PHILOSOPHY OF SOCIAL SCIENCE ROUNDTABLE
EUROPEAN NETWORK FOR PHILOSOPHY OF SOCIAL SCIENCE
2015 JOINT MEETING - ABSTRACTS

Keynote speakers

Abigail Stewart (University of Michigan): **Judging Others in the Academy: Implications of Uncertainty and Bias**

Abstract: In this talk I will focus on social science research suggesting that there are two major reasons for us to have far less confidence than we do in our judgments of one another in the academy: (1) the uncertainty associated with forecasting future behavior; and (2) evidence that our judgments are based on many factors other than the criteria we believe are relevant and appropriate. While these other factors are usually discussed as reflecting “bias,” I believe that term does not adequately summarize the several known threats to the validity of our judgments of each other. I will also outline what is known about how to reduce these threats to validity in a variety of contexts in which we judge one another: faculty hiring, faculty evaluation for tenure and promotion, assessment of grant applications, and selection of individuals for a variety of honors.

Biography: Abigail Stewart is Sandra Schwartz Tangri Distinguished University Professor in Psychology and Women’s Studies, and Director of the ADVANCE Program at the University of Michigan. She has published many scholarly articles and several books focusing on feminist theory and the psychology of women’s lives, personality, and adaptation to personal and social change. Her current research, which combines qualitative and quantitative methods, includes the comparative analyses of longitudinal studies of educated women’s lives and personalities; a collaborative study of race, gender and generation in the graduates of a Midwest high school; and research on gender and science and technology with middle-school-age girls, undergraduate students, and faculty. She is past Director of the Women’s Studies Program and the founding Director of the Institute for Research on Women and Gender. As Director of U-M ADVANCE she is involved in a multi-level program designed to improve the campus environment for all faculty—particularly women and underrepresented minorities-- in terms of recruitment, retention, climate and leadership.

William Wimsatt (University of Chicago and University of Minnesota): **Scaffolding and Entrenchment in Cultural Evolution**

Abstract: *Scaffolding* occurs when a structure or behavior is utilized to make possible, easier, or faster the attainment of a goal. Generative *entrenchment* is the building of dependent structures, processes, or behaviors (SPBs) on earlier SPBs in a way that facilitates their addition to (and scaffolding) an adaptive structure. This makes the primary SPBs more essential, their loss more severe in its effects, and thus makes entrenched elements more conservative in evolutionary processes. Scaffolding and entrenchment are endemic to the evolution of complex systems, in biology, technology, cognition, and culture. They have many consequences, from generating phylogenies, to making history relevant, to affecting more and less likely directions of evolutionary change. They play a role in the evolution of institutions, standards, and conventions. Wimsatt discusses how development and culturally induced population structure generate important differences between biological and cultural evolution and how technology, organizations, and institutions provide both targets for cultural change and formative influences on our cognitive development.

Biography: William Wimsatt is Winton Chair of Liberal Arts at the University of Minnesota and Ritzma Professor of Philosophy, Evolutionary Biology, and Conceptual and Historical Studies of Science emeritus at the University of Chicago. He studies the inexact and historical sciences, mathematical modeling, heuristics and biases, mechanistic explanations, complex systems, and the history of genetics and evolution. He applies evolutionary developmental biology to cultural evolution, particularly how norms, technology, social institutions, and science scaffold development and maintenance of knowledge and practice. His work helped make philosophy of biology a central focus in philosophy of science. *Re-Engineering Philosophy for Limited Beings: Piecewise Approximations to Reality* (Harvard, 2007) addresses these themes, as do the coedited volumes, *Characterizing the Robustness of Science* (Springer, 2012), and *Developing Scaffolds in Evolution, Culture and Cognition* (MIT, 2013).

