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Book of Abstracts



Keynotes

*Evidence Generation Across Qualitative and Quantitative Practices.
A reassessment and a Proposal*

Federica Russo

(Freudenthal Institute, Utrecht University)

Keynote

Evidence is ubiquitous in the practices of science, from natural to social science, from medicine to linguistic. In this talk, I begin by reconstructing two main debates in PoS: the ‘formal’ approach and ‘evidential pluralism’. While the first approach does not fix what evidence is (as ‘e’ is a place holder in specific probabilistic relations to confirm a hypothesis ‘h’ based on some evidence ‘e’), the second, is rather specific in fixing the ‘content’ of evidence’ (difference-making and mechanisms provide the fundamental evidential aspects to establish causal claims). In previous work, I have argued that neither approach, while valuable, goes at the core question of what evidence is. We instead need to capture some common features of ‘evidence’, considering that evidence, or pieces of evidence, can be very different things from a p-value to a biological specimen, from an audio recording to measurements conducted in a lab. I proposed to understand evidence as semantic information, a notion borrowed from the philosophy of information, and that cashes out information in a qualitative way, needing interpretation of data in a given modelling context. Following up on this line, I will here explore the prospects of ‘evidence as semantic information’ in the context of a long-standing debate in the social sciences, namely the so-called QUAL QUAN divide. The literature is vast, and the field of Mixed Methods Research has long thematised the need of using both qualitative and quantitative methods, alongside providing protocols for research practices to combine these approaches in various ways, and for different purposes. Building on Newman and Benz’s ‘interactive continuum’ approach (1998), I propose that QUAL and QUAN constitute a spectrum of integrated practices. I describe the process of QUANTification in QUAL practices, and the process of QUALification of QUAN practices, through the idea that QUAL and QUAN are semanticised (in the sense of semantic information), through QUAN and QUAL practices respectively.

Social Science, Social Policy and the Retreat from Ethics

Philipp Kitcher

(Columbia University)

Keynote

In the immediate aftermath of the Second World War, many countries shaped their policies by recognizing ethical obligations. This attitude was particularly evident in their approach to poverty, both at home and abroad. Politics was infused not only by social scientific understanding but by ethics as well.

For the past four decades, we have witnessed the retreat of ethics from politics. Any thoughts of global cooperation to achieve larger goods have been replaced by appeals to the concept of economic efficiency. The result is a dog-eat-dog world in which the primary imperative is to stay ahead of the competition. Economic inequality and poverty have increased in the previously affluent nations and in those that have become more prosperous. Meanwhile, many nations yearn for the development they see elsewhere, but are unable to achieve.

This is not only an ethical disaster. It also has grave consequences for the future of humanity and of the planet. Nearly forty years have passed since a prominent climate scientist informed the US Senate of the threat posed by climate change. Since then, little progress has been made. Many discussions have taken place. Inadequate targets have been set. Action has fallen significantly short of the stated goals.

My lecture will trace the dismal pace of climate action to the withdrawal of ethics from politics. Because of our inability to make firm predictions about the consequences of global heating, the problem of deciding what to do is exacerbated. As I shall explain, in a world where nations dismantle protections for the poor in the name of economic efficiency, it is entirely reasonable for many people to resist programs that would impose further burdens on them. The replacement of economic policies, constructed to achieve ethically valuable goals, by others emphasizing competition undermines the cooperation needed to meet the challenges of an overheated planet.

Contributions

*Symposium: Qualitative Approaches in Philosophy of Social Science and
Medicine – Philosophical Methods*

Nora Hangel, David Lambert, Kathryn Body, Helene Scott-Fordsmand

(University of Hannover, University of Bielefeld, University of Bristol, University of Cambridge)

Session 1

The symposium will showcase four examples of using qualitative, empirical methods in philosophy of social science and medicine. We frame each paper as a ‘method section’ – a written format only rarely encountered in philosophical literature – as a performative suggestion that philosophers may benefit from engaging with this aspect of academic writing. The papers will focus on methodological transparency and validity, exploring how we can use qualitative methods responsibly and effectively in philosophy.

Following the ‘practice turn’ in philosophy of science (Soler et al 2014) engagement between philosophy and the sciences has taken inspiration from established empirical methods in history, sociology, anthropology, and psychology. We suggest the active use of and engagement with qualitative methods can be a powerful tool by providing a systematic, empirical method for philosophers to get a grasp of scientific practices, and in turn, a better understanding of how to produce philosophical work that is responsive to current-day scientific phenomena and questions.

Empirical investigations beyond the use of historical case studies got some tailwind in 2015, when Wagenknecht, Nersessian and Andersen published a collected volume introducing qualitative methods to philosophy of science. Since then, the use of qualitative methods in philosophy is accompanied by a motivation to concentrate on contextualized linguistic representations, on processes, and on adequate and insightful descriptions of the investigated phenomena. Nersessian’s ‘socio-cognitive approach’, for example, showed how contextual and situational features shape reasoning, and epistemic and knowledge-producing practices (e.g., Nersessian et al 2003, MacLeod and Nersessian, 2016). Meanwhile, phenomenological traditions from philosophy of psychiatry spread into other areas of philosophy of science, revealing how embodiment and the human conditions for meaning-making are important to consider, if we want

to understand how and why scientists or citizens do what they do (e.g., Dolezal and Ratcliffe 2023, Berghofer and Wiltzsche 2020). And approaches such as actor-network-theory or phenomenotechnique have made their way into philosophical debates through historical epistemology, sociology of scientific knowledge, and science and technology studies – demonstrating that close attention to particular material and social conditions and interactions can help circumvent some traditional dilemmas of philosophy of science (e.g., Rheinberger 2010).

Despite these developments, philosophy continues to bear the mark of its earlier ways. While methodological integrity is championed in fields like psychology, anthropology, and sociology, many philosophical journals – despite including articles that explicitly draw on qualitative methods – still have little quality assessment of the methods employed, and only rarely deploy the same requirements on methodological openness which we see in parallel fields.

The panel will cover different approaches and topics in philosophy of science and medicine, including the use of fieldwork-based Reflective Thematic Analysis exploring pluralism in clinical psychiatry and psychiatric research; Cognitive Ethnographic studies of scientific reasoning in Judgment and Decision Making (JDM); short-term ethnographic studies of classification practices in orthopaedic surgery; and thematic, phenomenological analysis of qualitative surveys on embodiment and interpersonal relationships from the covid-19 pandemic. Each of these perspectives will show how qualitative methods are motivated and put to use depending on the philosophical research question.

Cognitive ethnography in Judgment and Decision Making (JDM): A Philosophical Analysis of Scientists' Strategies and Perceptions when generating knowledge

Nora Hangel

Contrary to more traditional approaches to philosophy of science this talk investigates questions about the reliability of knowledge from the stance of scientists' descriptions of their practice when experimenting and collaboratively assessing the evidence of results. When generating reliable results and effectively communicating them, researchers want to deliver meaningful contributions, maintain accountability, and attain recognition, for their academic viability. However, contribution, accountability, and the need for recognition form the context of an incentive structure for generating scientific knowledge, particularly in collaborative endeavors. I will present different aspects of sociality as an intricate part of scientific knowledge generation (Longino 2022). The talk will contribute to understanding the role of doing qualitative research, in concrete, cognitive ethnography, in philosophy of science by analysing scientists' reflections about self-corrective practices. I argue that qualitative methods benefit philosophy of science for understanding the social and normative aspects of scientific inquiry in practice.

The study participants for this qualitative expert-interview study are experimental scientists from social psychology, behavioral economics, and others in the field of JDM. I use cognitive

ethnography (participant observation and interviews) to study social epistemological processes, which are key to claims of objectivity, reliability, and empirical success in collaborative research. The study is part of the DFG project: The role of scientific judgment in generating knowledge (2022-25). The naturalistic approach of the empirically informed philosophy of science project follows the understanding of being continuous with science by a) using science as a resource and b) conducting empirical investigation to better understand social epistemological implications of scientists' practices.

The talk will first focus on how the selection of participants is organized systematically and will reflect on reactivity and the relation between the investigating philosopher and the field, in my case JDM.

Second, I will show how philosophical questions about epistemic aspects relevant to belief formation, reliability, evidence, and other processes involved in generating knowledge can be captured in a semi-structured interview guide before talking about the role of pilot studies when doing field studies. Third, I will show how descriptions of scientists' accounts of disagreement, epistemic dependence, and trust contribute a) to a better understanding of different aspects of sociality in cognitive labor and b) how normativity is embedded in scientists' interactions and reflections. Finally, I will compare the new findings to expert interviews conducted between 2010-13 (Hangel & Schickore 2017; Schickore & Hangel 2019) concerning the external validity of qualitative methods.

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Reflexive thematic analysis as a qualitative method for developing case studies in philosophy of science-in-practice

David Lambert

One aim of philosophers of science is to increase understanding of scientific practices through case studies. This paper advocates for the adoption of reflexive thematic analysis (rTA) as a qualitative method for developing case studies in philosophy of science. I will argue that this method addresses the lack of both transparency and explicitness about method in much past and current analytic philosophy of science. The unifying feature of thematic analyses is “the use of processes of coding and theme generation for analysing qualitative data [...] with a theme capturing a pattern of meaning across the dataset” (Braun et al 2022). rTA, in particular, is a recent addition introduced to psychological studies by Braun and Clarke (2006), and later refined into a distinct subtype of thematic analysis (2021, 2022). Notably, rTA offers four features conducive to achieving a better understanding of scientific practices by providing a method for interpreting patterns of meaning:

1) Reflexivity: Central to rTA is a commitment to constantly self-reflect, document, and comment on the recursive process of working with qualitative data, from (i) familiarisation, (ii) coding, (iii) theme generation, (iv) reviewing themes, (v) defining and naming themes, to (vi) producing a case study. This demands that philosophers of science – otherwise accustomed to omitting methodological sections – need to actively consider how they affect the generation of the interpretation of scientific practices.

2) Ontological and epistemological openness and dodging fights: rTA is non-committal regarding its qualitative paradigm. Beyond the quantitative-qualitative divide, qualitative methods have become highly differentiated, all with different ontological and epistemological baggage. rTA is flexible in the required commitments, and so the philosopher avoids getting into the trenches in long and increasingly sophisticated debates of the social and cultural sciences. Instead, rTA prescribes working out the researcher’s, that is, the philosopher’s, individual ontological and epistemological background assumptions.

3) Anti-atheoreticity: Relatedly, rTA is opposed to the possibility or even necessity of atheoreticity. This is in line with the received insight of philosophers of science of the inescapable theory-ladenness of observation, data generation, analysis, and interpretation (Brewer & Lambert 2001). The empirical philosopher of science making use of qualitative data must accept this for their own work, and a method consistent with this is required.

4) Wide applicability: RTA is applicable to a variety of qualitative data. I demonstrate its compatibility with an ethnographic approach to a philosophy of science-in-practice (Boumans & Leonelli 2013). For this, I will draw on my work on ‘treatment resistance research’ based on interviews with clinician-researchers and fieldnotes from my research visit at a psychiatric clinic. My main claim is this: Only by confronting myself both with (local) practice and reflexively analysing the data generated, I identified conceptual difficulties of ‘treatment resistance’ and how

they reverberate in research. While this showcases domain-specific applicability and fruitfulness, the method's detailed features are prima facie generalisable beyond philosophy of psychiatric research, inviting exploration of its usefulness in other domains.

*A phenomenological approach to analysing qualitative surveys:
Covid-19 and bodily experience*

Kathryn Body

There are many different approaches to qualitative analysis, emergent amongst them are philosophically grounded approaches aimed at doing qualitative research that is not merely inspired by philosophical concepts and ideas, but rather seeks to actively conduct empirical research within the discipline. Following this initiative, my PhD project uses a phenomenological framework to analyse qualitative survey responses about the lived experiences of adults in the UK, Japan and Mexico during the Covid-19 pandemic (Froese et al., 2021, James et al., 2022). More specifically, I explore how lived experiences of and attitudes towards the body are discussed by the survey respondents, having particular regard for how, if at all, these reports relate to descriptions of other kinds of bodily or felt adversity such as chronic illness (Carel 2016) or incarceration (Leder 2016).

In this paper, I discuss my experience of conducting qualitative research in philosophy, specifically of working with qualitative surveys. I will start by providing an overview of the two main qualitative approaches I use in my study, namely Reflexive Thematic Analysis or 'Reflexive TA' (Braun and Clarke 2006, 2021) and Phenomenologically Grounded Qualitative Research (PGQR) (Køster and Fernandez 2021). I will then turn my attention to three crucial methodological considerations when doing thematic qualitative research in philosophy – inspired by Braun and Clarke (2019), namely (i) articulating epistemological and methodological assumptions (ii) facilitating 'dialogue' between theoretical concepts/frameworks, the 'object' of study, and the researcher (iii) acknowledging the role of the researcher(s) in the research process. Whilst these considerations are by no means exhaustive, they can inform an approach to qualitative research in philosophy which accommodates flexibility and creativity while remaining consistent and systematic.

I will then discuss the challenges and limitations I have encountered while doing qualitative research in philosophy, in particular, navigating disciplinary boundaries and expectations, choosing a suitable method, keeping sight of contingencies (first-person accounts of lived experience) and relating these to broader, more abstract concepts and ideas in philosophy. Following this, I will explain how I have tried to overcome some of these challenges, drawing on expertise and knowledge from other disciplines like anthropology, whilst maintaining a distinct sort of 'phenomenological sensitivity' and philosophical attentiveness (Køster and Fernandez 2021). To end, I will reflect on how other qualitatively grounded philosophical studies may benefit from phenomenological studies like these that pay particular attention to embodiment and situatedness.

Integrating ethnography and philosophy of medical practice: philosophy and bodies

Helene Scott-Fordsmand

When I set out to use empirical material in philosophical enquiry, I planned to conduct phenomenological interviews with clinical practitioners following already established methods (e.g., Bevan 2014, Høffding & Martiny 2016). I wanted to understand how aversive reactions to patient bodies would affect epistemic attitudes in clinical enquiry. Initial interviews were exciting, but I struggled to link the descriptions with the philosophical question I had. This drove me to a change of method, and I supplemented the interviews with short-term ethnographic fieldwork, inserting myself in the daily practices of orthopaedic surgeons (e.g., Hammersley & Atkinson 2007, Ravn 2021).

In this paper, I reflect on the lessons from this first venture into qualitative methods – on reasons for choosing and changing methods, on benefits and limitations of them, and on their link to my philosophical aspirations. I highlight two features of qualitative work which initially seemed contrary to traditional framing of philosophy as the task of unveiling fundamental and universal truths. Namely (a) the contextual and case-specific kinds of insight provided by qualitative work. And (b) the reliance on me, not as an analytical mind, but as a concrete person and body that had to navigate new spaces, struggled with tendinitis from notetaking, depended on the kindness and time of others in bringing me along, and on chance encounters for relevant access.

I will argue, that explicitly addressing these features and the extent to which they hindered or was able to contribute towards philosophical conclusions, let to a healthy reflection on the epistemic status of philosophy, and eventually to a fruitful dialogue between concrete medical practices and abstract philosophical concepts. Drawing on parallel discussions from integrated history and philosophy of science, I posit that philosophy need not be about universal truths but can be about advancing our understanding through useful, abstract tools for meaning-making (Chang 2011, Hannon & Nguyen 2022). And that in this conception of philosophy, the idiographic nature of qualitative methods may not be a hindrance but a virtue. Secondly, drawing on phenomenological literature, feminist philosophy of science, and science and technology studies (Carel 2008, Haraway 1988, Latour 2004), I argue that ignoring the contingencies involved in being individual people (rather than brains in vats) does not make problems of bias and hegemony disappear in philosophy, and that in fact, actively embracing our particularity may lead to a healthier grasp on the epistemic challenges of philosophy. In the end, I put forward the position that the integration between ethnography and philosophy is not only possible, but can serve as an important heuristic moment, reminding philosophers that they too are and have bodies.

