise generalizations. As we seek to expand our inductive capacities, we iteratively refine the properties we impute to objects and the boundaries of objects themselves. Ideally, these refinements encourage the tightening of clusters, leading to adjustments in the kinds we identify.

Individuation of kinds can therefore be seen as a process of reflective equilibrium in Goodman's (1955) sense: it involves the modification of kinds, properties, and individuals all at once. It is decidedly not a process in which objects are individuated and important properties picked out prior to seeking kinds. I suggest that existing clustering theories of kinds take up this amendment, leading to a richer account of the practice of "kinding" (Kendig 2016).

---

## Scale Models in Climate Science: Using Temporal Scaling to Identify a Paleoclimate Analogue

*Aja Watkins*

*Boston University, United States*

ajawatki@bu.edu

Scale models are traditionally viewed as physical, constructed models that are useful because we can manipulate them and then convert properties of the model into properties of the target system. Unlike existing accounts of scale models, I argue that conversion of scale models' properties into properties of their targets is determined more by research context than by features of the model itself. Relatedly, I will argue that it is challenging to straightforwardly divide the properties of scale models into those shared with and not shared with their target systems. These insights may also apply to other cases of models, not just scale models.

In order to make my argument, I will examine an unusual class of scale models: paleoclimate analogues for contemporary climate change. I suggest paleoclimate analogues be seen as climate models, given the ways they are used to represent Earth's near-future climate. If paleoclimate analogues are models, they are concrete models. For the sake of argument, I suppose they are scale models, a plausible suggestion because there are scaling methods that need to be applied to them before they can be used. In particular, temporal scaling, which involves adjusting rates of paleoclimatic change (e.g., rates of CO2 release or temperature change) to account for the low temporal resolution data that we have about paleoclimate episodes, is needed in order to compare rates of change during paleoclimate episodes with the rates of change today. Briefly, temporal scaling works by noticing that there is a precise relationship between rates and the durations over which they are measured (a feature of the fractal nature of many natural processes). One can then adjust rates measured over certain durations, i.e., at certain temporal resolutions, to what these rates would have been if they had been measured over other durations.

There is a lot to say about paleoclimate analogues as scale models. For instance, paleoclimate analogues are naturally occurring and non-manipulable, unlike standard examples of scale models. Additionally, temporal scaling might be productively compared to downscaling, a process used to account for low spatial resolution in climate simulations. However, for the purposes of this talk, I will focus on two points. First, contrary to some accounts of scale modeling, scale models do not provide their own "key" by which to translate properties of the model into properties of its target. Paleoclimate analogues emphasize this point because it is unclear what duration/resolution to use to scale the rates, but I argue that this point also applies to other scale models as well. Second, I argue it is not straightforward to divide the properties of scale models into those that are or aren't shared with its target. In the case at hand, whether a paleoclimate episode and today's climate change share the same rates of change depends on how these rates are scaled, or at what temporal resolution they are compared. But I will argue that this is true of models in general, putting pressure on accounts of model-based science that rely on analogical reasoning.

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

*Oscar Westerblad*
*University of Cambridge, United Kingdom*
ow259@cam.ac.uk

Analytic philosophers cling to a conception of knowledge as propositional knowledge — a thoroughly unpractical view of epistemology, unfit for the image of science-as-practice. As the practice-turn and experimental turn in philosophy of science have taught us, the epistemic and practical achievements of the sciences are much more diverse than what the propositional conception of knowledge can account for. Propositional knowledge is static, but scientific practice is dynamic; science is not merely verbal, but best described by verbs. In this paper, I argue that the recent turn towards understanding in philosophy of science and epistemology can provide fruitful grounds for integrating achievements from practice-oriented philosophy of science with those found in the literature on the epistemology of science. Since understanding is not necessarily propositional, as I will argue, properly understanding scientific understanding can help us in thinking about the epistemic and practical achievements of science. This talk aims at formulating a pragmatist account of scientific understanding that fits within the image of science-as-practice. In the first part of the talk, I will detail a picture of science as given to us by practice-oriented philosophers of science. Following Hasok Chang, I take epistemic practices, activities, and operations as the units of analysis of scientific practice. I show how some notions of scientific understanding proposed by philosophers of science do not accommodate or fit into this picture of science; scientific activities are too varied and too dynamic to fit with conceptions of understanding that depend on explanation or propositional knowledge alone. In the second part of the talk, I formulate an account of scientific understanding that is able to accommodate this picture of science-as-practice, detailing a practice-oriented, pragmatist account of the epistemology of scientific understanding. Building on Joseph Rouse's detailed and deep account of conceptual understanding and conceptual articulation in the scientific image, I provide a reconstruction of two notions of understanding discussed by philosophers of science and epistemologists: pragmatic understanding and holistic understanding. I argue that the epistemic achievements of individual scientists' activities and operations are captured well by a notion of pragmatic understanding, while broader epistemic practices — being sense-making practices — is accounted for by holistic understanding. I suggest that pragmatic understanding is a matter of having the ability to perform activities that practically articulate methods and concepts, while holistic understanding is a matter of practically articulating locally coherent webs of semantic dependence. Holistic understanding, on this picture, depends on pragmatic understanding, grounding the epistemology of scientific understanding in practical (or pragmatic) activities. With these notions of understanding in hand, I have formulated pragmatist proposal for the epistemology of science, helping us understand how scientists gain understanding through their operations, activities, and practices, rather than by statically grasping propositions. This provides a promising way of bridging gaps between contemporary epistemology (of science) and practice-oriented studies of science.