Author(s): Marshall Abrams (University of Alabama, Birmingham)
Title: Where Does Cultural Coherence Come From? Modeling the Coevolution of Religion and Coordination in Balinese Rice Farming
Affiliation: University of Alabama, Birmingham
Email: mabrams@uab.edu

Abstract: There has been a great deal of modeling work on cultural evolution and cooperation, much of it focused on abstract models not derived from particular social contexts (Boyd, Richerson, Feldman, Enquist, Laland, Strimling, etc.), and most of it focused on simple models of culture that do not try to address complex interactions between cultural variants and between culture and environmental factors. In a previous publication (2013), I argued that extending common scientific methods to include more of what I call "moderate-complexity" agent-based models can allow deeper understanding of patterns of change across human populations. Such models represent populations as collections of so-called "agents—simplified abstractions of persons—each containing a simplified model of a few cognitive processes. The moderate-complexity strategy takes inspiration from, but differs from modeling in evolutionary biology and social sciences that represent each individual or collection by a few simple variables, and from AI modeling in which agents contain complex models of cognitive processes. Here I provide further justification for the thesis that moderate-complexity simulations can incorporate insights from humanistic social science into scientific research, extending existing social science models involving niche construction, and showing how Balinese religious symbolism and cooperative coordination of irrigation might have coevolved and spread.

Using a computer model of success-biased cultural transmission on a network, Lansing and Kremer (1993 and other publications) showed how sophisticated, cooperative coordination of planting schedules for sustainable water use and pest control can emerge from local decisions by Balinese rice farmers. Success-biased transmission occurs when individuals are more likely to learn from those who are perceived to be successful in some particular respect. In this case, "success" consists of having high rice production. In Lansing and Kremer's model, niche construction involving the effects of crops and pests on rice production generates feedback on decisions that lead to local pockets of similarity and difference in farmer's planting schedules.

However, Lansing (2006) later described psychological and cultural influences that routinely disrupt such cooperation. Lansing (2006) also described religious beliefs and practices that tend to suppress such disruption. How is it that some aspects of Balinese religious patterns came to have a character that seems tailored toward maintenance of cooperative coordination across large areas? These cultural patterns appear to differ from earlier Javanese patterns from which they are partly derived, and from contemporaneous religious practices in other nearby regions in Bali. Like much work in anthropology, Lansing's work did not offer a specific proposal of how this kind of cultural "coherence" came about. D.S. Wilson (2002) suggested that the religious patterns that Lansing described spread through an evolution-like process, but Wilson did not hint at what this process was like. I propose that religious practices among rice growers in Bali, like planting practices, spread through cultural transmission biased by success in the sense of greater rice production: Religious practices that tended to encourage successful cooperation with other rice farmers spread because those who engaged in such practices were more successful at growing rice.

Further, I explore the idea that analogies between religious beliefs and patterns of behavior directly relevant to rice farming practices encouraged those in farming communities to identify religious and practical dimensions. Analogies are relevant here in two respects: First, analogies between religious ideas and certain patterns of social interaction—rather than purely random cultural variation—provide part of the material on which the selective process of success-biased transmission can operate. Second, it's in part via such analogies, I suggest, that religious practices were effective at influencing practices that affect coordination in rice farming.

In order to explore these hypotheses, I first extend Janssen's (2007, 2013) version of Lansing and Kremer's model by showing how disruptive behaviors can interfere with the evolution of beneficial coordination of planting patterns. I then add a second channel of cultural transmission of "religious" values modeled in a very simple way: Religious patterns range from 0 (no effect on disruptive behavior) to 1 (maximum suppression of disruptive behavior). I show how religious values that tend to suppress disruptive behaviors can spread as a result of restoring beneficial planting coordination. This set of models illustrates, in the context of Balinese rice farming, a general process by which cultural patterns can spread because they have indirect social effects with practical benefits.

I then provide a more complex model of religious patterns by linking the preceding models to a model of the role of analogy in cultural transmission (cf. my 2013). In this model, some sets of religious beliefs are made "plausible"—made more likely to be believed—if they fit into analogies involving existing beliefs about practices connected to rice growing. This provides a model of the transmission of structured religious beliefs between individuals: religious beliefs that fit into analogies with existing beliefs are reinforced. Success bias has the effect of further reinforcing those sets of religious beliefs that, through their role in analogies to certain social practices, tend to suppress social disruption in the coordination of planting patterns. While the first set of models illustrates a general process by which any cultural patterns that suppress disruption to beneficial cooperation might spread, this set of models provides a model of the way in which particular Balinese religious patterns may have spread because of their role in suppressing disruption of the coordination of planting patterns.

I argue that my models illustrate, a number of levels of generality, ways in which cultural patterns interact and are selected for because of how they support outcomes that are desirable for other reasons. I further argue that these models illustrate the possibility of a greater incorporation of insights from humanistic and other qualitative research into models of population-level patterns of change over time.