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Altruism and the Simple Argument for Markets

Kevin Leportier

(Université Gustave Eiffel)

Session 2

Adam Smith, according to Friedman and Friedman (1990), analysed the way in which a market system could combine the freedom of individuals to pursue their own objectives with the extensive cooperation and collaboration needed in the economic field to produce our food, our clothing, our housing'. He showed that markets make individuals free to choose while at the same time delivering the goods. Both aspects are valuable, according to Friedman (1962/2002), since 'freedom in economic arrangement is itself a component of freedom broadly understood, (...) economic freedom is an end in itself'. However, contemporary economics has focused almost exclusively on the second aspect, putting forward the efficiency of a market system and its merit

for improving welfare. The fact that the Friedmans chose the title 'Free to Choose' for their popular book, relegating welfare in the background, suggests that this way of promoting markets is more in tune with common representations, or perhaps more likely to win support. An appeal to freedom of choice provides a simple and neat argument for expanding the sphere of the market. As Hausman et al. (2017, 94) sums it up, 'markets permit the simple freedom of being able to choose among alternative as one pleases (provided that one has the means, of course)'.

The reference to this 'simple' freedom is the basis of what I will call the 'simple argument for markets'. This argument can be found virtually everywhere—everywhere else than in standard economic analysis. Besides its role in philosophical and intellectual debates, it is also sometimes used in politics to convince people of the benefits of privatization, or at least to argue against state monopolies. Its a priori nature makes it powerful and appealing to those, like the Friedmans or Sen, who value freedom for itself. But it is seldom the subject of analytic scrutiny.

I will consider in this paper the value of this argument in justifying the creation of new markets. In substance, as Arrow (1972) formulated it, the possibility of performing new market transactions only adds to individuals' already existing alternatives—understood as mutually exclusive possibilities of actions—, leaving everything else unchanged. Indeed, people who are not interested in these transactions may abstain. Individuals' freedom is thus enhanced, as everyone has a bigger set of alternatives than before. Because of his simplicity, the argument does not appear to rely on any empirical premise or substantive value commitment concerning the definition and measurement of the set of alternatives, or 'opportunity sets', as they are called in the literature devoted to the measurement of freedom. Besides, contrary to what is sometimes claimed (Herzog 2021), this 'simple' argument does not presuppose any belief in pre-established libertarian rights, as Nozick (1974) has described them—although it obviously presupposes the existence of private property, which is necessary to make markets work.

I will show that the argument falls short of this promise by discussing a particular (and often ignored) aspect of the famous debate between Arrow and Titmuss about the merits of a market for blood transfusion, compared to a voluntary donor system. Titmuss (1970) pointed out that the institution of the market denies people 'the freedom to enter into gift relationships', to the disbelief of Arrow and many after him. A few philosophers (Singer 1973, Anderson 1990, Radin 2001, Archard 2002) have tried to make sense of this perplexing argument by arguing that the opening of a market, altering not the mere possibility but the meaning of a blood donation, may deprive individuals of the opportunity to give 'the gift of life' to the recipient of the transfusion.

I will take a different route and stick to the standard perspective and modelling practice of economists, to show that the satisfaction of some particular kind of preferences—those of 'impure altruists' (Andreoni 1990)—is denied by the market for blood. This different route leads to the same destination, however: the opportunity to give 'the gift of life' which was previously accessible to individuals disappeared. As this contradicts the premise according to which the opening of a new market leaves everything unchanged in terms of freedom, the simple argument no longer holds. This discussion illustrates and extends a claim made by Robert Sugden, which is that the definition of opportunity sets always appeals to some particular set of preferences or

value commitments—which means that it cannot be completely value-free (Sugden 2003). I will show that the simple argument only works if the preferences of impure altruists are disregarded, which means that contrary to what is often denied, markets may well impose a 'preordained pattern of value to which individuals must conform' (as Satz 2010 puts it), even when these individuals refuse to transact.

In the first section, I provide a detailed analysis of the simple argument for markets, and its connection to the idea that markets are a value-free space where anyone can express and develop their particular individuality, whatever may be their preferences. The second section presents Sugden's criticism of the claim that opportunity sets can be defined and measured without favouring implicitly some preferences or values. A simple principle for defining opportunities in real-world situations, which accords with the modelling practice of economists, is defined: according to the principle of relevance of value-differences, if some individual prefers some action to another one, then those two actions should be modelled as different opportunities accessible to him. The third section introduces the debate between Arrow and Titmuss about the 'right to give' and its reformulation by subsequent philosophers. The fourth section uses the principle defined earlier to show that the opening of a market for blood deprives individuals of a significant opportunity since the satisfaction of the preference of impure altruists is denied by the opening of a market for blood. I conclude by suggesting that markets are only amenable to what I call 'market-based' preferences, which are defined over the output of the market transactions—in terms of individual results and satisfaction—, neglecting preferences which also value contextual elements.

The Capability Approach in Tax Justice: A Reconciliation of Efficiency and Folk Intuitions

Tomasz Kwarciński & Marcin Gorazda

(Krakow University of Economics & Jagiellonian University)

Session 2

This paper explores the conceptual tension between utilitarian tax justice and the folk justice perceptions of taxation among laypeople, proposing a synthesized approach that respects both efficiency and moral intuitions. Utilitarianism, rooted in consequentialism and welfarism, traditionally frames tax justice, emphasizing efficiency and equality through the principle of diminishing marginal utility (Leviner, 2012). This approach advocates for income redistribution, arguing that transferring resources from the rich to the poor maximizes overall utility by addressing

the disparity in the marginal utility of income (Mankiw & Weinzierl, 2010). In the classical approach, if earnings are inelastic and individual utility functions are concave and homogeneous among individuals, there will be complete income redistribution. This implies that 100% of income would be taxed and then equally redistributed among citizens (Edgeworth, 1897).

On the one hand, utilitarianism emphasizes the importance of consequential reasoning in collecting and distributing taxes. It justifies the ability-to-pay principle of tax justice, which states that the tax burden should be allocated to those able to pay; wealthier individuals should pay higher taxes because they can afford to, and they will suffer less when their income is diminished. On the other hand, utilitarianism presents serious theoretical and practical problems: the utilitarian outcome is very sensitive to the specification of individual utility functions – if only slightly concave, then theoretically, all income should be redistributed. Moreover, scepticism towards the interpersonal comparability of utility prevents utilitarians from addressing heterogeneity in individual utility functions (differences in marginal utility among individuals). However, the most significant challenge is pragmatic; it is unlikely that people will be willing to agree to tax and redistribute their entire income (Saez & Stantcheva, 2016). Furthermore, the utilitarian justification of tax policy often contrasts with the deontological perspectives of folk justice, which emphasize fair procedures, rights, and deservingness alongside the consequences of taxation (Sheffrin, 2013). There is plenty of evidence that people are willing to accept unfavourable outcomes if they consider them the result of a fair procedure. They also pay attention to rights, assessing how far tax policy may undermine them. Moreover, the concept of deservingness plays a crucial role when people evaluate hard workers and frugal individuals as more deserving of tax exemptions than those perceived as leisure lovers and freeloaders (Saez & Stantcheva, 2016).

The divergence between utilitarianism's emphasis on consequences and deontological folk intuitions is crucial because the tax system's effectiveness relies significantly on public compliance, which is influenced by moral intuitions. On the one hand, utilitarianism, which focuses on outcomes, is seen as essential for tax efficiency. On the other, adherence to deontological principles is vital for securing compliance. These viewpoints seem at odds, highlighting the necessity for a theoretical framework to harmonize the efficiency associated with utilitarian and consequentialist approaches with the moral foundations of folk deontological perspectives on taxation. Addressing this divergence, our research investigates the potential of Amartya Sen's capability approach as a reconciliatory framework.

Sen's capability approach, compatible with consequentialism (Qizilbash, 2022), provides a flexible foundation that incorporates deontological elements by focusing on functionings (what people do and are) and capability sets (the abilities for doing and being), emphasizing real freedom (Sen, 1985; 2009). This approach presents an alternative to both utilitarianism and deontologism (often associated with libertarianism). Utilitarianism is criticized not for its focus on consequences but for evaluating them solely through welfare lenses. Deontologism, on the other hand, is critiqued for its lack of flexibility regarding rights protection. Both approaches have too narrow an information base for evaluation to serve as an adequate theory of social justice.

However, Sen (2000) values consequential evaluation and reinterprets the notion of outcome as “comprehensive” rather than “culmination” (Qizilbash, 2022). He suggests reinterpreting rights as goals rather than constraints on human actions (Sen, 1985) and highlights the importance of the deliberation process, which is an essential part of the intuition that procedural justice matters. Capabilities, rather than welfare (utility), form the basis of outcome evaluation, offering insights into concepts of deservingness and equality. The main features of the capability approach include plurality, openness, and a focus not on ideality but on alleviating poverty and deprivation in real life. By embracing capabilities, openness, plurality, and inclusiveness, the capability approach aligns with both the utilitarian aspects of tax justice and the deontological intuitions of taxpayers.

Our argument unfolds as follows: Traditional utilitarian tax justice, rooted in consequentialism and welfarism, focuses on efficiency and equality through the principle of diminishing marginal utility, advocating for the redistribution of income from the rich to the poor to maximize overall utility. However, utilitarian frameworks often stand in contrast to deontological perspectives of folk justice, which highlight fair procedures, rights, and deservingness, factors that influence public compliance with tax systems. We have identified a conceptual tension between utilitarian tax justice and folk justice perceptions, which impacts the effectiveness of tax systems. We then argue that Amartya Sen’s capability approach, while compatible with consequentialism, can incorporate deontological elements by emphasizing capabilities and real freedom. Consequently, the capability approach aligns with both the utilitarian aspects of tax justice and the deontological intuitions of taxpayers by embracing capabilities, plurality, openness, and inclusiveness. We conclude that this integrated approach could lead to a more effective and ethically robust tax system. By reconciling the efficiency-driven aspects of traditional utilitarian tax justice with the moral and procedural concerns of folk justice, the capability approach presents a promising path for developing a tax justice theory that is both normatively sound and practically effective in securing taxpayer compliance.

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Cost-Benefit Analysis, Incommensurability, and Imprecise Utilities

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Session 2

Cost-benefit analysis (CBA) is one of the most important tools to assess alternative options under scarcity of resources. In the last decades, CBA has enjoyed a rising success in public decision-making, with applications ranging over most aspects of public policy all over the world. In light of its importance, CBA has been the object of intense philosophical scrutiny, investigating its methodological and ethical assumptions (e.g. Hansson 2007, Adler & Posner 2006).

Among these, particular attention has been devoted to the assumption that all costs and benefits of an intervention are commensurable, i.e. that they can be meaningfully compared under a common metric, which is typically expressed in monetary terms (Hirsch Hadorn 2022). Without commensurability, different costs and benefits could not be traded off and aggregated in the standard CBA results (e.g. net present value, benefit-cost ratio, internal rate of return). However, some critics contend that many policy-relevant dimensions are incommensurable, perhaps because they are intrinsically invaluable or non-substitutable. Nonetheless, in the face of incommensurable goods and services we still need to make decisions involving important trade-offs. In this paper, we aim to provide a formal setting that can help CBA include some degree of consideration of incommensurability, which could make it a valuable instrument to render these trade-offs explicit and thus more open to scrutiny.

First, we note that the notion of incommensurability can be interpreted in two ways. Under the mainstream interpretation, incommensurability denotes a quality of the external reality such that the things under consideration cannot, even in principle, be traded off. Under a different interpretation, however, incommensurability is an epistemic quality of the agent that refers to their ignorance of the precise value of the two aspects to be compared, and can therefore be

reduced: if only they had enough information of the relevant kind, they would be able to meaningfully compare them.

Then, we present an approach that implements imprecise costs and benefits into CBA. Our approach adopts the machinery of imprecise Bayesianism in formal epistemology (Bradley 2017). Standard Bayesianism represents the agent's cognitive and conative states by a probability function and a utility function. In the imprecise setting, credences and desirabilities are represented by sets of probability and utility functions respectively (Weirich 2021).

We suggest that, in the context of CBA, incommensurability can be captured by making costs and benefits imprecise with the use of sets of real-valued utility functions (Weirich 2021). Incommensurability between costs and benefits can be formally captured in this setting. To this aim, we draw on an analogy with incomplete preferences: if two quantities are incommensurable, then any preference relation over them will be incomplete. Assuming that the agent's probability function is unique, if an agent has preferences $\alpha \preceq \beta$ and $\alpha \preceq \gamma$, but neither $\beta \preceq \gamma$ nor $\gamma \preceq \beta$, these can be represented with a set of complete utility functions such that according to some, the utility of β is greater than the utility of γ , and according to others the utility of γ is greater than the utility of β . The more opinionated the agent, the smaller the set of utility functions that are compatible with their preferences. Thus, a CBA that aimed to include considerations of incommensurability could employ imprecise costs and benefits.

We then turn to the interpretation of imprecise costs and benefits. Similarly to incommensurability, these can be interpreted either epistemically or non-epistemically. Under the epistemic interpretation, imprecise costs and benefits track our uncertainty over the actual and definite value of some aspect of the options under evaluation. Under a non-epistemic interpretation, these costs and benefits track instead an intrinsic feature of some aspects that do not have a matter-of-fact correct value that we need to discover. Consequently, in the first case imprecise costs and benefits can be used to capture incommensurability as ignorance of actual values, while in the second case they can represent it as an intrinsic feature of the world.

One can assume that all imprecise costs and benefits are epistemic; in that case, they are always the sign of incommensurability. Indeed, if they are always due to our ignorance over the actual costs and benefits, then they all represent epistemic incommensurability. Alternatively, one can assume that no uncertainty is possible over values, or that if there is it should be captured with other (e.g. probabilistic) instruments. In that case, imprecise costs and benefits could only be interpreted non-epistemically, and thus always be the sign of non-epistemic incommensurability. However, the two interpretations are not necessary in opposition: one could also assume that both are possible, and that the same instrument can be used to track both epistemic and non-epistemic incommensurability. Which one it tracks will depend on use.

We conclude that the use of imprecise costs and benefits in CBA allows for the application in that context of some interesting decision-making tools that have been developed in the literature on imprecise Bayesianism. These may increase the degree to which CBA can take into account issues of incommensurability, thus improving its applicability to complex decisions.

Symposium: Pseudoscience in Work Life

Inkeri Koskinen, Marko Forsell, Ilmari Hirvonen & Päivi Seppälä

(University of Helsinki)

Session 3

Theories and tools of dubious epistemic merit are frequently used in contemporary work life. Management and HR consultants appeal to theories that are relatively often epistemically questionable and, at times, clearly pseudoscientific. Epistemically problematic personality tests are used in recruitment processes. And the same goes on in the vast self-help literature dealing with work life. The philosophical literature on demarcation and pseudoscience has largely overlooked the forms of pseudoscience that are common in the workplace. In this panel, we address this oversight.

From a purely epistemic viewpoint, the demarcation between science and pseudoscience might not be as important a question as was thought in the first half of the 20th century. However, the discussion on demarcation serves an essential societal need, and philosophers of science have a societal duty to continue addressing it. Therefore, the philosophical literature on the topic should engage with all societally significant forms of pseudoscience and all areas of society where demarcation is needed.