---

## Idealization, De-Idealization, and Mechanistic Modeling

*Lauren Wilson*
*University of Minnesota, United States*
wils2306@umn.edu

The role of idealization within modeling has come under increasing scrutiny in philosophy. As many have noted, idealization seems misplaced in scientific modeling (at least on first glance), especially if one conceptualizes models as somehow representing reality or the real world. A concern about whether idealizations are misplaced in scientific modeling depends on how we interpret their purpose. Philosophers recognize that models are often best understood as tools for making our way in the world and that they do not aim at absolute truth (similar to a map). Instead, they provide a means for navigating our environment when they are accurate or appropriate with respect to our aims. On this general conception of modeling practices, idealization poses less of an issue, though some philosophers still posit that the best and most useful models will have circumscribed or limited idealizations, many of which can be removed secondarily. The process of removing idealizations from a model is de-idealization. De-idealization within the practice of scientific modeling is an emerging topic but has not received sufficient attention. This is partly because idealization and de-idealization are presumed to be reciprocal processes, which would mean that understanding idealization automatically yields an understanding of de-idealization. However, there are reasons to think this assumption is problematic (Knuuttila & Morgan 2019). This worry takes on added significance in domains of modeling where idealization has not been well studied, such as mechanistic models in biology (see, e.g., Love & Nathan 2015). We lack an adequate understanding of how mechanistic models can be idealized and therefore are handicapped in evaluating how they might be de-idealized. This paper attempts to fill that gap.

I begin by introducing idealization and its connection to modeling, distinguishing between idealization and abstraction for clarity. Then, I review existing literature on idealization and mechanistic explanation. With this as background, I go on to describe more generally how mechanistic models can be idealized in terms of their representation of organization, parts and activities, and spaciotemporal considerations. I then combine my framework for idealization in mechanistic models with the analysis of de-idealization as recomposing, reformulating, and situating in Knuuttila & Morgan (2019) to explore how de-idealization might be conceptualized in mechanistic models. To illustrate the value of this combination, I use a case study from neurobiology, the Hodgkin-Huxley model of nerve transmission, whose variations in structure correspond to both idealization and de-idealization choices. The longevity of the model and its place within the modeling literature make it a unique place to examine these types of modeling decisions. I find that idealization and de-idealization are not reciprocal and easily reversible processes, but rather unique processes that can occur through multiple methodologies and contain their own conceptual issues. I close with a call for more work on the topic of idealization and de-idealization in mechanistic modeling.

References

Knuuttila, T. & Morgan, M.S. (2019). De-idealization: No easy reversals. Philosophy of Science, 86(4), 641-661.

Love, A., & Nathan, M. (2015). The idealization of causation in mechanistic explanation. Philosophy of Science, 82(5), 761-774.

---

## Who Makes the Choice? Artificial Neural Networks in Science and the Non-Uniqueness Problem

*Siyu Yao and Amit Hagar*

*Indiana University Bloomington, United States*

siyuyao@iu.edu

Machine learning with artificial neural networks (ANNs) has become an essential part of many scientific inquiries, promoting novel discoveries. Here we distinguish between the output-oriented approach that utilizes the strong input-output matching power of ANNs while regarding them as black-boxes, and the feature-oriented approach that seeks to reveal and learn from the features captured by ANNs to achieve their optimal performance. Equipped with network interpretation strategies developed to illuminate the inner workings of ANNs, the feature-oriented approach is expected to overcome the potential danger of opaque automated reasoning and to reveal novel correlations or even causal mechanisms in the target domain. In this paper, we discuss the extent to which the feature-oriented approach meets these expectations and what its possible limitations are in scientific practices.

We distinguish three types of features involved in this approach: those appearing as network parameters (mathematical features), those revealed by network interpretation strategies (diagnostic features), and those that scientists expect to find in the target domain and could be understood in terms of concepts and theories therein (real-world features). The feature-oriented approach aims at obtaining knowledge about real-world features from the former two. However, we argue that the three types of features do not naturally match with each other: mathematical features determine the network performance, diagnostic features only partially summarize mathematical features, and neither mathematical nor diagnostic features imply any counterpart real-world features. Given this mismatch, we identify an epistemic non-uniqueness problem: multiple alternative mathematical or diagnostic features may stand out when scientists attempt to adopt them to enrich science, but scientists lack the knowledge to properly interpret them, match them to real-world features, and justify their choices among them.

We illustrate this epistemic non-uniqueness problem with a case study in cosmology. Cosmologists apply convolutional neural networks to weak gravitational lensing images in order to find features that are informative about the early universe but elude traditional statistical methods. We show how a variety of machine-captured features arise in this process and how scientists' choice among them is not well-justified. We further demonstrate with the case that this non-uniqueness problem cannot be solved even by rationalizing and interpreting machine-captured features with existing theoretical and methodological frameworks in the scientific domain, as this leads to a circularity when those features are expected to carry evidentiary power.

Given the non-uniqueness problem, we suggest that one should be cautious about the claimed creative roles and credibility of ANNs in science. The feature-oriented approach should stick to its heuristic value of offering a pool of features for further investigation, instead of being used evidentially to justify existing theories or to serve as the foundation for new theories or methodologies. We further suggest that scientists should be transparent about the multiplicity of features and always clarify on what ground they rationalize and choose those features. Moreover, the scientific community should promote plurality both by encouraging different diagnostic strategies and rationalizing as many features as possible with competing theories.