Author(s): Francesco Di Iorio
Title: The Two Reductionist Interpretations of Methodological Individualism
Affiliation: ESCP Europe, Paris France, and Luiss University, Italy
Email: francedi.iorio@gmail.com

Abstract: There is no univocally accepted definition of methodological Individualism. According to the most common view, methodological individualism is a reductionist program because it conceives society in atomistic terms and neglects the structural constraints which influence action. I challenge this definition because there is no equivalence between methodological individualism and atomism: the latter is only a variant of the former – its most simplistic variant, to be precise. Two coexisting approaches can be distinguished within the tradition of methodological individualism: one is atomistic, whereas the other is non-atomistic. The atomistic approach is employed by mainstream economics, and it is the foundation of the social contract theory. By contrast, the non-atomistic approach focuses on the real historical and socio-cultural presuppositions of action and social order, and it admits the existence of various structural constraints which affects human conduct (e.g. Tocqueville, Weber, Simmel, Menger, Hayek, Popper, Boudon, Elster).

I criticize the widespread view that the entire individualist tradition denies or belittles the effects of social conditioning. This view is often expressed by saying that this tradition is committed to reductionism, a term that is not univocally defined by critics of methodological individualism. Two variants of this approach can be distinguished both of which are discussed and criticized. The first (e.g. Bhaskar; Udehn) considers the individualist paradigm to be a form of idealist reductionism that denies the reality of social conditioning. The individualist theory of social systems, which is linked to a nominalist ontology and an inter-subjective theory of the social world, is interpreted as the idea that social constraints are nothing but mental constructs – pure opinion – to which no real limitations correspond. In other words, methodological individualism is accused of not understanding that social constraints are real and objective in the sense that they exist independently of the agent's opinion about what he or she is free or not free to do.

In my opinion, the interpretation of methodological individualism in terms of idealism misunderstands the interpretative perspective (Verstehen) of methodological individualism. The Verstehen approach explains social conditioning as a product of common understandings, i.e. of what Boudon calls "collective beliefs". This approach does not reduce sociology to the study of purely personal and subjective opinions. Methodological individualism is not interested in how a particular individual interprets his/her social environment, but focuses on intersubjectivity. It acknowledges that thinking differently about the social world does not alter the social world for an individual because the social world is not the product of a particular mind, but rather the largely unintentional consequence of intersubjectively shared meanings. From the viewpoint of methodological individualism, social conditioning exists and must be explained in terms of both collective beliefs and aggregate unintended effects.

The second interpretation of methodological individualism which I criticize is the interpetaion in terms of semantic reductionism (e.g. Kincaid; Sawyer), developed within analytic philosophy. According to this interpretation, the individualist idea that social systems and their mechanisms must be explained in terms of individuals is supportive of a principle of semantic reduction of social properties to individual ones – a principle which must be rejected because, since social phenomena are systemic phenomena, they cannot be analyzed without remainder to semantically irreducible concepts and laws. Moreover, this second variant levels the accusation that methodological individualism denies that semantically irreducible properties of the social system causally influence action and limit individual freedom of choice.
I criticize the interpretation of methodological individualism in terms of semantic reductionism. The explanations in terms of methodological individualism are semantically irreducible to individual laws (namely to psychological laws) because they acknowledge (i) the existence of unintended consequences of action and (ii) the existence of social factors that causally influence action. For instance, Menger and Hayek assume that there are factors such as the market prices (which are semantically irreducible to psychological laws because they unintentionally emerge from the combination of different individual evaluations) that causally influence action. There is a circular causality between individual choices and market prices: choices influence prices and the latter influence in turn choices. According to Menger's and Hayek's explanation of the market in terms of invisible hand, market prices are supposed to coordinate economic activities because they affect and limit the freedom of choice of individuals.

In my opinion, the authors who argue the equivalence between methodological individualism and semantic reductionism misunderstand the meaning of the individualist criticism of the substantialist theory of social wholes. On developing their criticism of holism, methodological individualists often used sentences like "Only individuals exist, thus we must explain social phenomena only in terms of individuals". Sentences of this type have been wrongly interpreted as proving the commitment of methodological individualism to semantic reductionism. However, sentences like this, do not mean that the use of semantically irreducible concepts and laws must be avoided. They mean something different: i.e. that the holist view that the social world consists of supra-individual substances, which exist independently of individuals and control them, must be rejected. There is a difference between the concept of wholes as independent substances which control individuals (a concept which is criticized by the individualists) and the concept of wholes as semantically irreducible entities (a concept which is accepted by the individualists) – a difference which seems to escape those authors who consider methodological individualism to be naïvely committed to semantic reductionism. The individualist rejection of the idea that 'wholes' are independent substances does not imply any endorsement of semantic reductionism. Similarly, there is a difference between the concept of holistic sociological laws – which assumes that the individual is an heteronomous being and describes the behavior of independent social substances which mechanically determine the behavior of its human emanations and the concept of irreducible laws in the sense of laws which are semantically irreducible to individual laws. Non-atomistic methodological individualism rejects the former kind of laws. Conversely, it has nothing to say against the latter, which it considers to be the foundations of the invisible-hand explanation theory.