The current focus in the philosophical discussion on demarcation is too narrow: philosophers involved in it largely (and in many ways understandably) concentrate on socially worrisome phenomena such as climate change denial and vaccine scepticism. The forms of pseudoscience that shape contemporary work life are more socially conservative, and do not typically question the value of science and academic research. However, overlooking these forms of pseudoscience leads to problems. Firstly, the current philosophical discussion on demarcation does not provide the general public with tools for the critical examination of this neglected subject area. Secondly, the narrow focus sometimes directs the philosophical discussion to overly restricted views on demarcation.

In this panel, we highlight two themes that should receive more attention in the philosophical discussion on demarcation.

Firstly, we argue that pseudotechnologies are more common than generally assumed in the demarcation literature. We introduce the concept of social pseudotechnology and examine

examples of pseudotechnologies used in contemporary work life: facial analysis AI systems and social technologies commonly employed in self-help literature and HR consultations.

Secondly, we ask how research communities – for instance, in management studies and psychology – should respond to workplace applications that are epistemically unreliable but are claimed to be based on research or to otherwise represent the best available knowledge on subjects these fields study. Such applications are often commercially lucrative. We argue that research communities in these fields face two risks: on the one hand, the risk of having too little impact on the applications, and, on the other, having too close commercial ties to the developers of such applications.

Commercialisation, Opacity, and Demarcation: The Use of Personality Tests in Recruitment

Inkeri Koskinen

Epistemic opacity brought on by commercial interests can complicate the demarcation between science and pseudoscience. This is noteworthy both for the discussions on demarcation and for the discussions on the commercialisation of science. In this paper, we integrate these discussions when examining the use of personality tests in job recruitment.

In contemporary philosophical discussions about pseudoscience and demarcation, the search for a single demarcation criterion capable of defining scientificity is no longer pursued. However, the practical need for criteria that could be used to figure out whether something is pseudoscience is as dire as ever. Philosophers have suggested different ways in which this need could be met. Some have proposed lists of demarcation indicators (Pigliucci 2013; Mahner 2013). Another influential suggestion is to focus on a descriptive definition of pseudoscience (Hansson 2009; 2013). These suggestions have one thing in common: they suppose that when suspicions of pseudoscientificity arise, the questionable theory or doctrine can be evaluated by comparing it to the suggested criteria: either to a list of the typical features of science or to the definition of pseudoscience complemented with an assessment of reliability. However, this is not always possible. Many questionable claims of scientificity or epistemic reliability presented today cannot be tested in this way, as the bases for these claims are considered trade secrets.

Many philosophers of science have noted that commercialisation weakens the self-correcting mechanisms of science, as a significant portion of research does not undergo peer review or become openly available (e.g., Carrier 2008; Radder 2010). However, commercialisation does not merely complicate epistemic assessment. It also paves the way for the rampant use of unfounded claims of scientificity. When the right to confidentiality due to commercial interests is considered stronger than the obligation to justify loose claims of scientificity or epistemic reliability, commercial environments become a fertile ground for pseudoscience that goes easily undetected.

To illustrate the need to better connect the philosophical discussion on pseudoscience and demarcation with the discussion on the commercialisation of science, we examine the use of personality tests in job recruitment. Typically, recruitment companies sell the testing services; this is a big business worldwide (Goldberg 2023). Some tests are based on psychological research, but many are developed by the recruitment companies. Although they, too, are often claimed to be based on research, they have not been validated in any publicly verifiable way. The tests are routinely treated as trade secrets. Furthermore, some of the genuinely validated psychological tests are also protected by test confidentiality.

The ability of personality tests – even those based on psychological research – to predict job performance has been questioned (Barrick, Mount & Judge 2001; Murphy & Dziewieczynski 2005; Morgenson et al. 2007). Moreover, there is ample evidence that some of the used tests are highly unreliable (Murphy & Dziewieczynski 2005; Paul 2010; Vermeren 2019). However, due to the secrecy, when employers decide to use a personality test provided by a recruitment company, they are, in practice, unable to assess the recruitment company's claims of the test's reliability and predictive power. The job applicant's chances of assessing the test are even weaker. It is typical that although the tester may discuss the test results with the applicant, the applicant does not automatically get to see the report of their results that goes to the employer. In addition to test confidentiality and trade secrets, this third veil of opacity further diminishes their opportunities for evaluation. We argue that the ways of identifying pseudoscience that philosophers have developed are unusable in such situations.

How Scientific is Management Studies?

Marko Forsell

The term “management science” is commonly used in the names of journals, conferences, and academic departments. Nevertheless, it remains unclear what relation management studies has, or should have, to science. In what sense can management studies be scientific? Can and should management practices be based on science? If this is the case, what is required for them to do so? This article provides tentative answers to these questions. To illustrate our claims, we will discuss three management practices with widely different levels of scientific justification, extending from clearly scientific to downright pseudoscientific.

The roots of management studies can be traced to Frederick Taylor's late 19th-century work on workforce productivity, further developed through Elton Mayo's human-centric approach in the 1930s. Both pioneers emphasized empirical methodologies, laying a foundation for the field's expansion into diverse areas, and culminating in the prominence of MBA degrees in academia.

However, management studies has been criticized for its lack of theoretical rigor, methodological concerns, insufficient empirical validation, and the issues of reproducibility and fragmentation in research (e.g. Cronin et al 2021; Antonakis 2017; Pfeffer 1993). Several critics have also pointed out that management studies has remarkably little influence on management practices (Rynes et al 2007).

In this article, we take a stance in an ongoing critical debate within the field by explicating three issues already discussed in management studies. Firstly, the scientific nature of the field varies with perspective: from an outsider's view, it seeks to understand business operations; from an insider's, it aims to provide actionable guidance. Secondly, the potential for management practices to be science-based is acknowledged. Lastly, we argue that transitioning to a science-based approach requires practice-oriented research, focused on achieving reliable outcomes for specified management goals.

To understand management studies' relation to science, comparing management studies to such disciplines as political and medical science is instructive. Political science takes an outsider's perspective, whereas medical science aims to guide insiders like physicians. Management studies are marked by a notable science-practice gap, indicating a deficiency in practice-oriented research. This leads to a problematic dominance of the outsider's perspective in a field that inherently deals with practical activities.

Researching human activity from an outsider's perspective enhances our understanding of that activity, while an insider's perspective seeks action knowledge (Hansson 2015; 2019). This can be illustrated with deterministic action recipes ("If you perform X, then Y will occur") or probabilistic ones ("If you perform X, then Y becomes more likely"). In management science, X could be a management practice and Y a desired workplace outcome. For such an action to be effective, the action (X) must increase the likelihood of achieving Y, and X's implementation should not lead to negative consequences that outweigh Y's benefits. Evaluating whether an action leads to its intended goal without significant negative effects is best done through directly action-guided experiments. These involve performing the action and observing outcomes compared to similar situations where the action is not performed. Much older than modern science, this approach has been a staple in farming and various crafts. Since the mid-20th century, such experiments have been formalized in medical science as clinical trials. (Hansson 2015.)

In stark contrast, management science still largely operates as medical science did a hundred years ago. While some management trials (action-guiding experiments) exist, they are rare. Most academic recommendations in management rely on theoretical considerations and anecdotes.

In order to shift towards a more empirically grounded methodology, management science needs to focus more on practice-oriented research. Ultimately, management practice recommendations should be based on management trials or, if unavailable, on robust inferences from relevant disciplines.

Pseudoscience and the Claim of Practical Utility: The Case of Thomas Erikson and the DISC Model

Ilmari Hirvonen

Pseudoscience in business consultation and self-help literature has yet to be studied adequately. Traditionally, the analyses of pseudoscience have mainly focused on clear cases, like climate change scepticism, Holocaust denial, astrology, intelligent design, and homoeopathy. We believe this has skewed influential definitions of pseudoscience. It has also led to underestimating how justification for various epistemically flawed theories is sought through their practical solutions.

Possessing some deceptive quality is commonly considered a necessary condition for pseudoscience (Hirvonen & Karisto 2022, 715; Kitcher 1982, 4–5). Traditionally, it has been suggested that pseudoscientists claim their theories are scientific or represent the best available knowledge (Baigrie 1988, 438; Mahner 2007, 547–548; Hansson 2013, 70–71). However, it is unusual for pseudoscientists to make such strong claims in business consultation and self-help literature. In these fields, pseudoscientists commonly appeal to the practical utility of their theories. The theories are claimed to constitute knowledge because the psychological or social pseudotechnologies derived from them work in practice.

Hansson (2020) argues that pseudotechnologies are rare because their failure to function is usually evident. He is mainly correct when it comes to technologies related to natural pseudosciences. This might also explain why various loose claims of practical utility have yet to be recognized as a significant form of fallacious reasoning invoked by pseudoscientists. However, in what we call social pseudotechnologies, the link between (un)functionality and its underlying pseudotheory is more intricate.

There is a strong tendency to think that the correctness of a theory is, by default, the best explanation for any perceived practical success of its applications. Alternative explanations for the success of the applications may therefore not even be considered. So, the mere impression of functionality might eliminate the motivation to explore why and how psychological and social technologies based on pseudotheories function. Several alternative mechanisms can explain their putative success, as the problems they are supposed to solve are often vaguely defined. Moreover, the impression of functionality might be illusory, and examining the actual mechanisms behind the solutions often uncovers moral problems and undesirable social consequences.

A recent example is the DISC model popularized by Thomas Erikson (2019). Based on their personalities, the model divides people into four colours—blue, green, yellow, and red. Erikson’s model has significantly influenced consulting circles, especially human resource management. Despite its widespread impact, the DISC model is not supported by sufficient scientific evidence, and Erikson’s arguments for the model are problematic, even from the point of view of everyday knowledge formation (Vermeren 2019).

Erikson is guilty of both forms of epistemic deception mentioned earlier. He has admitted, after some criticism, that the DISC model “is no exact science” even though it “work[s] for people” (Keynote Speaker 2024). However, later, he claimed that it is a “proven” theory (Erikson 2024). Such argumentation is typical in pseudoscientific business consulting. When pressed, consultants may acknowledge that their claims lack scientific basis, but this does not change their behaviour in the long run. Like Erikson, they return to making claims about research-basedness and scientificity after a while.

In this paper, we argue that even with loose claims of practical utility, the DISC model still crosses the threshold of pseudoscience. If we are right, demarcation endeavours do not require that pseudosciences be claimed to be scientific or to constitute the best available knowledge. Our paper also examines the perceived functionality of the DISC model and identifies some possible explanations for its success. Based on our findings, we draw general lessons concerning the proper definition of pseudoscience.

Are Facial Analysis AI Systems Pseudotechnologies?

Päivi Seppälä

Facial analysis AI applications have recently come under significant criticism. For example, they have been accused of being pseudoscientific (Ajunwa 2021; Roemmich et al. 2023; Scheurman et al. 2021; Sloane et al. 2022; Stark & Hutson 2021). In this paper, we assess the accuracy of such accusations. In some cases, facial analysis AI does indeed count as pseudoscientific. However, we argue that it should also be examined as a form of pseudotechnology. For this purpose, we will specify the concept of pseudotechnology and its relationship with pseudoscience. In addition, we will highlight some flaws and strengths of the previous definitions of pseudotechnology (e.g., Bunge 1976; Mahner 2007; Hansson 2020). We contend that pseudotechnology must meet two necessary conditions. For something to count as pseudotechnology, it must first be an alleged technology claimed to work in practice, and second, this claim must be unjustified. However, these are only necessary and not yet sufficient conditions. Additional domain-specific and gradual criteria are needed to classify something as pseudotechnology.

Facial analysis AI employs automated facial and body analysis technologies to classify individuals based on various attributes, including emotions, personality, intelligence, gender, and race. It finds applications in diverse fields such as hiring, policing, and education. It has even been used to predict criminal tendencies, sexual orientation, and political affiliations (Roemmich et al. 2023). Facial analysis AI has been criticised as pseudoscientific since it parallels outdated practices and theories like those applied physiognomy and phrenology (Ajunwa 2021). Critics argue that it lacks a solid scientific foundation and may be influenced more by social and behavioural factors than biological ones (Scheurman et al. 2021). As critics have warned, applying facial analysis AI for such purposes may have severe societal consequences (Crawford, 2021).

In this paper, we examine four applications of facial analysis AI and determine whether they qualify as pseudoscientific or pseudotechnologies. The applications are (1) personality assessment in hiring, (2) identifying emotional states from micro-expressions, (3) predicting criminal, sexual, or political behaviour, and (4) determining race or sex and gender (Roemmich et al. 2023; Wu & Zhang 2016; Wang & Kosinski 2018; Xi et al. 2020; Kosinski 2021; Mallon 2006; Schiffer 2020). We will evaluate the pseudoscientific or pseudotechnological status of these facial analysis AI applications within four domains – functionality, theory, research practices, and communication – and explicate some criteria for demarcation within these domains.

Hansson (2020) has noticed that while pseudosciences are numerous, pseudotechnologies are considerably rarer. He suggests this is because pseudotechnologies typically reveal themselves when used, unlike pseudosciences, which are more challenging to expose. We argue that Hansson is half right. Although pseudotechnologies are discussed considerably less than pseudosciences, several nevertheless exist. Hansson is right in that those pseudotechnologies that fall under the engineering and natural sciences typically reveal themselves by not functioning in the intended manner. However, this is not the case with pseudotechnologies applied to humans, for instance, psychological and social pseudotechnologies. Often in such cases of pseudotechnology, it is not entirely clear what the technology is precisely intended to do, whether this goal has been achieved, or why people believe that it has been achieved. Facial analysis AI, when crossing into pseudotechnological territory, exemplifies this phenomenon.

We also point out that while there are clear instances of bad science, pseudoscience, and pseudotechnology within facial analysis AI, some researchers and developers exercise sufficient caution in making their claims. Therefore, facial analysis AI technologies should not automatically be labelled pseudotechnological or pseudoscientific. Such labelling requires – at least a cursory – evaluation of functionality, theories, practices, and communication against the demarcation criteria presented in this article.

On Constitutive Explanation in Interpretive Social Science

Joonatan Nõgisto

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Session 4

Within the literature on social scientific explanation, it is commonly asserted that all explanation is causal explanation (e.g. Elster, 2007, p. 1; King et al., 1994, p. 75; Little, 1996, pp. 40–41). In recent years, a growing social scientific literature on constitutive explanation, stretching back to Alexander Wendt’s influential work (1998, 1999), has sought to challenge the causalist view (e.g. McCourt, 2016; Norman, 2021; Selg, 2020). Putative examples include constitutive explanations of “pecking orders” within the system of international diplomacy (Pouliot, 2016), the “wickedness” of the COVID-19 crisis as a problem of governance (Selg et al., 2022), and genocidal

participation during the Rwandan genocide (Fujii, 2009, Chapter 6). Touted as “a hallmark of contemporary interpretivism” (Lawler & Waldner, 2022, p. 238), some have suggested that recognition of the role of constitutive inquiry can serve to build bridges across the longstanding divides between interpretivist and positivist strands of social science (Lawler & Waldner, 2022; Norman, 2021). However, a review of the existing literature suggests that it is not entirely clear what constitutive explanations within this tradition are taken to be and how they relate to causal explanations. Furthermore, there is a lack of attention to the workings of actual constitutive explanations. I address these shortcomings to clarify the nature of constitutive explanation within the interpretivist social sciences, making three claims:

(a) Existing accounts of constitutive explanation have been too fine-grained in distinguishing the relata of constitutive explanations.