---

## Roles of scientific generalizations beyond explanation: The case of collective cell migration

*Yoshinari Yoshida*
*University of Minnesota, United States*
yoshi077@umn.edu

What value do generalizations have in science? Traditional philosophy of science would answer this question by appealing to contributions of generalizations to scientific explanations; generalizations are valuable because they enable us to explain phenomena, e.g., by serving as a universal claim in a deductive inference, providing a basis under which different phenomena are subsumed, or indicating invariance of causal relationships (Hempel 1966; Kitcher 1981; Woodward 2001). However, generalizations play broader roles in science than merely enabling explanations. This presentation focuses on one such role and provides an account of generalizations that is more sensitive to scientific practice. Generalizations (including their pursuit and development) promote productive interactions between subcommunities within an area of inquiry. This facilitates investigations of individual objects of research. As an example, I examine recent research on collective cell migration in developmental biology. This phenomenon has been studied by employing various seemingly unrelated biological systems from different organs and species, such as fruit fly ovary, vertebrate blood vessels, and rodents' mammary gland (e.g., Scarpa and Mayor 2016). The articulated cellular and molecular mechanisms are heterogeneous across biological systems. However, researchers have formulated "principles" across those mechanisms. Each of these principles is a generalization that concerns a specific aspect of collective cell migration and applies not universally, but to a certain range of biological systems. By comparing these principles with the standard forms of explanations in the area (i.e., detailed mechanistic explanations and highly formalized mathematical explanations), I argue that these principles are only weakly explanatory. If so, what roles do they play in research? By inquiring how the principles have been sought and formulated, I illustrate that they have played the role of promoting "mutual informing" between researchers studying different biological systems. In particular, I focus on the nonuniversality and multiplicity of these principles as an important resource. The principles support different cross-system comparisons, and together, they provide a platform in which mechanisms that operate in different biological systems are compared in different respects. This facilitates investigation and characterization of the individual mechanisms. To understand the roles of these principles, one should pay close attention to how the multiple, individually local and limited generalizations function together to facilitate research in the area.

References

Hempel CG (1966) Philosophy of natural science. Prentice Hall, Englewood Cliffs

Kitcher P (1981) Explanatory unification. Philosophy of Science 48(4): 507–531

Scarpa E, Mayor R (2016) Collective cell migration in development. Journal of Cell Biology 212(2):143–155

Woodward J (2001) Law and explanation in biology: Invariance is the kind of stability that matters. Philosophy of Science 68(1):1–20

# Abstracts of Posters (alphabetical by last name of lead author)

### Epidural Use and the Medicalized Birth Dispositif

*Robyn Allen*
*Brandon University, Canada*
Allenr18@brandonu.ca

Epidurals are key interventions in medicalized birth. Although epidurals can produce iatrogenic harms and cause negative outcomes, they are considered standard practices in Canada. Epidural use is guided by both hospital policies and professional standards of care. In this poster presentation I provide a new conceptual analytic for considering the ways that policy and clinical practice can intersect to produce harms and inequalities. I do so by employing a socio-historical analysis, which deploys an original suturing of Michel Foucault's concept of power and Gilles Deleuze's concept of the folded force. I explain how epidurals become a technique of medical authority subjectifying the unruly birthing body. Epidurals arise historically from (1) solutions to parturient mortality rates through pain management, and (2) become the dominant form of pain management despite its iatrogenic outcomes. Epidurals have been used (3) outside of the evidence-based medicine (EBM) paradigm with a coercive effect, and (4) are justified by professionals under the ideal of the painless birth. This study examines one rural hospital site in rural Manitoba, Canada, where the rates of epidural use and cesarean section are used above the provincial average and national averages. By deploying this conceptualization of the folded force, my analysis provides an explanation of the overuse of epidural at this site, and also provides a theoretical and historical method to explore the causes of health inequities at the intersection of health practices, knowledge, and clinical technologies.

---

# The 'Black Box Problem' of Artificial Intelligence-Based Clinical Decision Support Tools: An Illustration in Otolaryngology – Head and Neck Surgery

*Christopher Babu*[a], *Hal Rives*[b], *and Anaïs Rameau*[c]

[a,b,c] *Weill Cornell Medicine, United States*

[a]csb4001@med.cornell.edu; [b]har4001@med.cornell.edu; [c]anr2783@med.cornell.edu

The development of artificial intelligence (AI)-based clinical decision support tools in otolaryngology-head and neck has the potential to revolutionize care delivery in a poorly accessible surgical specialty. For example, pathomics software can now recognize cancers where traditional methods fail to do so, while voice recognition programs are able to diagnose, categorize and prognosticate voice and speech disorders with comparable accuracy to experienced physicians. (1) Despite the far-reaching benefits of AI, the inexplicability of its intrinsic design to external operators poses an ethical dilemma that currently limits its ability to be introduced into clinical and surgical practice. This paper outlines the aforementioned 'black box problem' using the illustration of diagnosing head and neck cancer, and presents arguments surrounding the implementation of AI technology into contemporary medical practice. First, we apply Wadden's and Liao's definition of the black box problem as a paradox that precludes the use of AI based technology. (2,3) We then expand upon well known vulnerabilities of AI, including its susceptibility to one-pixel attacks, limited external validity and the potential for bias within training algorithms that may adversely impact specific demographic groups in healthcare. (4) The dilemma of deep learning algorithms, therefore, is not merely just a matter of patient safety, but also one that interfaces with the ethical principles of beneficence and non-maleficence. Subsequently, we present counterarguments in favor of AI software using examples from medical history that have relied on unknowns, including poorly understood mechanisms of action for widely prescribed drugs and diagnostic tools that are still used despite a limited understanding of their precise underpinnings. Finally, we conclude our arguments by describing advances in post hoc analysis, which have the potential to not only open the black box, but make its contents increasingly visible to users of the system and thus, may represent a solution to AI's epistemological challenges.(5) AI has the potential to increase access to care and drastically alter the landscape of contemporary otolaryngology. This potential has to be weighed against the limitations of the technology and ways to remedy them. Though surgeons are regularly exposed to new technology, the epistemological opacity of AI present unique challenges that require multidisciplinary reflection prior to implementation.

References

1. Tama, B. A., Kim, D. H., Kim, G., Kim, S. W., & Lee, S. (2020). Recent Advances in the Application of Artificial Intelligence in Otorhinolaryngology - Head and Neck Surgery. Clinical and experimental otorhinolaryngology, 13(4), 326–339.

2. Wadden J. J. (2021). Defining the undefinable: the black box problem in healthcare artificial intelligence. Journal of medical ethics, medethics-2021-107529.