Author(s): Matt Drabek
Title: Social Practices as Embodied and Embedded Systems of Classification
Affiliation: University of Iowa
Email: matt.drabek@gmail.com

There has been a renewal of debate in recent years within the philosophy of social sciences over the nature of social practices. This debate has operated largely on the premise within a large block of social scientific and philosophical literature, dating to the work of social scientists and theorists like Pierre Bordieu and Sherry Ortner, that social practices are either a fundamental social phenomenon or an explanatory construct fundamental to theories of social life. The goal of this paper is to present a conception of social practices as embodied systems of classification of human activity that are embedded in everyday interactions between people.

The point of departure for this paper rests on the recent philosophical work of Stephen Turner, a critic of conceptions of social practices, and Theodore Schatzki, a defender of a 'normativist' approach to practices. I will use the work of Turner to suggest that a wide variety of ways of thinking about practices, namely the positing of practices as social forces or analogical entities, is mistaken. I will then concur with Schatzki that an important alternative way of thinking about practices is in terms of shared meaning at a site or setting. But I will argue against the central Schatzkian view that practices are grounded in normative authority (a view Turner also criticizes).

'Practices' is a multiply ambiguous word. It picks out things people do alone and things people do in groups. It picks out things people cultivate and develop and things people do nonchalantly or as a matter of course. One key feature of practices, as I conceive them, is that they are shared or distributed across people at a site. A site incorporates the people, physical infrastructure, and social expectations found in a particular time and place. This distribution is realized in the similarities found in the things people do. A second key feature is that people experience practices as having appropriate and inappropriate ways of being carried out and appropriate and inappropriate responses to them that people can provide. People experience practices as having a normative structure, expressed through these experiences of appropriateness and inappropriateness. Practices have two key components. First, they are activities people carry out. But they are not mere activities. They are set apart from activities by their second component, namely that people experience practices in particular ways at a site. Taken together, a practice is an activity that has standard or typical experiences and responses when encountered at a site. There is a range of ways people can do something at the site, and they know how to take and respond to the activities as a matter of course.

The final task of this paper is to address the charge, first articulated by Turner, that conceptions of social practice are relativistic. I do so by distinguishing between two forms of relativism, normative practice-relativism, a view I ascribe to Schatzki, and descriptive practice-relativism, a view I defend. The task, then, is not to defend my conception of social practices against the charge of relativism, but rather to argue that the version of relativism I accept is an unproblematic version. Normative practice-relativism is a view that relativizes practices to normativity, a concept that Turner rightly points out is slippery and unclear. Normativity requires its own novel explanation or theory. Attributions of normativity run into trouble in situations where there is a gap between what one is invited to do by a situation and what one ought to do in that situation. Descriptive practice-relativism, by contrast, is a view that makes no claim about the normative status of social practices and treats practices as a dependent variable. Rather than relativize practices to normativity, I prefer to relativize them directly to the material and social environments in which they are carried out. This view recognizes that the material and social environment produces standard ways of using or carrying out social activities. But it does so without thereby attributing normative authority to these environments. It allows for the possibility of questioning and reshaping those environments.

My preferred relativistic view incorporates a delicate balancing act between the underlying stability in the things people do and the seemingly contingent nature of human activities. The philosopher Ludwig Wittgenstein once discussed the notion of Spiel, which is quite helpful toward getting at this interplay. The German word 'Spiel' is typically translated into English as 'game.' Wittgenstein famously used the notion in his discussion of Sprachspiele, or language-games. But Spiel can be something much more broad and open-ended than a game. It is something more like the English word 'play,' a word picking out a broader range of activities than 'game'. While a game is usually a set of activities governed by constitutive rules, play also incorporates activities that are creative or spontaneous, as well as activities that are not directed toward set ends or goals.