Building on Robert Cummins account of explanation by functional analysis (1983), Wendt (1998, p. 105) characterizes constitutive explanations as explanations of the properties of entities through the underlying structures in virtue of which they exist. In more recent years, Petri Ylikoski’s (2012, 2013) similar but more refined account of constitutive explanation has been especially influential in the interpretivist literature. Constitutive explanations are taken to explain the causal capacities of systems through the properties and organization of their parts. This means that constitutive explanations are dispositional explanations—they provide modal information about what would or could happen under the right conditions (Ylikoski, 2013, p. 279). If causal capacities are understood as latent dispositions, constitutive explanations are distinctively “static”, as unlike causal explanations, they “do not rely on references to processes, triggers, or mechanisms” (Norman, 2021, p. 939). However, this conflicts with occasional claims in the literature that constitutive explanations may involve processual entities, and some have even emphasized the inherently processual nature of constitutive explanation (Pouliot, 2016, pp. 14–16; Selg, 2020, p. 47). I suggest that this discrepancy has parallels in the literature on constitutive-mechanistic phenomena in the new mechanistic literature and that constitutive explanations need not target only causal capacities, but also their manifestations (Kaiser & Krickel, 2017; Krickel, 2018, Chapter 6.1). Such explanations involve constitutive relations between temporally extended yet synchronously linked entities across a time interval. I consider Lee Ann Fujii’s (2009) explanation of genocidal participation during the Rwandan genocide as an explanation where the explanans and explanandum phenomena are causal processes, yet the relevant explanation-backing dependence relation between them is constitutive. Such cases suggest that either “constitution” in the social sciences refers to different “vertical” metaphysical dependence relations or that constitutive explanations are backed by a more general relation (such as grounding) which is less restricted in the kinds of entities fit to serve as relata. The example poses a further interesting contrast with Ylikoski’s (2013, sec. 4) example of hybrid causal-constitutive “developmental” explanations, where the explanans and explanandum phenomena are causal capacities, yet the dependence is causal. Rather than further proliferating explanatory types, I suggest that what is distinctive about constitutive explanation is the constitution relation itself, rather than the explanantia and explananda phenomena. Although the constitution relation is

likely restricted in terms of what kinds of entities are fit to serve as relata, examples of constitutive explanation suggest there is more diversity than previously thought.

(b) “Mutual constitution” should be rejected, and constitutive explanations making use of such notion should be reinterpreted through analogy to common cause explanations.

Despite ubiquitous use of “constitution” in a social scientific context, it is not clear whether there is consensus on the nature of the constitution relation itself. While it is widely acknowledged that constitution is a synchronic dependence relation between not wholly distinct entities, important issues remain. Within the interpretivist literature, there is frequent reference to “mutually constitutive” phenomena (e.g. Selg et al., 2022, p. 20; Wendt, 1998, p. 103), suggesting that constitution is not asymmetric (contra Ylikoski, 2013). This would undercut the purported explanatory role of constitution—it would mean that constitution is unable to provide a direction of explanation. I propose that mutual constitution be rejected, as putative examples in the literature either mistake causal feedback loops for constitutive relations or are unproblematically interpretable as cases of common constitutive ground, with a structure analogous to that of common cause (see Ismael & Schaffer, 2020). This further supports the claim that constitutive explanations may run in both micro-to-macro and macro-to-micro directions (Van Bouwel, 2019).

(c) Interpretivist researchers should focus on the similarities to causal explanation to clarify norms for constitutive inquiry.

Constitutive explanation in the social sciences has been associated with the interpretivist tradition, which emphasizes the constitutive role of subject-centered meanings and beliefs in the social world. The impetus for championing constitutive inquiry lay in the contention that interpretive research can be properly explanatory rather than “merely” descriptive. The existence of constitutive explanations does challenge the view that all social scientific explanation is only causal. However, actual constitutive explanations are typically inseparably intertwined with causal claims. Interpretivists cannot treat constitutive explanation as entirely separate. The recent turn to clarifying causal explanation in interpretivist research is a welcome one (e.g. Norman, 2021). Development of causal and constitutive inquiry in interpretivist social science should go hand-in-hand. Interpretivist defenders of constitutive explanation have so far focused on the differences with causal explanation (e.g. Selg, 2020). More attention should be paid to relevant similarities to clarify norms for constitutive inquiry, not only in interpretive research, but in the social sciences broadly.

Social mechanisms and growth of knowledge

Petri Ylikoski

(University of Helsinki)

The core idea of analytical sociology is developing middle-range theories about social mechanisms (Hedström & Udéhn 2009). Associated with it is an idea about the growth of theoretical knowledge that has never been fully articulated. According to this toolbox view (Elster, 2015; Hedström & Ylikoski, 2010), the core theoretical knowledge in sociology comprises a collection of mechanism schemes that can be adapted to particular situations and explanatory tasks. This paper aims to rectify this situation by presenting how a mechanistic toolbox works.

The toolbox idea challenges older ideas about the nature of social scientific theory: general social scientific knowledge does not consist of collections of empirical generalizations or highly general principles but of a growing body of mechanism schemes. Theoretical understanding of the social world accumulates when the number of known mechanisms increases or the knowledge of particular mechanism schemes becomes more detailed. There is also room for progress via systematization; mechanism schemes should be mutually compatible, so knowledge progresses as new ways of combining mechanism schemes are developed. This vision of knowledge does not require that mechanisms be ultimately organized into a grand unified theory, the idea of which has always been a distraction in the social sciences. The toolbox view also provides a way to think about how social scientific knowledge could become better integrated. Mechanism schemes are something various subfields of sociology (or more generally social sciences) could share. While the subfields of sociology are currently increasingly distant from each other and develop their own local theoretical vocabularies and theories, the shared toolbox of causal mechanisms could provide the means to integrate the fields in a fruitful manner. The various subfields could employ and develop the same theoretical toolbox and thereby benefit from each other's work. What is missing is a more detailed account of how the idea of a toolbox works.

First, I argue that to understand how mechanism schemes work, we have to grasp the distinction between causal mechanism schemes and causal scenarios (Ylikoski 2019). Causal mechanism schemes are abstract representations of mechanisms that could bring about effects of a certain kind. They are not primarily explanations of particular facts but building blocks for constructing them. Causal mechanism schemes are abstract sketches of causal configurations that can be adapted and combined to serve as parts of causal scenarios. In contrast, a causal scenario is a (selective) representation of a specific causal process responsible for some concrete event or phenomenon. A single causal scenario might be a combination of multiple mechanism schemes and could even contain mechanism schemes that have opposite causal effects. The skeleton provided by a mechanism scheme allows many, often incompatible, ways of building a representation of a particular causal scenario. Thus, it would be a mistake to assume that causal scenarios are just instantiations of mechanism schemes. Mechanism schemes can also be represented and studied by means of formal (e.g. rational choice, agent-based, etc.) models. These often highly abstract models do not address any particular empirical fact. Rather, they are

used to explore the properties of the modeled mechanisms and their combinations: what kinds of things they could explain.

Second, I argue that it is crucial to recognize the primacy of exemplars. Most of the time, social scientists do not contribute to the toolbox by formulating abstractly formulated mechanism schemes but by providing concrete studies – empirical case studies, models, etc. – that provide exemplars of mechanisms. I argue that these exemplars are primary from the point of view of transmitting mechanistic knowledge. While there are some benefits in explicating causal mechanisms schemes more formally, this work builds on exemplars of causal scenarios. Causal mechanism schemes are not empirical law-statements, not are they like symbolic generalizations of the physical sciences. They are cognitive abstractions from concrete causal scenarios and they cannot incorporate all relevant causal information from a particular scenario. Thus, for transmitting causal social scientific knowledge, the exemplars are necessary. From the point of view of constructing causal scenarios, mechanism schemes provide a menu of elements that can be adapted for the purposes of explaining the empirical facts of interest. Known mechanism schemes represent knowledge about causal possibilities: what kinds of things could explain outcomes of a specified type.

Finally, I argue that the main principle for organizing mechanisms is the idea that mechanisms are mechanisms for something. Social scientific knowledge grows when we come to know more alternative ways to generate a particular kind of social pattern: segregation, self-fulfilling prophecy, etc. The toolbox consists of how-possibly mechanisms, and the growth of the toolbox allows the use of research strategies that look for evidence that helps discriminate between competing explanatory scenarios. The identification of mechanisms by their effects implies that although mechanisms can be combined, they cannot be treated like Lego-bricks. Particular causal scenarios can be complex in terms of mechanisms involved. Furthermore, the taxonomy of mechanisms can become messy as there is no principled and non-overlapping way to classify different explananda.

I conclude my presentation with some practical considerations as to how the community of analytical sociologists could facilitate the development of the common toolbox of social mechanisms.

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How-Possibly Explanations, What-If Inferences, and the Problem of Stable Regularities

Patricia Marino

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Session 4

This paper considers the relationship between how-possibly explanations and counterfactual reasoning in the use of theoretical models in the social science context. It's been proposed that when theoretical models fail to provide how-actually explanations (HAEs), they may still provide how-possibly explanations (HPEs); in these contexts, understanding has been characterized as the ability to reason counterfactually and thus "to make correct what-if inferences" (Ylikoski and Aydinonat, Grüne-Yanoff and Verrault-Julien, etc.) I argue that an ambiguity arises in interpreting the concept of "what if" inferences: there is a distinction between counterfactual reasoning saying what happens in a possible world, and counterfactual reasoning addressing what effects are likely to result from changes to the actual world. While much has been written on the difference between learning about the model world versus learning about the actual world, I argue that the second kind of inference requires not only a similarity between the model world and the real world, but also a more substantive -- and potentially implausible -- assumption that the regularities represented by the model are stable in the actual world -- that is, they apply with broad enough scope and sufficient independence from other factors that they function in the same way in the actual world and in various counterfactual circumstances.

To explicate these ideas, I consider two examples of theoretical models often interpreted as providing how-possibly explanations (HPEs) rather than how-actually ones (HAEs): the checkerboard model of residential segregation due to Sakoda and Schelling and the Arrow-Debreu equilibrium model in economics.

The checkerboard model involves coins arranged on a checkerboard then moved according to certain rules -- typically, that a coin is not "content," and wants to relocate, if a certain proportion (say, a third), of its immediate neighbors are of the other type. The model is taken to show that residential segregation can arise through mild, shared, symmetrical in-group preferences. Since many known actual causes of segregation are due to racism, the model is often seen as providing an HPE. Mathematical sociologists using the model have proposed that residential segregation

may be "overdetermined" which means that "discriminatory practices by institutions and white majorities may be sufficient to produce segregation, but eliminating these factors may have minimal impact on ethnic residential distributions due to the persisting effects of ethnic in-group preferences" (Macy 2006; see also Fossett 2006).

There is, of course, a difference between "in the absence of racism, racial residential segregation is possible" and "we have reason to believe that if racism were to disappear, racial residential segregation would persist." The first can be understood to follow from relatively simple assumptions that the structures of the checkerboard are similar enough to structures in the world.

For the second, it is recognized that some model-world relationship is needed, but I argue that the inference also requires potentially dubious assumptions about the nature of the regularities in question. Sugden says the belief that people have "mild segregationist preferences" is justified by "psychological and sociological evidence," and "coheres with common intuition and experience"; in the context of "overdetermination," sociologists ask people what kind of preferences they have for living in different kinds of neighborhoods (Fossett 2006). But the counterfactual conclusion requires also assuming that people's "mild segregationist preferences" would manifest in the same way in the counterfactual conditions indicated. We may be skeptical about this assumption because racial segregationist preferences are interdependent with causes and effects of racism in our society: if racism were to disappear, why would we continue to have in-group segregationist preferences based on race? In the absence of other evidence, it seems more plausible they would disappear or shift to other features such as class or behavior.

Equilibrium models are used to establish the fundamental theorems of welfare economics, including the first theorem specifying the conditions under which an economy will reach a Pareto efficient equilibrium. In a discussion of how we gain understanding from theoretical models, Verrault-Julien (2017) says that these can provide mathematical HPEs: though the Arrow-Debreu model has assumptions that are "descriptively false," it is useful for showing that there is a way to conceptualize the ideas of Smith's "invisible hand" that entails no contradictions. Verrault-Julien says we can infer the model provides a "true HPE," telling us about what is mathematically possible in the world, and not just in the model -- because it can be used to answer what-if-things-had-been-different questions. The model is interpreted as helping us to understand what it is about the actual world that gets in the way of our attaining efficient equilibrium outcomes, thus providing a "measure of our dysfunction" (Athreya 2013).

Here, with respect to "what-if" questions, there is a difference between inferring that the model shows there is some possible world where the model assumptions lead to Pareto optimal equilibria and saying that changes to our world would get us closer to a Pareto optimal equilibrium. The first follows from the interpretation in which the proof shows the formal consistency of the assumptions. But saying that the model is a "measure of our dysfunction" requires the second, and the second requires assuming that the regularities identified by the model -- that people are rational, self-interested, etc. -- are not only empirically apt but also fundamental and stable enough to be unchanged as society changes. As these are generally

regarded as idealizations, and as economic theory is known to be "reflexive" (or "performative"), it is unclear whether such an assumption is plausible.

I conclude that using theoretical models to draw conclusions about interventions or to speculate about how events will unfold in the actual world requires not only that the regularities in the model bear some similarity relationship to those in the world, but also that we assume they are sufficiently fundamental and stable, and independent from other mechanism in play, that they would function in the same way in the relevant counterfactual circumstances. In some cases, the assumption is implausible or questionable.

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Vigilant Trust in Scientific Expertise

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Session 5

This talk investigates the value of trust and the proper attitude lay people ought to have towards scientific experts. Trust in expertise is usually considered to be valuable, whereas distrust is mostly analyzed as something that is harmful and needs to be eliminated. I will draw on accounts from political philosophy and argue that it is not only public trust that is valuable when it comes to scientific expertise – but also public vigilance. Expertise may be distorted in different ways, which cannot be remedied by internal control mechanisms alone. This reveals the importance of some forms of democratic oversight. The proper attitude is vigilant trust in expertise. However, vigilant trust seems to be a contradictory notion: How can one be trusting and watchful at the same time? I will show that it is not, and that trust and vigilance can be compatible to a certain extent. I will do so by distinguishing between different levels of both trust and vigilance.

Interestingly, this argument requires acknowledging the value of some forms of distrust in scientific expertise.

Complexities of economic expertise

Teemu Lari

(University of Helsinki)

Session 5

Philosophers have intensely studied the epistemic and other questions related to expertise (Goldman, 2001; Martini, 2019; Selinger & Crease, 2006). Which experts should one trust and when? What is the proper role of experts in political decision-making? How to react to expert disagreement? I examine the hypothesis that when such questions are asked about expertise in economics, many standard answers are likely to be insufficient or at least require further elaboration. In this presentation, I point out three distinctive characteristics of the discipline of economics and explore how they may complicate questions of expertise more than has been recognized in the existing literature on expertise and economics (e.g. Contessa, 2022; Martini, 2015; Martini & Boumans, 2014; Reiss, 2014).

1) The discipline of economics is divided into theoretical and methodological traditions and schools of thought that make expert disagreements very complex and opaque (Dow, 2004; Lari, 2021; Negru, 2013). Moreover, these traditions and schools stand in a peculiar social hierarchy in which some traditions are mainstream and others peripheral and hardly recognized. This complicates questions related to consensus of experts. For example, if expert consensus on a question is believed to indicate that laypeople should defer to experts regarding that question, is a consensus among the dominant schools and traditions sufficient to constitute a consensus in the relevant sense?

2) During the course of its history, economic theory has been shaped by various extra-academic interests, arguably to a higher degree than many other scientific disciplines (Amadae, 2003; Goodwin, 1998). This complicates questions of expert impartiality and legitimacy. If laypeople should trust experts when they are disinterested and seemingly un-political, does it matter that their theoretical tradition has flourished in part because of political interests?