3. Liao, S. M. (2020). Ethics of Artificial Intelligence. Oxford University Press.

4. Siontis, G., Sweda, R., Noseworthy, P. A., Friedman, P. A., Siontis, K. C., & Patel, C. J. (2021). Development and validation pathways of artificial intelligence tools evaluated in randomised clinical trials. BMJ health & care informatics, 28(1), e100466.

5. Kenny, E. M., Ford, C., Quinn, M., & Keane, M. T. (2021). Explaining black-box classifiers using post-hoc explanations-by-example: The effect of explanations and error-rates in XAI user studies. Artificial Intelligence, 294.

---

## A functional account of causation: from type to token

*Sander Beckers*

*University of Tübingen, Germany*

srekcebrednas@gmail.com

In his recent book Woodward defends a functional account of causation as an alternative to metaphysical, descriptive, or other kinds of accounts. Different kinds of accounts impose different kinds of normative requirements that an analysis of causation ought to satisfy. A functional account distinguishes itself by primarily appealing to the practical and epistemic norms that guide the usage of causal claims in both science and in everyday life. The core idea is that causation is best analysed with respect to its usefulness in achieving particular outcomes through manipulations that are under our control. Simply put, X causes Y iff interventions on X are useful for achieving particular values of Y.

The first aim of this paper is to unpack the notion of usefulness at work here by offering a formal definition of Woodward's degree of sensitivity, which captures the extent to which a causal relation depends on the fulfilment of specific background conditions. Contrary to most prior work on causal strength, our approach does not rely on any probabilistic information about the causal model. Instead, we rely on detailed information about the causal equations that allows us to make the relevant background conditions explicit. We thereby obtain a more fine-grained comparison of the usefulness of different causes that complements existing probabilistic measures.

The second aim is to build formal accounts of both type and token causation on the basis of this degree of sensitivity. Although the relation between these two forms of causation has been the subject of much debate, there have been few attempts to make it both formally precise and functionally motivated. The developed accounts are meant to be as permissive as possible, in the sense that we want to include all pairs X and Y so that under some circumstances – however rare – interventions on X can be somewhat useful for achieving particular values of Y. Further selection of causal relations can then proceed on the basis of comparing their respective degrees of sensitivity.

Concretely, we proceed as follows: given a particular outcome Y=y, we consider all pairs X=x and X=x' so that there exist background conditions under which X=x achieves Y=y, and the degree of sensitivity of X=x for achieving Y=y is at least as small as the degree of sensitivity of X=x' for achieving Y=y. Type causation is then defined as the existence of a pair that meets this requirement. We prove several results regarding this definition and its relation to existing causal notions from the literature.

With this in hand, the definition of token causation – aka actual causation – is deceptively simple: all it takes is for the values X=x and Y=y, and for the relevant background conditions mentioned above, to be actualized in the particular context under consideration. We prove that this results in a slight generalization of a recent definition by Beckers. We functionally motivate actual causation by explaining how the observation of actual causes offers evidence for increasing the difference in degree of sensitivity between a cause X=x and its contrast value X=x'.

**The linear model and beyond : positioning scientific knowledge in pandemic times**

*Thomas Bonnin*[a] *and Elodie Giroux* [b]
[a]*Université Clermont Auvergne, France;* [b]*Université Lyon 3, France*
[a]thomas.bonnin.hps@gmail.com; [b]elodie.giroux@univ-lyon3.fr

The emergence of the Covid-19 pandemic at a global scale has been brutal and unexpected, and its current development, in all aspects, retains a degree of novelty and unpredictability. It is not surprising, then, that our society's governing bodies have been looking for resources to help them navigate this context of high uncertainty.

For many governments, scientific knowledge is thought to provide such guidance - hence the oft-repeated mantra to "follow the science". On this view, it is believed that public health decisions can be guided by objective data, and thereby avoid arbitrariness. By doing this, decision-makers demonstrate a belief in a linear relationship between science and policy: the reduction of political uncertainty can be done by reducing scientific uncertainty on the issue of interest.

We provide a critical analysis of this linear model. While seductive at first sight, the application of the linear model quickly runs into epistemic issues. For situations where uncertainty is minimal and values are shared by all concerned stakeholders, it is feasible and desirable to simply "follow the science". In most other situations, however, such an approach places unrealistically high expectations on what scientific knowledge can provide.

In the poster, we then sketches the conditions of a just contribution from scientific knowledge to political decision-making. Building on existing works in the philosophy of science, we argue that (a) transparency, (b) inclusivity and (c) scientific pluralism are cardinal values to uphold in this process. The respect of such values raises tensions and difficulties which are also crucial to document.

## How to Properly Investigate Human Cognitive Difference and Diversity?

*Ingo Brigandt*
*University of Alberta, Canada*
brigandt@ualberta.ca

In neuroscience and cognitive science, there already are different scientific approaches to investigate human cognitive difference and diversity. Brain organization theory is largely focused on finding out about sex differences in the brain, which are deemed to form a biological basis of gendered behaviour. Apart from the well-known publication bias (where negative findings about cognitive difference tend to not be published), I point to inadequacies of the analytical and representational frameworks often used by such studies. In contrast, cultural psychology and cultural neuroscience are more open to capturing a larger range of human cognitive diversity and viewing cognitive variation as modulated by social influences. Pointing to fruitful methodological resources that already exist in science while also making further methodological recommendations, this talk will discuss how one can and should properly investigate human cognitive variation so as to do justice to human diversity. My analysis centers on two aspects of methodology, both of which can restrict and bias, but also enhance research: (1) experimental and other practical investigative strategies, and (2) analytical and representational frameworks. The former include the reliance on rodent models and the use of brain imaging studies. Regarding the latter, even though there are serious practical and financial limits on investigating human cognitive diversity, I discuss how various analytical categories such as sex, gender, sexuality, race, culture and socioeconomic status can be fruitfully be employed. Studies should represent the variation within any such analytical category and attempt to charter the complex distributions among various individual cognitive traits. Finally, while many approaches study cognitive differences without attempting to offer a causal explanation for them, I argue that the kind of explanatory framework that one could pursue can still have a significant impact on such research. For the breadth of an intended explanatory framework (e.g., accounting for neurocognitive variation also in terms of such socio-environmental influences as gendered behaviour) impacts what kinds of data a study gathers and how its findings are represented and connected up with previous studies.