While I will not have the time to explore these broader issues at length in this paper, the conception of social practices I defend is the foundation of a much broader project in social philosophy. The goal of this broader project is to get at how classifications of people and their activities can marginalize them. Getting clear on how practices function as embodied classifications is an early step in that project.

Author(s): Brian Epstein
Title: How many kinds of glue hold the social world together?
Affiliation: Tufts University
Email: brian.epstein@tufts.edu

Abstract: Among the most useful skills we have, as humans, is our ability to anchor new social kinds. We do this routinely. The furniture of today's world includes brands like Nike, Budweiser, and Blackberry; financial instruments like variable annuities, CDOs, and swaptions; technologies like screwdrivers, smartphones, and web services; dances like the lindy hop, jitterbug, and krump; textiles like gabardine, herringbone, and bouclé, subcultures like hipster, gopnik, and cybergoth; jobs like professor, President, barista, and climatologist; and so on. All of these are social creations, populating the world more densely than it once was before their introduction.

Many philosophers endorse what Guala has called "the Standard Model of Social Ontology," i.e., the view that social kinds exist in virtue of our having a particular sort of reflexive collective attitude. (The most widely held view is Searle's, taking institutional facts to be put in place by our holding collective acceptance (or recognition) attitudes toward constitutive rules.) Other philosophers have proposed less intellectualist pictures, in which properties of tokens in the world — not just our attitudes — figure into determining the instantiation conditions of social kinds. (E.g., Millikan 1984, Elder 2009).

In this paper, I argue against the idea that social kinds are anchored in one uniform way. Rather, there is a variety of "anchoring schemas" by which new social kinds are generated. I explain the distinction between a kind's instantiation conditions and the anchors for that kind's instantiation conditions — i.e., the facts that determine that the kind's instantiation conditions are what they are. I then consider the materials available for anchoring social kinds in different circumstances.

I describe three different social kinds, each with a different sort of instantiation conditions. One kind is plausibly "teleofunctional" in Millikan's sense, with historical grounding conditions. The second kind has, as its instantiation conditions, performance of a causal role and possession of certain qualitative characteristics. And the third kind has purely qualitative instantiation conditions. Yet all three of these have similar reproductive histories, with slight variations. I argue that each of these kinds, in being anchored as it is, makes very different use of reproduced tokens, their relations to one another, and other features of the environment.

Millikan has argued that the "glue" holding together kinds like the latter are not "sticky enough" to support inductive generalizations. I show that her criticism is misplaced, and indeed, that even kinds anchored according to her favored story are not guaranteed to support inductive generalizations. Rather, the normal success of any given anchoring schema depends on contingent regularities in the environment.

These observations about anchoring schemas are only the first step in reconstructing a social ontology free of commitment to one "secret sauce" that makes the social world exist. An inquiry into the anchoring of the social world, I suggest, might better begin with broad investigation of diverse cases of social kinds, and investigation into the purposes social kinds may play. With these, we have a better hope of finding the various practical schemas by which social objects, properties, and kinds are set up, such that they — as a practical matter — tend to fill their roles and purposes.

Author(s): Roberto Fumagalli
Title: Economics, Psychology and the Unity of the Decision Sciences

Affiliation(s): University of Bayreuth and London School of Economics
Email: R.Fumagalli@lse.ac.uk

Abstract: The philosophical and methodological literature on the relationship between economics and neuro-psychology has grown remarkably during the last two decades (e.g. Bruni and Sugden, 2007, Hausman, 2008, Kahneman et al., 1997, Rabin, 1998). The proposed accounts differ in their details, but frequently reconstruct this relationship in terms of the following three-stage narrative (henceforth, the 'standard view'). First, we find a period of unity, going approximately from the marginal revolution in the 1870s till the 1910s, during which neoclassical economic theory was grounded on psycho-physiological foundations. Second, there is a phase of progressive separation, prompted by the emergence of ordinal utility theory and revealed preference theory, culminated in the 1950s with the elimination of psychological constructs from economic theory. Finally, an ongoing process of reunification between economics and neuro-psychology builds on advances in experimental, behavioural and neuroeconomics to construct a unified interdisciplinary framework for modelling choice.