3) Economics is a unique discipline in the sense that it is characterized by a double identity. On the one hand, economists see their discipline as the study of the economy; on the other hand, they see economics as a particular research approach or perspective (Crespo, 2011; Rodrik,

2015). The former (domain-based) identity encourages economists to see themselves, and the public to see them, as the most knowledgeable experts on topics related to the economy (e.g., employment, inequality, human capital, etc.); the latter (approach-based) identity encourages economists to study these topics only from specific perspectives, using a limited range of approaches and frameworks. This makes opaque which aspects of economic phenomena economists are experts of, thereby complicating attempts to identify the domain and limits of economic expertise.

Observations 1 through 3 are interlinked in complex ways. For example, extra-academic interests and societal values (2) have affected the way the hierarchy between various schools and traditions (1) has evolved, and the observations about double identity (3) probably applies more to neoclassical economics (broadly understood) than to other, more peripheral traditions.

As the above outline conveys, this presentation covers a broad range of questions and does not attempt to provide definitive answers. Instead, the aim of the presentation is to identify and evaluate potential avenues for further, more detailed research.

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Modeling the normativity of joint commitments

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Session 6

Commitments are made, not born with. However, there is considerable unclarity about what precisely one commits oneself to, when one undertakes a commitment to act together with another person. According to normativists about joint action, planning a joint action with another individual entails an obligation to perform one's part and to seek the other's permission, if one wishes to liberate oneself from the joint commitment (as well as correlated rights and obligations for the other participants—see Gilbert 1990; 2013; Gomez-Lavin&Rachar 2019). According to non-normativists, such mutual rights and obligations are only contingently connected to joint action, and are a result of circumstantial considerations, such as agreement making, expectation setting, or mutual promises (Bratman 1999). Commitments can also be used strategically: given the psychological need for coherence (Thagard 2002) and the fact that “we want to be (and to be seen) as consistent with our existing commitments... communicators who can get us to take a pre-suasive step, even a small one, in the direction of a particular idea or entity will increase our willingness to take a much larger, congruent step when asked.” (Cialdini 2016)

In previous philosophical research, the normativity of joint action has been studied primarily by means of conceptual analysis (Bratman 1999; Gilbert 1990; 2013), but also the tools of experimental philosophy have been leveled at our understanding of joint commitments (Gomez-

Lavin&Rachar 2019). Moreover, cognitive scientists (e.g. Michael 2022) and philosophers of economics (e.g. Guala 2013) have recently taken an interest in the normativity of joint actions. For example, the cognitive scientist and philosopher John Michael (2022) has presented experimental data suggesting that individuals' sense of commitment is modulated by a) the investment or effort that each participant is perceived to exert in performing their parts, b) the indispensability of each participant's contribution, c) the recurring nature of the joint action, 4) the degree of coordination between the participants. Similarly, Francesco Guala (2013) has presented evidence from economic experiments that repeated play tends to induce commitments in the case of game-theoretically modelled coordination situations. However, although normativists seem to have had the upper hand in most recent discussions of joint commitment, the debate is far from settled partly because the type of evidence that each side has brought to the table is not generally speaking accepted as valid by the other side to the debate.

My presentation attempts to untangle this impasse by arguing that previous research on the normativity of joint actions has been hampered by insufficient attention to the nature of their explananda as well as the restrictions that are associated with their method of study. Whereas researchers drawing on experimental philosophy (e.g. Gomez-Lavin&Rachar 2019) and Weberian-interpretivist approaches to social science (e.g. Gilbert 1990; 2013) have often assumed that they are dealing with a psychological explanandum (when and how individuals do feel committed), non-normativists have typically taken issue with the moral and social obligations and rights that follow from joint commitments (when individuals ought to feel committed—with worries about bootstrapping moral reasons into the world simply by dint of acting together (e.g. in genocide) motivating a minimalist approach to normativity). Clearly, there are important differences with respect to how one should investigate these phenomena: while social psychological experiments and surveys may seem appropriate to a psychological explanandum, one would not expect the moral or social issues to be settled by similar means (absent further assumptions, such as a contractualist approach to moral normativity). On the other hand, while some type of method of reflective equilibrium would seem appropriate for addressing moral issues that are in a relevant sense “up to us”, philosophers do not seem to share the same intuitions about what type of normativity is involved in joint commitment. One should also be more sensitive to the internal and external validity of the methods that one employs: for example, moral opportunism or self-serving bias may obfuscate intuitions (cf. Malle 2006), while surveys and experiments may be undermined by improper study design (see Löhr 2022). In general, I believe that there is much to be gained by bringing insights from the vast literature on experimental social science (e.g. Guala 2005) and philosophical methodology to bear on discussions about the normativity of joint action.

My paper also develops a new perspective on the normativity of joint action. Instead of assuming that joint actions either are intrinsically normative or that their normativity stems entirely from circumstantial considerations, as normativists and non-normativists would have it, I will develop the idea that (perceptions of) the normativity of joint actions depend(s) in part on folk psychological models (Godfrey-Smith 2005; Sarkia 2021; 2022) that individuals bring to bear in thinking about their joint activities (which may in turn be a matter of framing, and triggered by circumstantial cues). Given a multi-systems view of mindreading (e.g. Goldman 2006;

Christensen&Michael 2016; Apperly&Butterfill 2009), individuals have a broad range of more and less theoretically laden resources to draw on thinking about their joint activities, and the models that they employ also have implications for the normativity of joint actions. Moreover, I will argue that individuals can exert a degree of control in choosing the models that they employ—in contrast to how frames are understood e.g. in the theory of team reasoning (Bacharach 1999)—and this allows us to accommodate the strategic aspects of reasoning about commitments. Accordingly, I may have reason to make demands of you that you do not have reason to acquiesce with, and individuals can rationally disagree about the normativity of joint actions. Although there may never be a unified theory about the normativity of joint commitments, commitments will surely continue to be contested and fought over.

*Power, Reasons and Ideology:
on the epistemology and Metaphysics of Noumenal Power*

Matteo Bianchin

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Session 6

Rainer Forst recently advanced a conception of “noumenal power” designed to model power relations as driven by reasons, so that power relations turn out subject to the normative standards that determine what counts as a justifying reason. Power relations occurs whenever agents are given a reason to think or do things they would not have thought or done otherwise, be it by threats, sanctions, deceit, commands, or arguments. In this paper, I maintain that the theory fares well with both agential and structural powers, yet contend that revisions are needed to meet two demands it places on critical theory. First, the demand for diagnosing domination. Forst advances a conception of domination as arbitrary power, yet it is unclear how noumenal power can accommodate arbitrariness. Second, the demand for a theory of ideology that license ideology critique. The crux here is that ideologies trade on merely apparent reasons that causally depend on the power relations they function to undergird. It is dubious that noumenal powers can account for all that.

In the first section, I discuss noumenal power as a social ontological concept and maintain that, while defined in agential, relational terms, it can account for structural power. Yet I suggest that analyzing how structural power works requires distinguishing between first-order and second-order noumenal power. First-order power marks off the powers enacted by institutions, social

norms, legal rules, bargaining threats etc. Second-order power is the power to be motivated by reasons to accept first-order powers. This distinction is consistent with a wide variety of social ontological accounts of the power displayed by institutions and social structures.

In the second section, I contend that Forst's theory of noumenal power cannot accommodate the conception of domination as arbitrary power it advances, and suggest how to emend it. The crux is that, according to the theory, "bad" justifications must still count somehow as justifications in order to account for social relations that are not justified but look nonetheless legitimate to those who are subject to them. This is problematic when it comes to conceptualize domination. On a harmless reading, bad justifications are justifications that are worse than others, but still provide some (partial, weak, inconclusive) reason for a claim. Yet this does not square with the idea that domination is arbitrary power. On a less harmless reading, they are failed (attempt of) justification that fall short of providing reasons. Yet this is hard to square with the view that domination is an order for which there is somehow a justification that legitimate it.

I suggest that a possible way out is modeling justifications as second-order noumenal powers and distinguishing sharply between real and apparent reasons in order to detect domination as any power that lacks justification and only appear to be justified. Apparent reasons are not "bad" reasons. They are no reason at all. They are rather whatever agents treat as a reason because of entertaining some beliefs whose truth would give them a reason. This makes sense of how noumenal power can be arbitrary. Domination occurs wherever first order noumenal powers are not backed by second-order noumenal power.

The implication, however, is that domination is not a social order legitimated by a somehow defective justification, but a social order for which there is no justification at all – an order governed by naked first order (noumenal) power, whose apparent justification plays at best a stabilizing ideological role. Domination is disguised coercion. Interestingly, however, it can count as structural power as long as first order (noumenal) power relations are structural, which seems to be the case, for instance, where political coercion is not backed by anything near to democratic rule, as in a fascist regime, or where widely asymmetric disagreement points in bargaining interactions depend on structural constraints, as for capitalists and workers in the labor market or for different racial groups in a racist society.

In the third section, I discuss how noumenal power fares with explaining ideology. A sensible reading is that ideologies emerge wherever public discourse or belief formation are systematically distorted by social factors to generate apparent justifications that are resistant to evidence and function to stabilize unjust social arrangements. On this reading ideologies not only provide apparent rather than real reasons to accept social arrangements, but also they are generated by systematic distorting mechanisms whose working causally depend on the (first order) power relations they undergird, and perform the social function to stabilize them. In short, three constraints must be jointly satisfied.

1. Functional constraint: ideologies have the function of sustaining and stabilizing social arrangements by inducing agents to accept and enact them.

2. Epistemic constraint: ideologies result from systematically distorted belief formation processes that affect their epistemic standing.

3. Etiological constraint: the working of the distorting mechanisms can be traced back to social causal factors shaped by the social arrangements their outputs underpin.

Anything that falls short of functioning to undergird social arrangements can hardly count as an ideology. Similarly, nothing can count as an ideology if it is epistemically flawed for contingent or socially irrelevant reasons. Non-contingent distortions and social causes must factor in the process that brings it about. Thus, a sensible account of ideology needs to qualify the functional, epistemic, and etiological properties of ideologies, explain how they connect, and trace how they affect whatever counts as an ideology. The upshot is that a critical theory of power must make room for the functional properties of ideology and for the causal mechanisms that explain how it originates and persists. In short, ideology cannot be accounted for only in terms of noumenal power, and that the theory needs to be supplemented with conceptual resources that come from mechanistic, social structural, and functional explanation.

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Against Evidence-Based Social Science

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Session 7

I wish to offer three arguments meant to counter any potential enthusiasm for developing a research paradigm in the social sciences that is substantially analogous to evidence-based medicine (EBM). I write from the perspective of a philosopher of science who works in a school of medicine in the United States, meaning: I write from the perspective of someone whose work must conform to the various practical and professional norms of evidence-based medicine, and also someone who has written philosophical papers about evidence-based medicine. My arguments therefore are derived from both participant-observer insights and engagement with scholarship — clinical, scientific, and philosophical — about evidence-based medicine.

What is evidence-based medicine? It is an epistemology, an ideology, and an economic framework. It is an epistemology because it proposes an ordered hierarchy of “types” of evidence, ranked in terms of their relative reliability.^{1–3} The hierarchy is meant to be universal and practically comprehensive: all the types of evidence relevant to clinical medicine can be mapped into this hierarchy. It is an ideology because the political economy of medical research in the US

is organized around the production of high quality — i.e. highly-ranked in the EBM hierarchy — trials and studies, and so it is normally the case that the only beliefs about evidence held by practitioners of EBM that are rational are just those beliefs that largely cohere with evidence-based medicine’s epistemology. It is an economic framework because it assigns precise economic value to all sorts of local choices and activities that scientists and administrators can take, and because of this, it has the appearance of rationalizing — even naturalizing — the ideology and the epistemology.

Evidence-based social science is an idea that has received a certain amount of enthusiasm from social scientists, clinicians, and philosophers over the years.^{4–6} Often, but not always,⁷ it is conceptualized using an analogy with evidence-based medicine. My three arguments against attempting to implement this idea are as follows.

1. Again, evidence-based medicine is a universal, comprehensive epistemology. If some data or result (e.g., a proof in pure mathematics) cannot be mapped into the hierarchy, then it is not evidence. Or, if not that, then more subtly: even if it is evidence (relative to some other epistemology), it would be prudentially irrational to treat the data or result as if it were evidence for people who claim to be practicing evidence-based medicine.

There are compelling reasons to think that the social sciences should not embrace universal epistemologies. Perhaps the simplest is just the argument from scientific progress: it has always been the case that different methods — indeed, different scientific epistemologies — have been important causes of scientific progress. Feyerabend is often misunderstood: “Anything goes!” does not mean “anything works”, but rather, you do know that something works until you try it, so you have to be willing to try anything to figure out everything about what works.⁸

A second sub-argument is more explicitly ethical. Put as simply as possible for the purpose of this abstract, epistemic pluralism is an important moral good enacted by the global community of social scientists. This pluralism supports an expansive conception of what kinds of activities count as scientific, and consequently what kinds of activities should be valued because they are scientific. Much would be lost about the comparative inclusiveness of the social sciences if this pluralism were destabilized by efforts to build in the social sciences something like evidence-based medicine, because of the later paradigm’s totalizing conception of evidence.

2. There is no universal causal calculus: the various different causal calculi that have been developed in the last five decades are all known to be governed by non-trivial metaphysical or practical limitations that are often enough violated by reality.^{9,10} This does not render them useless, but it does mean that there is no “one-size-fits-all” technique that can be used to detect, model, or otherwise make reliable inferences about causation in all social contexts.^{11,12}

Researchers in the evidence-based medicine paradigm have an admirable history of successfully detecting pattern-forming causal effects using randomized control trials. While there is healthy criticism about the technical details of reasoning about (for example) average effects or the best way to use informative priors, the hierarchy of evidence-based medicine also excludes from serious study all sorts of causal questions that either cannot be assessed using the logic of the

types of evidence in EBM's hierarchy, or, more subtly, would be imprudent to assess given the ideology and economics of evidence-based medicine. An example of this is the dearth of ethnographic research in both clinical research and nursing science. But the point here is just that standardization of techniques for causal inference that are patently successful in their specific "metaphysical area of application" is not evidence of the discovery of a universal causal calculus.

We are likely to more fully map all of the different techniques for making reliable causal inferences in social sciences if no single technique or family for making causal inferences becomes standardized. Were one technique to become standardized on the model of EBM, it would be more difficult, compared to the status quo, to make continued progress accumulating causal knowledge of differing social worlds.

3. There is no ethically or politically neutral way of describing any social reality. Social scientists who wish to (for instance) understand the role that values play in their science or (to pick a bigger example) make their scientific work the functional core of a larger project of ethical or political advocacy must necessarily work with an ontology that coheres — ideally causally — with both their ethics and their methodology.

Evidence-based medicine violates this principle. Its universalism about evidence is often taken to license a strong form of naive realism, according to which the highly organized clinical spaces that are governed by administrators and layers of regulation, policy, and conventions of best practices are seen as "the way the world just is" and are not seen as, for instance, contingent expressions particular balances of economic and political power. More importantly, this conception of reality itself is taken to be ethically inert: in the US, there are thousands of clinicians working in the area of health equity who treat unequal outcomes as simple causal effects, a causal logic that, because it misrepresents any social factors that cannot be treated as simple causes, does not cohere with many of these clinicians' background egalitarian political commitments.

There is a risk, then, that ethically substantive social science would be undermined if, in an effort to model evidence-based medicine, social scientists adopted methods or beliefs that violate the principle mentioned above — by for instance believing that some kind of naive realism is licensed by successful application in the social sciences of techniques for causal analysis borrowed from clinical research.