## Generalisations for Cell Biological Explanations: Distinguishing between Principles and Laws

*Sepehr Ehsani*
*University College London, United Kingdom*
ehsani@uclmail.net

Laws have figured in the development of modern biology (e.g. Mendelian laws of inheritance), but there is a tacit assumption particularly in contemporary cell and molecular biology that laws are only of the 'strict' kind (e.g. the laws of motion or universal gravitation), which cell biology appears to lack. Moreover, the cell-biology-specific non-universal laws that do exist (e.g. scaling laws in biochemical networks within single cells) are few and far between. As discussed elsewhere (and not further argued for in this paper), mechanistic explanations face challenges in cell biology and their utility has been chequered in different biomedical areas. Just as laws and mechanisms figure in organic chemistry and ecology, fields that deal with lower- and higher-scale phenomena compared to cell biology, respectively, it should not be assumed that cell biology is somehow in a unique position where few or no laws could be discovered and used in its explanations. An impediment to discovering lawlike generalisations in cell biology is that the understanding of many cellular phenomena is still quite qualitative and imprecise. This paper is motivated by the premise that mechanisms and laws can both be in the foreground of explanations in cell biology and that a framework should be developed to encourage and facilitate the discovery of laws specific to and operative at the individual cell level. To that end, in the domain of scientifically-relevant non-universal (i.e. non-exceptionless) generalisations, which some philosophers equate with the notion of ceteris paribus laws (henceforth, 'cp-laws'), I propose that a cp-law might have one or more corresponding 'principles'. Using a running example of generalisations of oscillatory movements from physics with direct relevance to cell biology, I argue that whilst a cp-law and its paired principle(s) might have the same explanatory theme (e.g. explain the same phenomenon), a principle is broader in scope compared to its paired cp-law but less expectable or reliable in its predictions. This is because in this framework, principles are more qualitative and less numerically precise compared to cp-laws, reflective of our lack of precise understanding of the systems to which the generalisations apply. The principles–laws concept makes for a more lenient approach for what could count as a lawlike generalisation and can encourage the discovery of novel generalisations in areas of cell biology where no specific generalisations typically figure in explanations. A principle could be thought of as providing a programme for explanation, whereas its paired law provides explanations for specific instances. Newly posited principles could augment mechanistic explanations and also potentially lead to the discovery of corresponding cp-laws.

---

## How Philosophy of Science Can be For Practice: A Case Study of Teaching Models Used For Undergraduate Medical Education

*Mark Fedyk*[a] *and Nicolas Sawyer*[b]
[a,b] *University of California–Davis, United States*
[a]mfedyk@ucdavis.edu; [b]ntsawyer@ucdavis.edu

We would like to present a poster that illustrates what can be called teaching models — being models tuned towards the act of helping learners acquire incrementally more sophisticated mental models that eventually make them cognitively competent to engage in medical practice. The contrast here is with "research models", being models that are tuned towards making reliable inferences about otherwise difficult to access natural and/or social processes. Of course, medical practice does resemble scientific inquiry in that last regard, and some teaching models are also research models in medicine — but it is nevertheless the case that the models used in education serve different epistemic ends than most research models.

We would like to use our poster to display examples of four teaching models that we formulated for the pre-clerkship "Health Systems Science" curriculum at the medical school where we both work. These models are derived from research in the social sciences, philosophy of science, and medical education; each repurposes or otherwise synthesizes concepts from these disciplines. But the models also work together (a bit like a disciplinary matrix) to facilitate deeper inferences into, specifically, the relationships between the institutions that support the delivery of medical care in the USA and highly individual patients who will have unique interactions with those institutions, and whose care will be characterized by a unique "journey" through those institutions.

Perhaps the most interesting of our models — given the audience at SPSP — is one that we took to calling "The Health System Science Paradigm" when introducing it to our colleagues in the School of Medicine where we work. This name allowed us to leverage the fact that a concept of a paradigm has become commonplace academia and certain academy-adjacent discourses, though it has lost much of the meaning that Kuhn originally attached to the concept. In fact, the model has a structure that more closely resembles Lakatos' description of research programmes, having a core principle, a shell, and (in combination with some of the other models) the potential to generate clinically interesting inferences about patients. Because of its salience to SPSP attendees, we expect that about 40

But it is also this model that will help us share with SPSP attendees the power of the concept of a teaching model. Because the model employs (again) concepts and a structure that will likely be familiar to most SPSP attendees, it can bridge between their background knowledge and the practical cognitive skills that physicians must acquire in order to be competent. The model thus reflexively illustrates the idea of a teaching model through one of its key epistemic functions: its ability to act as an epistemic escalator, potentially moving those who interact with it to more advanced epistemic grounds with respect to both the nature of undergraduate medical education and the role that models can play in this process. Or, to put the same point more simply, this model can teach SPSP participants the concept of a teaching model, which we hope will make for an interesting and engaging poster presentation.

---

## Uberized Science is the New Black

*Sacha Ferrari*
*KU Leuven, Belgium*
sacha.ferrari@kuleuven.be

This early 21st century faces a severe skepticism toward science. Among these challengers, we can find various pseudo-scientific communities such as the Flat Earth Society, anti-vaxxers, astrologers, etc. Besides these groups, some individuals decided to take part in the fight against orthodox science on their own. Their solitary practices include, for example, seeking online information about the reliability of Covid vaccines, medical auto-diagnosis by consulting a health forum (like Doctissimo.fr in the French-speaking community), building up a home-made experiment in one's garden to detect the curvature of the Earth, and creating a DIY biology lab in one's garage (Simons, 2021). This new way of inquiring information inside a social-media context can be seen as an uberization of scientific knowledge. Science is no more a matter of hierarchical verticality (with experts above lay people), but something horizontal and isonomic where information is produced, shared and sought from equal to equal (with individuals all interacting at the same level). According to this view, each of us is considered as an autonomous entrepreneur, a self-made and self-employed scientist who can run his or her own epistemological 'business' by themself in order to obtain reliable knowledge without relying on the blind scientific authority. This is the gist of the uberized science.