In this paper, I draw on recent developments in philosophy of science, economic methodology and neuro-psychological research to provide a descriptive and normative critique of this standard view. Moreover, I put forward a reconstruction of the relationship between economics and neuro-psychology that, I claim, better fits both the available historical evidence and the methodological foundations of these disciplines. I shall argue for the following three claims. First, contrary to the standard view, neoclassical economic theory was not grounded on psycho-physiological foundations. Second, the standard view significantly overstates economics' alleged separation from psychology and implausibly downplays the role psychological findings have played in 20th century economic theorizing. And third, the increasing integration between economics and neuro-psychology falls short of licensing the standard view's claim that a reunification between these disciplines is under way and should be implemented.

The contents are organized in three main sections as follows:

1) Section 1 outlines the standard view's reconstruction of the relationship between economics and neuro-psychology, focusing on three research programs' purported contribution to the reunification between these disciplines. First, behavioural economics is claimed to reunify economics and psychology by improving the realism of the psychological assumptions underlying economic theory (e.g. Camerer, 1999). Second, experimental economics vastly increases the range of experimental methods and findings available to economists (e.g. Guala, 2005). Finally, neuroeconomics provides economists with unprecedented means to observe and causally manipulate the neural substrates of choice. The proponents of neuroeconomics aim not merely to build more predictive and explanatory models of choice, but also to develop "a single unified [theory] of human decision making" (Glimcher, 2011, 4). This, in turn, is said to "complete the research program [of] the early classics" (Rustichini, 2005, 203) and fulfil Jevons' and Edgeworth's ambition to "reground economic behaviour in [...] cognitive neuroscience" (Quartz, 2008, 460).

2) Section 2 draws on historical and methodological considerations to articulate a constructive critique of the standard view. The proponents of the standard view persuasively argue that several neoclassical economists made substantive psychological assumptions in their works (e.g. that economic agents are motivated by the desire for wealth). This, however, implies neither that "the psychology of sensation was an essential part of economics" (Bruni and Sugden, 2007, 154) nor that "economics and psychology are essentially siblings separated at birth" (Loewenstein et al., 2008, 648). Indeed, leading economists of the time remarked that neoclassical economic theory does not rest on any psychological presuppositions (e.g. Hands, 2010, for a review).

Concerning the putative separation between economics and psychology, the development of ordinal utility theory and revealed preference theory did enable economists to represent consistent choice patterns without making psychological assumptions. This, however, falls short of implying that psychological findings played no role in economic theory. In fact, psychological findings figured prominently in such theory (e.g. Lehtinen and Kuorikoski, 2007, on rational choice theory's applications that rely on psychological assumptions). As to the purported reunification between economics and neuro-psychology, researchers have made promising integrative advances, ranging from the incorporation of psychological insights into economic models (e.g. Loomes and Sugden, 1982) to attempts to link neuroscientific measurements and observed choices through rigorous axiomatic statements (e.g. Caplin et al., 2010). Still, it is one thing to claim that fruitful integrations have taken place between economics and neuro-psychology. It is quite another thing to maintain that such integrations foster a reunification between these disciplines (XXX). In this respect, the challenge for the proponents of the standard view is to specify why exactly the ongoing integrative advances would constitute genuine unification - as opposed to local integrations - between economics and neuro-psychology.

3) Suppose, for the sake of argument, that the standard view provides a plausible reconstruction of the relationship between economics and neuro-psychology. Even so, there are several reasons to question the project of developing a unified interdisciplinary framework for modelling choice (XXX). In Section 3, I examine and rebut two claims that influential authors made in support of interdisciplinary unification. The first claim is that economists have failed to provide precise and plausible criteria to demarcate the domain of economic theory from that of other decision sciences (e.g. Bruni and Sugden, 2007, Lewin, 1996, Rosenberg, 1992, ch.4). The second claim criticizes economists for assuming that agents' preferences satisfy strong consistency axioms without testing such axioms against the empirical evidence (e.g. Bruni and Sugden, 2007, Sen, 1973, Sugden, 1991).

In addressing these two claims, I argue that recent calls to develop a unified interdisciplinary framework for modelling choice rest on disputable presuppositions concerning both the domain of economic theory and the relationship between this theory's axiomatic foundations and empirical findings about the neuro-psychological substrates of choice. My contribution aims to advance the ongoing reflection on the relationship between economics and neuro-psychology in three respects of general interest to the philosophers of these disciplines. First, it builds on the specialized historical literature to provide a constructive critique of prominent accounts of such relationship. Second, it clears the ground of often-made criticisms of economic theory that rest on questionable evidential and methodological assumptions. And third, it paves the way for a more systematic understanding of the philosophical interconnections between economics and other decision sciences.