These three arguments overlap, of course; each tries to highlight one aspect of the contemporary social sciences that may be undermined if enthusiasm for EBM is taken to mean that something like evidence-based medicine should be institutionalized in the social sciences. Taken together, my arguments are different ways of asserting that it would be a scientific — and likely also an ethical — mistake if the social sciences, in an effort to mirror EBM, weakened their commitment to methodological pluralism.

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Applying Evidential Pluralism to Evidence-Based Law

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Session 7

Evidence-based law (EBL) is a recent approach to the law that seeks to make best use of evidence to justify laws and regulations (Rachlinski, 2011). Evidence-based law can be understood as an extension of evidence-based policy. The question arises as to how exactly to make best use of evidence to justify laws and regulations.

The most obvious methodological approach to try is that of evidence-based medicine (EBM) and evidence-based policy (EBP): these are evaluation methods and evidence hierarchies that place a strong emphasis on RCTs (Luján, 2023). The question naturally arises as to whether these standard methods do in fact provide the best means to assess the effectiveness of laws (Davis, 2019; Marin dos Santos 2021; Rantala et al., 2023). The main reason to think that evidence-based law may need to look beyond standard methods is that it can be practically impossible to produce RCTs of high enough quality to establish the effectiveness of laws.

In this paper, we develop an approach to evidence-based law, called EBL+, based on the principles of Evidential Pluralism. Evidential Pluralism is a theory of causal enquiry which maintains that one needs to scrutinize mechanistic studies alongside the experimental and observational studies that are the focus of present-day EBM and EBP (Russo and Williamson, 2007; Parkkinen et al. 2018; Shan and Williamson, 2023).

To illustrate the methods of EBL+ and its benefits, we discuss the example of Covid-19 public face mask mandates. The novel, rapidly changing and complex problem of Covid-19 has prompted calls to move to a more inclusive approach to evidence (Aronson et al., 2020; Greenhalgh et al., 2022). A narrow focus on experimental studies, especially RCTs, resulted in controversy and uncertainty concerning the effectiveness of public health interventions to reduce the spread of Covid-19, including legal requirements to wear a face mask in public. An influential Cochrane Systematic Review, which included only RCTs, found face masks make little or no difference to the spread of Covid-19 (Jefferson et al., 2023). Various limitations of the review and of the studies included have been highlighted (Greenhalgh et al. 2022). Taking account of a broader range of evidence is crucial to understanding the effectiveness of face masks in reducing transmission. Understanding the effectiveness of face mask mandates requires taking account of an even

broader range of evidence, including crucial insights from social and behavioural sciences. As we show, Evidential Pluralism offers a framework for systematically combining a wide range of evidence to provide robust, overall evidence of the effectiveness of Covid-19 face mask mandates. This case study therefore provides a good example of the need for and benefits of an approach to evidence-based law based on Evidential Pluralism.

*A Novel Type of Precautionary Argument for Situations of Severe Uncertainty
in Science and Policy*

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Session 8

Slippery slope arguments (henceforth, SSAs) oppose particular actions/policies by arguing that these actions/policies likely lead to specific unacceptable (e.g. catastrophic) consequences (e.g. Walton, 1992, 50-52, Whitman, 1994, 85). SSAs are articulated and debated in a wide range of contexts across science/policy (e.g. Fumagalli, 2020, Rizzo and Whitman, 2009). However, SSAs are not applicable in situations of severe uncertainty, i.e. situations where policy makers ignore both the probabilities of different possible outcomes and some of the possible outcomes (e.g. Hansson, 1996, 376). For in such situations policy makers lack the knowledge of probabilities/possible outcomes required to show that the examined actions/policies likely lead to specific unacceptable consequences (e.g. Hill, 2019, 225-6). Yet, situations of severe uncertainty are increasingly widespread across science/policy contexts (e.g. Stern et al., 2022, on climate change policies; Connell, 2017, on genetic manipulation technologies; Bostrom, 2014, ch.7-9, on AI applications).

In this paper, I articulate and defend a novel type of precautionary argument for situations of severe uncertainty in science and policy, which I term precautionary slippery slope arguments (henceforth, PSSAs). Whereas standard SSAs oppose particular actions/policies by arguing that those actions/policies likely lead to specific unacceptable consequences, PSSAs prescribe specific precautionary measures (e.g. pausing, delaying or banning the development/rollout of a novel technology) by arguing that the examined actions/policies place policy makers on a slippery slope with likely negative and possibly catastrophic unknown endpoints (e.g. Lenton et al., 2019, on the possibly catastrophic consequences of trespassing climate tipping points; Evans, 2021,

on the possibly catastrophic consequences of forthcoming applications of human germline genome editing; Roff, 2014, on the possibly catastrophic consequences of the proliferation of lethal autonomous weapon systems). My main claim is that despite several influential objections put forward in the philosophy of science/social science literature, PSSAs provide cogent reasons/evidence for the precautionary measures they prescribe in a wide range of situations of severe uncertainty across science/policy. As such, PSSAs effectively demonstrate how policy makers can justify precautionary measures against potentially catastrophic consequences even in cases where they lack detailed information about such consequences.

The paper is organized as follows. Section 2 explicates the argument structure of PSSAs and identifies several factors that bear on the strength of PSSAs, i.e. the extent to which PSSAs provide cogent reasons/evidence for the precautionary measures they prescribe. Section 3 defends PSSAs against six influential objections put forward in the philosophy of science/social science literature, namely:

- the objection from insufficient evidence, which holds that PSSAs do not withstand scrutiny because PSSAs typically fail to demonstrate that the examined actions/policies lead to possibly catastrophic consequences (e.g. Sunstein, 2021, 43-60, Walton, 2015, 303-5);
- the objection from excessive precautions, which holds that PSSAs do not withstand scrutiny because PSSAs' precautionary prescriptions typically prevent people/society from enjoying the benefits of scientific progress and technological innovations (e.g. Castro and McLaughlin, 2019, 12-18, Savulescu, 2001, 415-9);
- the objection from underdetermined precautions, which holds that PSSAs do not withstand scrutiny because PSSAs typically fail to provide precise and plausible criteria to select among the many precautionary measures available to policy makers (e.g. Posner, 2004, 140, Sunstein, 2005, 26);
- the objection from arbitrary thresholds, which holds that PSSAs do not withstand scrutiny because PSSAs typically fail to provide precise and plausible specifications of what levels of risk suffice to trigger PSSAs' precautionary measures (e.g. Jackson and Smith, 2006, 275-7, Stefansson, 2019, 1211-3);
- the objection from absolutism, which holds that PSSAs do not withstand scrutiny because PSSAs typically assign absolute priority to avoiding catastrophic consequences over other reasonable goals (e.g. maximization of expected benefits) and therefore can lead to irrational decisions (e.g. Colyvan et al., 2010, 224-6, Peterson, 2006, 598-9);
- the objection from diachronic incoherence, which holds that PSSAs do not withstand scrutiny because PSSAs' precautionary prescriptions typically vary in implausible ways depending on whether policy makers target specific actions/policies (taken individually) or multiple actions/policies (taken collectively, e.g. Andreou, 2007, 240-2, Thoma, 2022, 61-63).

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Climate Change, New Social Risks, and Collective Agency

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Session 8

Some branches of sociological inquiry recently suggest that the rising impact of disasters, and the push towards ecological transitions, require a comprehensive understanding of how social and environmental hazards are interlocked at a global and local level (Cucca et al., 2023). Therefore, current environmental challenges drive social science researchers, international institutions, and practitioners to interweave different concepts of hazards. For instance, investigation on eco-social work and policy is inclined to organically link the notions of “climate risks” (Johansson et al., 2016) and “social risks” (Hirvilammi et al., 2023). Nevertheless, this association is challenging for many reasons.

First, risks related to unpredictable eco-social systems involve hazardous events that are often interdependent; therefore, pairing them can deepen the uncertainty in decision-making (Helgeson, 2020) within mitigation or adaptation strategies. Second, the structural intersection of global and local factors varies contextually and engenders radically different risks. In other terms, understanding how to inhabit the uncertainty of the social impact of ecological crises can be conceived as a “superwicked problem” (Levin et al., 2012).

However, countering climate risks demands several coordinated activities, usually developed by collective actors, such as institutions, NGOs, groups of researchers, panels of experts, and others. Shared awareness regarding risks among scientists, policymakers, and laypeople is also routinely evoked as a crucial factor in solving ecological crises (Khatibi et al., 2021). Following this line of thought, philosophical contributions within collective epistemology (Schmid et al., 2011) and responsibility (Bazargan-Forward & Tollefsen, 2020) fields might illuminate the interaction between the knowledge corpus and decision theory when different social and climate risks are jointly considered. Nonetheless, investigations on climate-related social risks and collective agency have yet to dialogue systematically. At first glance, this theoretical interaction could provide elements for reframing epistemological and decision-making issues. Against this background, this paper examines how we can account for collectively inhabiting uncertainty in contexts where social and climate risks intertwine. It will be argued that a coherent theoretical image of decision-making under deep uncertainty aimed at tackling climate-related

social risks should consider how social risk knowledge production, sharing, and management involve plural collective agencies.

The investigation will start by analyzing some standard accounts of climate, ecological, and social risks, and define a few open theoretical issues that have recently arisen within the literature concerning eco-policies (Costella et al., 2023). More specifically, the emergence of “new social risks” opens the conceptual issue of the variety of notions of social risks and vulnerability (Lupu, 2019).

From a different angle it can also be highlighted that several authors convergently recognize that the vast plurality of losses due to climate change can affect both individuals and communities (IPCC, 2023). Nevertheless, hazards affecting collective subjects are not treated specifically, but they are rather considered as mere aggregations of individual ones. These issues can be related to the unclear relation between climate and new social risks which are ontologically and epistemologically assimilated in the disciplinary literature. Conversely, philosophical research on social ontology has highlighted that “social kinds” (Haslanger, 2012), such as vulnerability, demand a different ontological understanding than “natural” ones. Consequently, a critical examination of the links between social risk assessment and decision-making will be sketched from the standpoint of collective actors. By considering the frequent misalignment between experts, laypeople, and policymakers, it will be suggested that the link between knowledge corpus and risk management in real-world contexts seems indirect and circular (Ebert & Durbach, 2023).

Tackling this issue, it will then be proposed that the notion of collective epistemic agency (Fleisher & Šešelja, 2023) can play a crucial role and given that risks are the product of hazard, exposure, and vulnerability, it can be stressed that vulnerability evaluations are strongly subject-relative, for example different actors could attribute varying ethical and epistemic salience to different risks. To overcome these ambiguities, the literature on risk has increasingly been paying more attention to the notion of “collective responsibility” (Placani & Broadhead, 2023), and this tendency suggests the relevance of developing some (new) forms of collective agency. However, accounting for decision-making under deep uncertainty and risk communication between experts and laypeople from the lens of collective action is challenging. In fact well-defined collective actors that can cope with new social risks have yet to be created. Frequently, there is only a bundle of heterogeneous subjects with different powers, epistemic assessments, preferences, and values (Thorstad, 2022), where group reasoning and a group ethos are absent (Pettit, 2023; Tuomela, 2013).

Nonetheless, following Frank Hindriks’ theses on collective responsibility, these situations can be understood as cases where singular actors should join forces to cope with a shared challenging problem (Hindriks, 2019). More specifically, this line of thought suggests investigating whether any specific collective epistemic responsibility can be identified. It is worth noting that during the past few decades there has been a growing interest in understanding whether groups can be identified as epistemic agents in a non-derivative sense (Tollefsen, 2004). Furthermore, recent contributions on the epistemology of hazards seem to suggest that focusing on “how” scientific communities decide what to communicate to lay people can have a significant impact in

improving the experts' sense of (collective) accountability of risk communication (Zanetti et al., 2023). Therefore, drawing on current philosophical research on agency, it has been suggested that collective actions are "layered" (Ferrero, 2021) and dynamic, social risk assessment, risk-taking, and risk imposition can be framed as discrete levels of joint actions that can build collective decisions.

To sum up, avoiding epistemic harm related to social risk prevention may require collective epistemic duties on the part of experts to join their forces to help mobilize people by responsibly structuring risk communication and decision-making under deep uncertainty. In addition, this collective framing of decision-making hints at considering the issue of collective learning in iterative decision-making cycles (Shteynberg et al., 2020).

And lastly, a few implications will be outlined regarding interdisciplinary investigations on risk and expertise regarding vulnerability related to climate challenges (Pongiglione & Martini, 2022). It will be argued that experts might act as a two-way interface between laypeople and scholars in identifying which research problems should be tackled, and listen to laypeople's concerns, thus fostering a collective epistemic (sense of) agency as suggested by research on "knowledge co-production" with indigenous people (Latulippe & Klenk, 2020).

Towards an account of responsible modelling in economics

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Session 9

Economics has immense power and epistemic authority among both policy makers and the public. Its models affect our beliefs, behaviors, and institutional arrangements. They can do so directly when they are used by policy makers to justify their policies, but also more subtly, when they affect our self-understanding, incentives, and behaviors. With this power and authority however also comes responsibilities, specifically, a responsibility towards the consequences of one's scientific claims, including consequences that extend outside the strict bounds of scientific domain (Douglas 2003). But what kind of responsibility should economists bear? How is it to be fulfilled in economists' modelling practices without compromising their epistemic performance?

In this paper we focus on the responsibility of economics for unintended harmful effects of their models and on what meeting this responsibility entails for current modelling practices in economics.

To address these questions, we will proceed as follows. First, we draw on current literature on responsibility to distinguish relevant kinds and dimensions of responsibility (notably, epistemic vs. moral responsibility; forward-looking and backward-looking; individual, collective, and institutional). Second, we examine the responsibility for unintended harmful effects of their in the following sets of cases: use, misuse, and dissemination.

With model use we refer to cases in which a model (or set thereof) is deployed to justify a policy or design. For example, in the 80s and 90s policy makers appealed to international trade models to justify their trade-liberalization policies, policies that turned out to cause significant harms to big segments of the population in the countries where they were enthusiastically implemented. Many have argued that economists should be concerned with such harms. For example, DeMartino (2022) argues that economists are responsible for such economics-induced harm and should pay (more) attention to such harms and ways to avoid or mitigate them.

We agree. Yet, DeMartino's framework appears to pinpoint economists' responsibility to their model's actual causal contribution to a given harm. What about less clear-cut cases of economics-induced harms, like when a policy maker implements a policy based on the wrong interpretation of an economic model, i.e., when the model has been misused? Blaming economic modelers for harms caused by an intervention they have only very marginally contributed to bring about seems unnecessarily harsh. It may also have undesirable epistemic consequences if economists qua scientists were to be burdened with the responsibility for all possible misuses of their models. We show that if we focus on forward-looking (rather than backward-looking) responsibility, which is not tied to causal contribution but to the ability to do something about a harm, there is room for retaining economists' responsibility to foresee and mitigate.

A similar argument applies to cases in which harms do not result from the use of a model for policy or design purposes but from its dissemination. For example, it has been argued that studying economics makes us more self-interested, an outcome society may consider harmful (e.g., Frank et al. 1993, Ferraro et al. 2005). It has also been suggested that the economics framework provides a justification for the over-zealous marketization trend, which has likewise been regarded to have negative societal effects (e.g., Sandel 2013). As in the previous cases, economists have the responsibility to foresee such effects and devise ways of mitigating them even if they were not directly involved in bringing about such effects. It could be objected that if the unintended effects of a policy intervention can be predicted to an extent and this is in fact what much of economics is in the business of doing, what, if any, will be the effects of the dissemination of particular model-based scientific claim are hard or even impossible to predict. We argue that the difference in predictability between this and the previous cases is only one of degree rather than kind. In some circumstances, the likely harms of dissemination may be relatively easy to foresee, while in other a policy's unintended consequences may be impossible to reliably predict in advance of the policy's implementation.