This talk aims to understand the causes of the emergence of this new epistemic strategy. We will argue that this uberization has been produced by (at least) three different factors: a technological one, a metaphysical one, and a political one. First, the rise of the Internet and new means of communication and information allowed a democratization and liberalization of the speech market (Aupers and de Wildt, 2021; Bronner, 2003). This opened a huge theatre stage where orthodox and newcomer heterodox scientific ideas are relentlessly struggling. Facing these epistemic battles behind a screen, each of us is free to choose their winner without fearing the peers' judgement of the outside world. Secondly, this new knowledge paradigm is the result of the disenchantment of our Western cultures induced by the failure of the 20th century Grand Narratives such as liberalism, communism, and Enlightenment (Lyotard, 1984). The positivist credo of the 19th century, promising progress of mankind by the help of science, is no longer credible. In light of its conflicts of interest (with Big Pharma for instance) and its legitimization of injustices (e.g. craniology), scientific authority appears nowadays illegitimate as the only reliable source of knowledge. Lastly, the rise of a new kind of neoliberal sociopolitical structure gave a political and metaphysical autonomy and independence to individuals. This new spirit of the capitalism started with new management techniques within companies in the nineteen-eighties (Boltanski and Chiapello, 2018) and expanded to the organization of the society itself. We will argue that the axiology of this DIY scientific inquiry relies on the same background as this neoliberal axiology (in its philosophical perspective at least). This paper demonstrates that uberized science is not an epiphenomenon but is a profound turnover of our society.

References

Aupers S. and de Wildt L. 2021. Down the Rabbit Hole: Heterodox Science on the Internet. In: Houtman D., Aupers S., Laermans R. (eds) Science under Siege. New-York: Palgrave Macmillan.

Boltanski, L. and Chiapello, E. 2017. The New Spirit of Capitalism. Translated by Gegory Elliott. London: Verso.

Bronner, G. 2003. L'empire des croyances. Paris: Presses universitaire de France.

Lyotard, J-F. 1984. The Postmodern Condition: A Report on Knowledge. Translated by Geoff Bennington and Brian Massumi. Mineapolis: University of Minnesota Press.

Simons, M. 2021. 'Science Without Scientists': DIY Biology and the Renegotiation of the Life Sciences. In: Houtman D., Aupers S., Laermans

R. (eds) Science under Siege. New York: Palgrave Macmillan.

---

## A Study of Unlikely Scientific Success - Jeanne Altmann's 1974 Paper

*Samara Greenwood*
*University of Melbourne, Australia*
sjgreenwood@student.unimelb.edu.au

In 1974, Jeanne Altmann – part-time researcher in primate studies and active feminist - published her first sole authored, peer-reviewed paper. Under the unassuming title 'Observational Study of Behavior: Sampling Methods,' the paper was published in the leading journal 'Behaviour' and served as a practical guide for the working scientist. In the paper Altmann surveyed various methods for recording observed animal behaviour before making recommendations on their appropriate (and inappropriate) use. Despite its seeming modesty, the paper was wildly successful. As of 2022, the paper has been cited more than 17,000 times and has been widely credited with helping transform primate field studies from an inconsistent practice, heavily prone to male-centric observer bias, into a rigorous, quantitative discipline. While Altmann's paper has drawn some attention from historians and philosophers of science, it has been referenced mostly in passing or in relation to feminist philosophy of science. There has been no detailed analysis of how a seemingly modest paper on observational methods, at once infused with feminist-inspired concerns, was able to achieve such disproportionate success.

In particular, no one has adequately explained:

1. What motivated Altmann to write the paper? Although methodological difficulties were somewhat acknowledged in the primatology community, there had been no call for a systematic review and no-one, other than Altmann, appears to have identified the central problem.

2. How was Altmann able to successfully advocate for highly politicised feminist concerns within the scientific norms of her time and place? Altmann acknowledges her paper was influenced by feminist issues, yet no-one has adequately explained why the papers recommendations were taken up by a broad range of scientists, many of whom would have described themselves as non-feminist, or even anti-feminist.

3. Why were the recommendations of a junior, female researcher embraced by the scientific community? Recommendations to change practices are often met with significant resistance (or ignored) even when proposed

by individuals with high professional standing, so why the community enthusiastically took up the recommendations of a junior, female researcher in this instance also warrants further attention.

In this poster, I present an analysis of the context, formation, and impact of the paper in order to explore these questions and better understand the paper's broad effectiveness. The analysis is broken into two parts. First, I present the paper's background context, showing the societal and disciplinary conditions within which Altmann came to conceive her project. Second, I present a close analysis of Altmann's intervention, showing the experiences and ideas that informed her work, the formation and content of the paper and, finally, the impact the publication had over time. On laying out the many dimensions of the story, I conclude that a 'perfect storm' of factors combined to amplify the paper's effectiveness beyond its explicit goals. Additionally, Altmann capitalised on these conditions by not only solving a single, clear problem, but also by addressing an array of supplementary challenges which ranged across societal, disciplinary, and epistemic domains, which further contributed to the outsized success of this work.

---

## Multiple historic trajectories generate multiplicity in the concept of validity

*Yingying Han*

*Institute for Science in Society (iSiS), Radboud University, Netherlands*

yingying.han@ru.nl

Validity has been widely acknowledged to be essential in scientific research but the technical criteria that constitute such quality vary, resulting in multiplicity in the concept of validity and proliferating taxonomies of validity.