Finally, we argue that the responsibility of foresight and mitigation is a collective and institutional one. This is because these issues are linked to shared norms and institutional practices within the economics discipline and their answers cannot come from the individual scientist alone.

Therefore, responsible modelling practices may require changes to norms and institutions prevalent in economics. To give only one example, the publishing culture in economics encourages drawing policy conclusions from highly unrealistic models and often with little concern for what would happen if such policies were implemented as such. Perhaps economists should be more cautious when drawing policy conclusions. Or as suggested by Dani Rodrik (2015), their models should be accompanied by explicit user's guides to minimize the risk of misuse. In both, current norms in the discipline seem to require revision.

To conclude, economic modellers should care about the unintended effects of the use and dissemination of models. Bearing this kind of responsibility requires rethinking of the norms and practices that characterise economics in ways that both promote its epistemic project and fulfils its responsibilities to society.

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The chances of Verstehen as understanding in multispecies ethnography

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Session 10

The aim of this contribution is to evaluate the understanding gained in a recent orientation in cultural anthropology known as multispecies ethnography from the point of view of the specific sort of understanding known as Verstehen. The aim of this new orientation is to extend the ethnographic inquiry beyond humans to other nonhuman species. Understanding in anthropology was claimed in the tradition of Verstehen – the specific understanding in humanities and social sciences. I will argue that the new orientation puts pressure on interpreting understanding as Verstehen. This happens mainly due to the reliance on scientific knowledge that play an essential role in the approach.

In the first part I will first introduce the new orientation of multispecies ethnography, its goals and promises as well as some critical reactions in the field. I will further identify two main critical points that will impact on my analysis of understanding. The first is the pretention of the orientation to approach via ethnographical methods any species: the closer species to humans as well as the distant ones. This makes it vulnerable to an objection based on the range of application (ORA). The second one marks the reliance on scientific knowledge delivered by natural sciences for engaging such an approach and therefore contaminating the specific sort of understanding of ethnography with a scientific one (therefore the contamination objection (CO)).

In the next step I will set the framework of my analysis of understanding by recalling the three interpretations of Verstehen as presented in Martin's reconstruction of Dilthey's original position (Martin2000). These are the reliving interpretation that consists in reliving the subject's experiences using empathy, the reconstruction interpretation involving the reconstruction of the subject's inner life and the cultural context interpretation that involves relating the subject's behavior to a cultural context. I will leave out the second one since it seems to be problematic for our inquiry due to the difficulties of accessing the other species' inner life.

The cultural context interpretation implies in our case a reference to animals' cultures. Despite the existence of well-articulated general definitions of culture that can be applied across species (as for example Ramsey's definition) and a burgeoning recent scientific literature investigating cultures in animals, the interpretation presents a serious limitation. This resides in the fact that the ethnographers' access to such cultures is mediated by ethological or other scientific

approaches, so that their understanding is subjected to the CO. As from a Geertzian perspective the ethnographers have to build their interpretation on the one of the investigated subjects, which in our case this is missing or delivered by the other scientists. So, they might rather interpret the scientists' interpretations than the subjects' one. This situation amplifies in case of distant species. For the closer species there might be some space to be claimed for a specific ethnographic contribution. This will be discussed in more detail under the remaining interpretation.

This last interpretation of Verstehen involves reliving the experiences of the subjects via a procedure known as perspective taking (PT) in which empathy plays a central role. One might claim some plausibility of such an approach by invoking some relaxed circumstances esp. in closer to human species. I will adopt Maibom's reading of PT (Maibom2022) as situating oneself imaginatively "in the web of the relationships of the subject" (or "seeing the world in terms of how it affords action and satisfies interests") since it is the most plausible to be applied to other species. Even in such a case we see that the understanding fails under CI since knowledge of the web is delivered through a scientific approach. A direct failure of the PT procedure might be attributed in a narrow way to the limitation of emotion attribution and interpretation in other species, as scientifically documented. This is more obvious in distant species where emotion-attribution is more problematic, but it holds even in closer species regarding emotion interpretation. In a broader sense the failure might be attributed to the sort of cognition that is species-specific due to its embodied nature of situating in a world esp. in terms of action affordance and satisfaction of interests.

In the second part I will analyze directly the way understanding is gained in multispecies ethnography by discussing a recent study of J. Hartigan (2021) on wild horses in Galizia, Spain. Hartigan intends to make a consistent contribution to the methodology of multispecies ethnography by arguing "for an ethologically informed ethnography that extends cultural analysis to other social species". I will argue that we might have difficulties in cashing something on the side of Verstehen in his account.

Hartigan studies the annual ritual of shaving the wild horses (*rapa das bestas*) with the intention of approaching horses' sociability directly and study the impact of the ritual on them. He applies ethological techniques in his direct observations of the subjects and gains a first level of understanding. This is not different from any other understanding gained via an ethological inquiry. The specific touch of Verstehen might come on the second level when the author applies concepts from social analysis - Goffman's theory of social interaction. The author claims that horses as social subjects 'engage in ongoing interpretative work in understanding, reproducing and contesting their relationships'. Nevertheless, it seems we have here rather an analogical attribution via a conceptual transfer and not a direct access via any of the known ways of Verstehen.

In the end I will briefly suggest that some recent theories of understanding might better account for the understanding gained in our case. I will take as reference two of them: Wilkenfeld's (2013) and Dellsen's (2020) accounts that take understanding to involve representation manipulability

or grasping a model of phenomenon's dependence relations. Both theories can accommodate our case in a reasonable way and the chances of retrieving some of *Verstehen* ingredients, though not inexistent would involve some deeper reconsiderations on the part of the *Verstehen* adherents.

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Necessity and contingency in history

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Session 11

Was World War One inevitable or the result of unlikely chance? Such questions arise ubiquitously in history. And not just history: analogous questions arise with respect to possible future events too. The continued rise of populism, the displacement of professions by AI, or war in Taiwan – are these inevitable, or are they easily preventable? What sense can we make of these questions, and what evidence could bear on them? Recent work in the theory of causal explanation points the way forward, but this is not yet fully appreciated. It turns out to contradict several influential views.

The sense of necessity standard in metaphysics or modal logic cannot be the answer here: no event of interest to historians or social scientists is true in every possible world. A better answer is to analyze in terms of causal sensitivity: a 'contingent' event is one that would not have occurred had conditions been only slightly different; a 'necessary' event is one that would still have occurred in a wide range of alternative conditions (Ben-Menahem 2009). Analogously, a ball will end up at the bottom of a bowl from any starting position within the bowl – it does not matter which

of a large range of initial starting positions is selected, and in this sense the ball's finishing position is 'necessary'. A ball ending up balanced on top of a perch, on the other hand, is highly contingent because even a very small change in the ball's history would have led to a different finishing position. Causal sensitivity is a matter of degree; thus, necessity and contingency in our sense are also a matter of degree.

This analysis may be formalized using mainstream theories of causal explanation (Woodward 2003, 2006). From that secure foundation, I derive consequences for a wide range of issues.

What is subjective and what is objective about necessity and contingency, understood in our sense? On the subjective side, they depend on what variation is deemed salient to a judgment of causal sensitivity. In the World War One example, is it enough to ask if the war would still have happened if Franz Ferdinand had not been assassinated? What of larger changes than that? And how much variation is admissible on the effect side – would a roughly similar war starting a little later still count as 'World War One'? How similar, and how much later? Specifying these relativizations is interest-relative. But once they are specified, thereafter historical necessity and contingency are no less objective than other relational concepts such as compass bearing or relative velocity.

What kind of evidence is required to support claims of historical necessity and contingency? The answer is evidence bearing on what would have happened in the relevant scenarios in which history is altered. In essence, this is no different to regular causal claims, which also require consideration of such counterfactuals. Contrary to many influential views in philosophy of history (e.g., White 1973), claims of historical necessity and contingency are therefore no more arbitrary or unscientific than are regular causal claims.

Historical contingency need not be associated with chance or randomness. Instead, both necessity and contingency may be analyzed causally. Further, because causal sensitivity is a matter of degree, historical necessity need no longer be associated with full determinism.

Concepts such as 'fate' and 'teleology' may also be glossed causally, as referring to outcomes that are necessary in our historical sense. In this way, the sense of inevitability that motivates these concepts may be accommodated without any resort to dubious metaphysics.

Human free will may also be accommodated, at least so far as historical explanations are concerned. Contrary to, e.g., Isaiah Berlin (1954), we need not dismiss talk of historical inevitability as a misplaced attempt to emulate the lawfulness of natural science that squeezes out any role for human freedom and responsibility. Rather, individual human actions, just like any other causal factor, are more likely to be difference-making for contingent events, less likely to be so for necessary ones.

Those who wish to emphasize the possibility of change will naturally emphasize aspects of social reality that are historically contingent, or they will argue that these aspects are more contingent and less necessary than they might seem. Thus, we see why Marx, Nietzsche, Foucault, and other radicals naturally emphasize causal sensitivity. Class structure, economic relations, moral

mores, cultural power – argue these authors – are not inevitable. They might have been different, or might in the future be changed, more easily than is commonly supposed.

A causal framework can incorporate chance. The indeterminism that matters here is regarding what effect results from a particular specification of cause and background conditions, in other words, it is indeterminism relative to a relevant model or explanation. Indeterminism in this sense is ubiquitous in history and social science. In essence, ‘chance’ then refers to factors that are unmodeled. Other sciences, too, such as biology and neuroscience, appeal to such a notion of chance all the time. It is then easy to represent the extent of the causal sensitivity accounted for by our models and theories, versus the extent that is due to chance. (It is also easy to represent how, contrary to the views of many historians, e.g., E. H. Carr (1961), chance may play a role in historical explanations.)

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Scientific Constructivism and Relativism about Causal Inquiry in the Social Sciences

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Session 11

In its most recent annual report on the state of global democracy, the Economist Intelligence Unit claimed that “the increasing incidence of violent conflict has badly dented the global democracy score” (Economist Intelligence Unit 2024). Claims such as these rely on constructed concepts

and measures, like ‘democracy’ and ‘violent conflict’: philosophers, political scientists and laymen alike agree that democracy, for instance, is not one homogeneous property but instead a family resemblance covering different aspects of a political regime (cf. Cartwright and Runhardt 2014). Which of these aspects is considered relevant to (or constitutive of) democracy depends on the community one is in (cf. Crasnow 2015). Does this mean that the Economist Intelligence Unit’s claim that violent conflict caused a decline in democracy globally, is true for some communities but not for others? Or can one reach some level of intersubjective agreement about such claims across communities?

Such questions are at the foundation of scientific constructivism, a purportedly new methodology for causal inquiry in the social sciences defended by prominent sociologist and political scientist James Mahoney (Mahoney 2021; 2023b). Mahoney’s methodology assumes both that the way concepts like democracy and conflict are defined depends on the “collective understandings” of communities (Mahoney 2021: 13), but also that one can use evidence to rationally assess the truth of causal claims which involve such concepts. Mahoney’s methodological arguments matter greatly to philosophers of the social sciences interested in describing and evaluating alternatives to traditional quantitative methods; for instance, one of Mahoney’s central claims is that variable-based analysis is inherently flawed, because it assumes variables have hidden essences which “confer predictable dispositions on category members” (Mahoney 2021: 25). Mahoney also claims that out of the existing qualitative method alternatives, set-theoretic methods like Charles Ragin’s qualitative comparative analysis (see e.g., Ragin and Fiss 2017) are best at avoiding this pitfall. According to Mahoney, set theoretic methods allow researchers to move beyond essentialism yet still discover causal truths. Recently, Stephen Turner has critically examined the consequences of Mahoney’s methodology for inquiry (Turner 2023; see also the reply Mahoney 2023a), amongst others by placing it within broader traditions in philosophy of the social sciences but also by drawing out some of its advantages at handling causal heterogeneity.

In this conference paper, however, I will discuss a more damaging consequence of Mahoney’s work for (the philosophy of) causal inquiry. First, I show that the set-theoretic method defended by Mahoney makes the truth value of causal claims relative to the semantic context of the community of individuals who make the claim. I contrast this with Mahoney’s (and many others’) implicit belief that some communities are epistemically privileged about social scientific claims and that some agreement about social causal claims is even to be expected even across communities. Second, using real examples of social scientific research, I show the assumptions together lead to a relativist dilemma: either (1) one accepts a more extreme version of relativism about causal claims than scientific constructivists like Mahoney wish to accept (one in which there is no rational assessment of the truth of social scientific causal claims); or (2) one admits the existence of what I will call stabilizing factors for causal inquiry, which greatly weaken scientific constructivism’s anti-essentialist move.

The conference paper, then, is set up as follows. In the first part, I outline Mahoney’s scientific constructivism as well as briefly survey its reception by philosophers like Turner. In the second part, I introduce the new and damaging relativist dilemma inherent in scientific constructivism to

provide pushback to scientific constructivism's expansive philosophical and methodological claims. In the last part of the paper, I discuss the stabilizing factors for causal inquiry one may use to solve the relativist dilemma. I argue that these stabilizing factors are not causal mechanisms (as amongst others Turner has assumed in his discussion of Mahoney). Instead, I argue that categories like democracy depend on what Sharon Crasnow has called "(real) configurations of polities, made up of people, institutions, and practices" (Crasnow 2021, 1210). As such, the Economist Intelligence Unit claim that violent conflict has hurt democratization across the globe must be based on "some degree of public accessibility" (ibid) to these real configurations, which lends these claims (a measure of) "coherence objectivity". Moreover, following Crasnow I argue that such configurations can on occasion be tracked using traditional quantitative methods, without the need for a set-theoretic approach.

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Vindication of “game-playing” as rational progress in economic theorizing?

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Session 12

In the recently revived debate on scientific progress (Shan 2022) two accounts are particularly relevant to economics: semantic (Niiniluoto 2012; 2022) and functional (Boumans, Herfeld 2022). The semantic account explains scientific progress in terms of increasing truthlikeness, while the functional account defines progress in terms of the usefulness of a scientific practice for reproducibly defining and solving problems. Proponents of the semantic approach claim that such marks of progress as increasing knowledge or understanding, or repeatable problem-solving can be explained as consequences of getting closer to the truth (whatever the correct measure of truthlikeness). While the general response of functionalists may be that the semantic account is difficult to apply to specific cases, M. Boumans and C. Herfeld (2022) substantiate this view by examining how the reuse of templates in economic modeling allows problems to be defined and solved. (García-Lapeña 2023) addresses a different problem of the semantic approach, where a focus on adequacy to particular truths may compromise the nomic regularity. The proposed solution is to measure likeness to the truth in a two-dimensional space of accuracy and nomicity.

This presentation focuses on the question to what extent this amendment to the semantic view of progress might give it an advantage over the functionalist alternative when economic modeling is concerned. I briefly present arguments in favor of the two-dimensional approach to truthlikeness in a visual format (with minimal engagement of the underlying technicalities). The original illustration shows that the progress from Aristotelian to Newtonian and then to Einsteinian physics was a simultaneous advance along both dimensions, but the second step was a mainly an improvement in nomicity, and less – in accuracy. Given that the examples used are exclusively from physics, the question is whether and how this two-dimensional account of approaching the truth can be extended to capture progress in economics? I follow (Boumans, Herfeld 2022, 224) in limiting the discussion to “those subfields in economics that predominantly use models”.

Following a recent debate between (Gilboa et al. 2022) and (Sugden 2023), I focus the argument on analytical modelling in economic theory. In particular, I draw on an important insight in (Sugden 2023) and the distinction drawn between economic theories such as R. Aumann’s that make minimal use of evidence and the remaining majority that make more substantial use of evidence.

R. Sugden argues to the effect that the latter, without some kind of semantic understanding of progress, can turn into “game” among economic theorists whose purpose turns out to be to outsmart other theorists rather than to explain the evidence, make predictions, or provide useful policy recommendations. Drawing on Sugden’s distinction, I argue that the two-dimensional model of truthlikeness may actually vindicate this kind of theoretical “game-playing” in economics as a genuine progress toward truth. By a systematic decoupling nomicity from accuracy, the two-dimensional model allows for a positive measure of progress in cases where the level of accuracy or the level of nomicity is fixed at some point. This would imply either that one can progress by increasing nomicity without a corresponding increase in accuracy, or, conversely, that one can progress by increasing accuracy without an increase in nomicity. A pertinent lesson from Sugden’s analysis is that, even with a fixed level of accuracy, it still matters, at what level the accuracy was fixed. For if the level of accuracy is inherently low (e.g. Aumann’s model), the claim that there is a theoretical progress may well be defensible. For higher levels of fixed accuracy, however, as I argue, it is very likely that a decoupled nomic measure of progress vindicates theoretical game playing in economics. However, this falls short not only of the semantic, but also of the functional account of progress, as I argue. A more general lesson here will be to specify conditions for economic models under which the decoupling of accuracy and nomicity would not undermine genuine progress.

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Major disciplinary crises in the history of psychology as a discipline

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Session 12

In recent years there has been much talk of replication crisis in psychology and what it means for the scientific status of the discipline. However, it is not the first time that psychology is experiencing a disciplinary crisis. There were two other major crises that swept the discipline earlier: the late 19th and early 20th century crisis about the possibility of having a scientific psychology and the 1960s and 1970s crisis of method, relevance and scientific status of the discipline.

Looking at these earlier crises helps make sense of some of the causes of replication crisis as well as the proposed solutions to it. Besides, one can trace similar debates across all these major disciplinary crises, revealing that earlier crises were never really resolved and similar questions haunted the discipline again. Taking a *longue durée* perspective enables us to make sense of psychology as a discipline and its methodological, social, epistemic, ethical, and political implications, and that the replication crisis should not distract us from other issues like theoretical and methodological hegemony which were more at issue in the earlier crises and never completely resolved.

I begin with Kuhn's definition of crisis in terms of the accumulation of anomalies in normal science. Yet Kuhn considered anomalies in terms of discrepancy and clashes between theory and data. In contrast, I argue that, at least in psychology, anomalies can encompass a wider range of clashes and confusions in the dominant form of research practices. With a broad definition of crisis, I trace three periods of crises in psychology, namely the late 19th and early 20th century crisis about the possibility of psychology being a scientific discipline, the 1960s and 1970s crisis of methods and relevance and the replication crisis of the early 21st century.

In the late 19th and early 20th century there was no shortage of discussions among philosophers and psychologists challenging the possibility of psychology being a mature and scientific discipline. Although one can witness the emergence of proper object and subject matter of psychological inquiry in 19th century, in which Immanuel Kant's philosophy and criticism played

a decisive role, there was also a wide disagreement about whether the natural scientific method is the best approach to understand human psychology. The crisis revolved around whether psychology can take a scientific form at all (e.g., Kant, William James) or whether it should take a humanistic psychology road or natural scientific one (e.g., Lev Vygotsky).

A second disciplinary crisis erupted in the 1960s and 1970s in various branches of psychology, such as social psychology and psychiatry. Major criticisms arose in response to the domination of laboratory experimentation as the major and nearly sole research method in leading journals and textbooks (Faye 2012). There were many experiments showing how experimental findings were the artifact of experimental design, with subjects responding to experimenters' demands, treating experimental situation as a puzzle solving situation, and presenting a desirable version of themselves. Many psychologists at the time also questioned the ecological validity and representativeness of the experiments, especially because the experimental subjects were mostly undergraduate students and therefore not representative of population. Moreover, there were major questions regarding the social relevance of psychological findings, theoretical weaknesses of conceptual frameworks, and the status quo biases of psychological studies that were harmful to marginalized groups (e.g., Franz Fanon). Finally, there were questions regarding the spatiotemporal scope of psychological findings and how psychology had been an asocial, ahistorical, and a-cultural discipline (e.g., Kenneth Gergen).

The most recent crisis in psychology is the replication crisis which started with a case of data fabrication (Diederik Stapel Case in 2011, see Retraction Watch) and coupled with major studies pointing to widespread questionable research practices and problematic norms (e.g., publishing only significant results), and failed replication of studies published in major journals (Open Science Collaboration, 2015). In replication crisis, the major debates were about the influence of external factors (e.g., publication pressure, external incentives) and internal factors such as lack of check and balance for transparency and authenticity of data and research practices.

The three crises are somehow different. The cause of crises differs from philosophical debate, meta-scientific reflections, to failures of replication. In contrast to crises of 1960s and 1970s and replication crisis, the late 19th and early 20th century crisis was at the pre-paradigmatic state of psychology as a scientific discipline and the major questions were about its scientific status and direction (humanistic vs. objectivistic). In 1960s and 1970s it was mainly those at the margin of the discipline criticizing the discipline, however during the replication crisis it is the practitioners of mainstream psychology that recognized and debated the crisis (mainly the younger generations of psychologists).

There are however similarities between the three crises. The criticism of experimental design was common between the two recent crises. Although the only outcomes of 1960s-1970s crisis was the introduction of meta-analysis to the methodological toolbox of practitioners of mainstream psychology, methodological solutions have been more common after replication crisis (e.g., open science movement, pre-registration). There are continuities amongst the crises in terms of the theoretical/methodological positions that are taken. For instance, 1960s-1970s witness the proliferation of many new perspectives in psychology including discursive, feminist, postmodern,

decolonial, and critical perspectives in psychology, with a major shift to qualitative methods and similar understanding to humanistic psychology which some were advocating up until 1920s.

Although there are similar discussions across the three crises such as traces of similar controversies over mechanistic causality, role of theory, and the role of laboratory experimentations in producing psychological knowledge, the solutions to replication crisis seem to be dominated with methodological solutions. I think it is important to take the study of the history of crisis in psychology seriously with an emphasis on the possibilities that opened up for the discipline at every crisis moment but not realized, leading to return of similar questions.

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Credibility and trustworthiness: the twin promises of sociogenomic integration

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Session 13

The tools of genomics (such as the so-called polygenic scores) have recently been enthusiastically embraced in several social science disciplines, including psychology, sociology, economics, and social policy research. The integrative field of study emerging from this cross-disciplinary effort has been known as social science genomics, or sociogenomics. In this paper, I identify two main epistemic and non-epistemic promises that drive sociogenomic integration: credibility and trustworthiness. The perceived payoff of the integration for the social sciences consists in generating credibility for causal inferences about individual outcomes such as educational attainment or well-being. In turn, the involvement of social scientists in behavioural genetic and genomic research is thought to improve the trustworthiness of this latter area of science by countering its determinist and reductionist tendencies. Having thus characterised the main promises of sociogenomic integration, I proceed to argue that sociogenomic research does not succeed in realising them. This is because credible causal claims in the social sciences

require valid measurement as well as qualitative evidence - two methodological areas sociogenomics consistently neglects or de-prioritises. Moreover, the integration of social structural explanations does not provide as much pushback against genetic determinism or reductionism as is typically assumed.

*Introducing Philosophy:
investigating effective methods for explaining philosophy to social scientists*

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Session 13

The links between the social sciences and philosophy have always been close; each have contributed to the other at various points in history. Nevertheless, philosophical research is sometimes met with reticence, doubt, or downright scepticism from the side of social scientists, over and above the general tensions often associated with interdisciplinary collaboration. For ease, we call this philosophy-hesitancy. Given the importance of interdisciplinarity for philosophy, the social sciences and beyond (Higgins and Dyschkant 2014), philosophy-hesitancy is a real problem. Through a combination of analysis of the literature and qualitative empirical research, this project aims to deepen our understanding of the reservations that social scientists might have about philosophy in order to answer the question: How does one best explain the usefulness of philosophy to empirically oriented social scientists? We formulate and test the hypothesis that effectively addressing philosophy-hesitancy requires explaining not only the relevance, but also the reliability of philosophical research.

A root cause of philosophy-hesitancy, we think, is a lack of knowledge about the inner workings of philosophical research.¹ While philosophers often have an understanding, at least in broad terms, of the functioning of the empirical science they work with, social scientists rarely have a clear idea of the functioning of philosophy. This is not strange: the aims and methods of the empirical sciences are part of the philosophy (of science) curriculum, but most curricula in the empirical sciences do not cover the aims and methods of philosophical research. Especially when the resulting lack of knowledge mixes with popular misconceptions about philosophy, the emergence

¹ Social scientists have approached similar issues from a group-psychology perspective. See for example (Bolduc et al 2022; Klein 2005; Hemmings et al. 2013). Whilst useful, these studies tend to be very general, while our approach deals with what we perceive to be philosophy-specific challenges.

of philosophy-hesitancy is understandable: why should social scientists take seriously the products and representatives of a discipline which rarely engages in empirical research itself and seems far removed from their own ways of producing knowledge?

Much of the existing literature attempts to address philosophy-hesitancy by emphasising the relevance of knowledge produced by philosophy to the social sciences. We have distinguished three kinds of approaches. The first, exemplified by (Thagard 2009), strives to show the relevance of philosophy by emphasising the overlap in subject matter between an empirical science and some portion of philosophy. The second approach, exemplified by (Laplaine et al. 2019), attempts to show the relevance of philosophy for science by pointing at the historical record. The third approach attempts to explain the relationship between philosophical knowledge and scientific knowledge by means of metaphors. This approach is pursued at the end of (Thagard 2009), where references are made to Descartes' notion of philosophy as providing the foundations of science; to Peirce's notion of philosophy as a cable comprised of numerous fibres; and to Neurath's image of knowledge-construction as rebuilding a ship on the open sea.

While we agree that these approaches go some way toward showing the relevance of philosophy, we think that they only partially explain its usefulness for the social sciences. In particular, we find the approaches fail to explain why philosophical research and the knowledge it produces is trustworthy or reliable. If philosophers were told that clairvoyants consider philosophical issues part of their subject matter; that many philosophers have consulted clairvoyants in the past; and that the relationship between clairvoyance and philosophy can be described with metaphors, this shouldn't be a sufficient reason for philosophers to trust "knowledge" produced by clairvoyants. This would require explaining how clairvoyants produce their "knowledge" in a way that meshes with the basic epistemic beliefs philosophers have about reliable knowledge-production. Thus, we hypothesise that existing approaches lack a discussion of the inner workings of philosophical research that directly deals with the methods philosophers use and how they mesh with other methods for producing knowledge used in the social sciences. On this analysis, a more comprehensive explanation of philosophy's usefulness will address both relevance and reliability if it is to diffuse philosophy-hesitancy effectively.

This project aims to rectify the perceived shortcomings of existing approaches to explaining the usefulness of philosophy by focusing on how we should communicate about the reliability of philosophical research with social scientists. It proceeds in two phases.

In Phase I, we develop an account of philosophical methods that centres around two notions. The first, reasoned arguments, we break down into 1) conceptual analysis; 2) logical reasoning; and 3) genealogical analysis. For the second, we adopt (Hájek 2014)'s notion of philosophical heuristics. This account is specifically designed to emphasise the reliability of philosophical methods by stressing their continuity with the methods of the social sciences.

In Phase II, we will empirically investigate both the accuracy of our analysis of the problem of philosophy-hesitancy and the effectiveness of the solution we developed in Phase I. More specifically, we will conduct a survey and focus groups with participants drawn from a large

interdisciplinary collaboration between a number of social sciences and philosophy based in the Netherlands.

The empirical portion of the project will take place in April 2024. At the ENPOSS conference we will therefore present: the background and motivation behind the project, our analysis of the problem, the setup and results of our empirical work, and our conclusions.

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Overcoming Index Number Monism: from Harberler and Neurath to the 21st Century

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Session 14

Index numbers mesmerize the public, in times of galloping inflation even more so. They are thought to have the capacity to condense complex phenomena to a single numeral in a scientifically objective manner and to order seemingly incommensurable phenomena, as well as to justify policies. Recent criticisms have spotlighted misuses and pitfalls of quantification in general (Newfield et al 2023), and of the underestimated role of value judgements in the creation, calculation, and application of price indices in particular (Reiss 2008, 2022). Despite these criticisms and increasing heterogeneity of lifestyles and consumer choices, mainstream economics, public discourse, and policy are by and large bogged down to a single, allegedly encompassing consumer price index (for recent developments see e.g. Schultze & Mackie 2002, Sichel & Mackie 2022).

Aiming to illuminate the persistence of “the” standard consumer price index, “the” GDP, and other unique indices, this paper combines a historical (I), a systematic philosophical (II), and a political practical perspective.

(I) We rationally reconstruct early criticisms of index numbers and how they are used in practice, and we show that these concerns anticipated many contemporary demurs. Given almost a century of immunity to such critical arguments, it is not surprising that piling up and refining further similar objections is unlikely to trigger fundamental change in the scientific or political sphere.

In the cautionary spirit of Viennese Late Enlightenment, Austrian economists Gottfried Haberler (1927) and logical empiricist Otto Neurath (1910, 1911, 1937, 1939) critically discussed the limitations of index numbers right from the start. Their own constructive contributions impress by their highly modern pluralism and contextualism: a meaningful assessment of “the standard of living” or of “the price level” is dependent on the context and purpose of the inquiry. According to Haberler and in line with Neurath’s critique of “pseudorationalism”, a single price index can never satisfy all our epistemic needs. Yet, Neurath is even more skeptical and thinks that meaning- and useful index numbers cannot be defined for many contexts of interest. Both Haberler and Neurath

reject talk of “the one true price level” as an entity which is supposedly independent of any well-defined index.

(II) Contrary to many contemporary philosophers of social science, Haberler and Neurath upheld versions of a fact-value dichotomy. Having said that, they certainly acknowledged that value judgements enter deliberations and actions involving index numbers in various ways. We will differentiate and discuss the different types of value judgements which enter the process at different stages, from the identification of a relevant theoretical or practical problem, via the definition of a particular index concept, its operationalization, the collection of data, up until the use of the index in the justification, implementation, or assessment of a policy. Ideally, the resulting input-output matrix allows for assertions of the form: “the input of value judgements a₃, b₉, c₂ yields the index concept i₇”; “the input of value judgements a₆, b₉, c₁ yields the index concept i₄”.

Subsequently, we employ our analysis in a twofold manner: First, we examine the philosophical question whether a fact-value dichotomy in line with Haberler’s and Neurath’s positions is defensible in light of strong contemporary claims of fact-value entanglement.

(III) Second, in a political practical outlook, we sketch a strategy for promoting a plurality of indices. Arguably, one of the reasons for the persistence of a single prize index and index number monism in general is the multifariousness of actual and hypothetical alternatives. Indecisiveness or disagreement which of the many options to choose for a particular purpose can paralyze further action. The need to use some index and the lack of guidelines for a choice motivates a prompt relapse to the established default option. Based on our input-output matrix, we sketch the idea for an automated tool which asks the operator to enter several value judgements and provides an - not ‘the’ - adequate index concept in response. Admittedly, the adequacy of an index concept for a specific purpose will often remain a matter of contention among methodologists. The proposed tool would, however, fulfill the practical need for a quick decision for an index concept and at the same time pay heed to many old and new criticisms. In particular, the dependency of an adequate index number on its purpose would be highlighted for every practitioner and an, albeit limited, plurality of index numbers would be promoted.

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