In this poster, I will present a brief introduction (section 1) followed by influential validity theories and discussions on three different practices in behavioral sciences, namely measurement and testing in psychometrics (section 2), experimentation in experimental psychology, and experimental and behavioral economics (section 3), and animal modeling in biomedical research (section 4). In each section, I put the validity taxonomies back into the theoretical, practical, and historical contexts and conclude with a discussion (section 5) highlighting two important aspects: validity 'of' what practices and validity 'for' what purpose, to connect the validity concepts to the rich contexts that gave rise to their specific meaning and relevance.

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

---

## Scientific Experiments Beyond Surprise and Beauty

*Anatolii Kozlov*

*Institut Jean Nicod, France*

anatolii.kozlov@outlook.com

Some experimental results in science are productively surprising or beautiful. Such results, for example, can be disruptive in their epistemic nature: by violating epistemic expectations they mark the phenomenon at hand as worthy of further investigation (Morgan 2005; Currie 2018; French and Murphy 2021; Ivanova 2022). Yet, surprise and appreciation of disruptive beauty are also psychological entities. Can it be that emotions beyond these two are also useful in scientific experimentation?

To answer this question, first, I conduct a structured sociological survey among practising scientists to capture the landscape of affective experiences common to their experimental practice. I identify the stage of learning the results of an experiment as the highest emotional point in the experimenting process, with a variety of emotions possibly taking place. For example, experimental results can be challenging, beautiful, boring, they can worry, amuse, make one sad, and so on. They also can drive meta-cognitive evaluations as well as motivate specific research-related actions.

From these results, I argue that the co-occurrence of affective, cognitive, and conative elements in experimental practice is not contingent and is a byproduct of epistemic evaluation of experimental results. I also suggest that different emotions can be useful for it.

I appeal to the following analogy. Suppose on the way to the office we are suddenly unsure whether we closed the door at home. This state of epistemic uncertainty can be accompanied by emotions of worry, ideations, and specific actions. We may start unfolding conditional scenarios ("If I closed the door, I must have heard the sound of turning the key"), appeal to episodic memory to test auxiliary hypotheses ("Do I remember hearing the lock? Do I remember holding the keys in my hand?"), devise actions (e.g.

calling a neighbour), and generate imaginings and motivations ("Thieves will come and steal everything! I should really figure out something about the door").

Experimental results are potential triggers of epistemic conflict. However, I claim that the fact of epistemic conflict (or lack thereof) doesn't say whether it comes from mistaken expectations or faulty experimental procedures. In this sense, the initial epistemic status of experimental results is inherently ambiguous. Hence, just like with the door and the keys, the experimenter is invited to evaluate different counterfactual possibilities (or, as Woodward (2000) puts it, patterns of counterfactual dependence). Such an evaluation is not just a matter of assessing theoretical beliefs but requires an active engagement with memory, imagination, perception, etc. Because the epistemic status of the new results is at stake, emotions – positive and negative – can be a byproduct of this activity. But they are not useless. Given their general evaluative and motivating properties, emotions – and not just disruptive – can help accommodate new experimental results by motivating, navigating, and glueing together different evaluative moves.

Thus, I conclude that (a) the co-occurrence of affective, cognitive, and conative is not contingent; and (b) emotions beyond surprise and disruptive beauty can be epistemically useful in experimental research.

References

Currie, Adrian. 2018. 'The Argument from Surprise'. Canadian Journal of Philosophy 48 (5): 639–61.

French, Steven, and Alice Murphy. 2021. 'The Value of Surprise in Science', March. `http://philsci-archive.pitt.edu/18839/`.

Ivanova, Milena. 2022. 'What Is a Beautiful Experiment?' Erkenntnis, January.

Morgan, Mary S. 2005. 'Experiments versus Models: New Phenomena, Inference and Surprise'. Journal of Economic Methodology 12 (2): 317–29.

Woodward, Jim. 2000. 'Data, Phenomena, and Reliability'. Philosophy of Science 67: S163–79.

## Applying the Institutional Compass: The case of the Santiago-Guadalajara River Basin

*Alma Ruby Félix Puga[a], Michèle Friend[b], and Iris Ayón[c]*
[a,c]*Universidad de Guadalajara, Mexico;* [b]*Université de Lille, France*
[a]rubysoul27@gmail.com; [b]michele@gwu.edu; [c]irisayon22@gmail.com

Human decisions are made in an ecological context. This makes the decision-making complex. Ecological values are quite different from monetary values, and the time-scale for the natural environment is slower than for the market. Existing decision aids either convert natural resources and services into monetary terms (to have one scalar measure) or they look very complex and do not help a non-expert in making a decision.

We present a new multi-criteria (not just money) decision aide that does not require expertise to read, is multi-valued, multi-time scale, comprehensive, socially inclusive, intuitive, holistic, and properly objective. "Properly objective" means that when objective measurements can be made, they are, but cultural priorities are also tabulated and considered. These are inescapably political, and not objective.

Economic, social, and environmental quantitative data are tabulated, analyzed, and amalgamated into one reading. The reading is represented as an arrow on a circle. The arrow has a length – indicating importance, and a degree, indicating an adjective, or quality. There are three general qualities: harmony, discipline, and excitement; and all of the nuances between these. No quality is good or bad in and of itself. The degree is not scalar. However, as a community, we might have a preference for, say, excitement over harmony. We call the representation an Institutional Compass – henceforth, IC.

The purpose of the IC is to simplify and illustrate the quality direction in which a region is heading. The methodology behind the IC is significantly rooted in philosophical commitments —going from Kuhn's broad considerations regarding the role that emotions play for the achievement and evaluation of scientific knowledge (Cf. Kuhn 1970) to more concrete naturalist perspectives on ecological matters (Cf. Code 2006, Papineau 2016, Price 2019).

This poster presentation consists of two main parts:

The first part is to show the method to construct the IC.

The second part aims at presenting an application of the IC for the ecological study of the Santiago-Guadalajara River Basin (SGB). The SGB is an area located in the central-western part of Mexico which is experiencing an imminent cross-cutting environmental disaster. Because the SGB includes 36 counties, most of which have been seriously contaminated due to the low chemical and biological quality of the river, the actions that are taken to deal with such contamination vary significantly from county to county.

The combination of complexity makes it almost impossible to build a comprehensive analysis of the SGB that is both sensitive to the particularities of each county as well as general enough to provide scientists with an accurate global understanding of the SGB as an economic, social, and ecological phenomenon. Our application of the IC to the SGB shows that this is not only possible but attainable.

References

1. Friend, Michele Indira (forthcoming). The Institutional Compass, Meth- ods Series 18, Springer.

2. Kuhn, Thomas S. (1970). A Response to My Critics, in Imre Lakatos and Alan Musgrave (eds.), Criticism and the Growth of Knowledge. Cambridge University Press.

3. Code, Lorraine (2006). Ecological Thinking: The Politics of Epistemic Location, OUP.

4. Papineau, David (2016). "Naturalism", in Zalta, Edward N. (ed.), The Stanford Encyclopedia of Philosophy (Winter 2016 ed.).

5. Price, Leigh (2019). The possibility of deep naturalism: a philosophy for ecology, Journal of Critical Realism, 18:4, 352-367.

---

## Model Transfer and Universal Patterns - Lessons From the Yule Process

*Sebastiaan Tieleman*
*Utrecht University, Netherlands*
s.b.tieleman@uu.nl

Model transfer refers to the observation that particular model structures are used across multiple distinct scientific domains. This paper puts forward an account to explain the inter-domain transfer of model structures. Central in the account is the role of validation criteria in determining whether a model is considered to be useful by practitioners. Validation criteria are points of reference to which model correctness for a particular purpose is assessed. I argue that validation criteria can be categorized as mathematical, theoretical and phenomenological criteria. Mathematical criteria include analytical tractability and analytical solvability. Theoretical criteria imply an assessment of whether the model is in line with certain theoretical notions. Phenomenological criteria refers to the assessment of whether the model output is in line with empirically observed facts about phenomena.

Model transfer is explained by overlap in validation criteria between scientific domains. If the same validation criteria hold across multiple distinct domains the original model structure may be considered useful across multiple distinct domains. Particular emphasis is placed on overlap between phenomenological criteria. Overlap in phenomenological criteria can be explained through the notion of universal patterns. Universal patterns are abstract structures that can be made to refer to multiple distinct phenomena when coupled with phenomena specific empirical content. The notion of universal patterns points to a degree of generality in the facts we observe across multiple distinct domains.

I present the case study of the Yule Process, which was originally developed in evolutionary biology but was later transferred to economics. In this case study, universal patterns play a crucial role in understanding why the model was transferred. In its original context the Yule process was used to reproduce the distribution of genera size in terms of its number of lower ranking species. In its new context the Yule Process was used to reproduce the distribution of firm size. The distribution that genera and firms have in common, is a universal pattern. I argue that the ability to reproduce this common pattern was what enabled model transfer, rather than overlap in theoretical validation criteria. The paper provides an account of model transfer that stays close to modelling practice and expands existing accounts by introducing the notion of universal patterns.

---

## Mechanistic or Dynamic Explanation? Theory and Practice in the Study of Consciousness with Network Neuroscience

*Siyu Yao*

*Indiana University Bloomington, United States*

siyuyao@iu.edu

Over the last three decades, the study of brain and cognitive activities has witnessed an increasing inclusion of mathematical models and physical analogies in the domain. Two prominent examples are network science and dynamical systems theory. In contrast to new mechanists' promotion of mechanistic explanation in most life sciences, many philosophers have pointed out that this change in the domain has revived the importance of covering-law explanation and explanatory unification in light of the structural-dynamic representation of the target phenomena. Is the suitable pattern of explanation solely determined by the theoretical framework invoked to represent the system? Theoretical reasons are indeed important, as other philosophers have pointed out, but I also suggest a possibility that pragmatic and contingent factors sometimes serve a non-trivial role.

I examine some recent attempts to explain consciousness (or the loss of consciousness) in neuroscience aided by network science and dynamical systems theory. In these studies, consciousness is identified as a complex combination of differentiation and integration in the brain, and this complexity is explained with certain dynamical properties derived from the brain functional network. I first analyze the explanatory patterns used in this whole project into three stages with different explananda and explanantia: (i) the explanation of consciousness, defined in non-neurophysiological terms (e.g., in terms of everyday life, phenomenology, clinics, or psychology), with neural activities that correlate with it; (ii) the explanation of the these neural correlates with causal interaction between neurons; (iii) the explanation of some other relevant properties of consciousness, such as its structural constraint, universality, and specificity.

Next, I point out that mechanistic explanation only plays a minor role in this series of explanatory attempts. Once an abstract model of the brain network has been established, the detailed mechanisms become irrelevant to answering most of the explananda above. Instead, the explanatory power comes more from the derivation of dynamical properties from the abstract models, as well as the universality of such properties across different network models and different instantiations of them. This concords with the lack of explanatory relevance of mechanistic explanation argued by other philosophers.

Beyond this, I suggest that in my case, there are also non-theoretical reasons why the explanatory unification with abstract models is more crucial and applicable than mechanistic explanation. The characterization of consciousness with unspecified abstract properties is the condition for such an approach to explanation. This characterization has a philosophical root from which the neuroscientific inquiry contingently stems. Moreover, holding such properties is methodologically beneficial. Many experiments in neuroscience remain limited in sample size, are susceptible to individual differences, and might be biased by specific experimental settings. With this, meta-analysis becomes a crucial point of synthesis where more reliable and general conclusions can be drawn, and the consilience of multiple lines of evidence serves as a strong case of confirmation. A concept of consciousness that is unspecified and abstract enough to allow for divergent experimental implementations ties well with the application of meta-analysis and consilience.