Author(s): Elihu Gerson
Title: Institutions and Repertoires: Capacity Concepts in the Analysis of Social Organization
Affiliation: Tremont Research Institute
Email: emg@tremontresearch.org

The notion of institution is an important idea in the social sciences, but there have been few attempts to consider it from a philosophical perspective. There is broad agreement on a loose sense of the term: institutions are systems of conventional practices, more-or-less regular ways of doing things, what Everett Hughes called "a collective enterprise carried on in a somewhat established and expected way" (Hughes 1942: 307). "Established and expected" means that there's a usual or conventional way of doing something, and doing it some other way is likely to bring attention. "Somewhat" means things aren't always done the usual way; sometimes, somebody does it a different way, accidently or deliberately. Sometimes this difference is treated as deviance to be corrected; sometimes it's ignored; sometimes, people adopt the variant, modifying, and incorporating it into the way they do things. Sometimes, the variant spreads from person to person, organization to organization, place to place.

Institutions are thus malleable, but they are not completely fluid. Yet social scientists have no general ways of dealing with a series of important questions, for example: How can institutions be characterized in a way that expresses and retains the imperfect regularity at their core? How can we frame the relationship between individual actors (organizations and individual people) on the one hand, and institutions on the other? How do established ways of doing things get established, and how do they change?

This paper suggests that these questions can be addressed by dividing the notions of institution (including such cognate notions as "convention", "repertoire", "practice", "protocol", "template", "schema", paradigm", and similar ideas) into two separate categories, which I will call institutions and repertoires. Both can be defined as the collective capacity of actors to carry out some group of related activities. These collective capacities are constituted in part by particular skills, and in part by relevant common knowledge; that is, each participating actor knows that the other participants know certain relevant facts, assumptions and procedures.

Repertoires are capacities of a particular group of people, an organization, who are working together formally or informally at specific times and places. The activities they conduct are the tasks of the organization (which may be small, local, informal, and temporary). Participants vary in their capacity to perform the tasks of a repertoire. Some are not (yet) equipped to do so, some are immature, ill, criminal, reformers, or innovative. Differences in immediate circumstances mean that individual performances of the same repertoire vary from occasion to occasion, and this variation (some of which is error, some of which is "within tolerance", some of which is innovation) provides the raw material of institutional change.

Institutions, like repertoires, are concrete, not abstractions. They have start dates, end dates, and articulated parts (their common knowledge). On the other hand, institutions are desituated; the activities they specify are stripped of characteristics relating to immediate circumstance. For example, a work of music can be performed by many different groups without changing its melody. The groups have certain works in their repertoires, but the works exist independently of the groups as long as there are some players that can perform them. The concrete, material character of institutions resides in this capacity for potential enactment: if some person or group can perform them in ways that have material consequences, then they are material. Institutions often entail one another; e.g., performing musical works means that someone is building and distributing instruments that can be played in conventional ways, that suitable spaces are available, and so forth. On the other hand, the arrangements for linking complementary institutions (e.g., markets, web sites), and the broadly distributed common knowledges that goes with them, can often eliminate or sharply reduce the need for direct coordination among participants. This is an important way that institutions differ sharply from repertoires, which typically require significant coordinative interaction among situated participants. We can all learn our separate parts at home, but effective performance requires us to rehearse together.

The repertoires of particular organizations perform multiple institutions at different times. For example, complex organizations such as universities can have multiple near-independent repertoires (e.g., disciplines), each performed in different departments. Thus, organizations often have many institutions in their repertoires, and each institution is performed in multiple organizations. Because of these overlaps, performances can interact and affect one another, directly or indirectly. For example, administrative regulations may allow one department of an organization to consume a disproportionate share of available resources, thus affecting the performance of other departments.
It is the interaction among repertoires and institutions that gives social order its peculiar character of durable malleability. Together, institutions and repertoires perform general and standard protocols in specific settings with local characteristics. In musical concerts, for example, the scales, melodies, and instrument designs are institutionalized, while the choice and style of works performed is part of the local repertoire. A given performance is not complete without both, but performances vary within limits set, in part, by each. Institutions and repertoires are integrated over multiple performances by their overlapping and complementary, but independent, common knowledges.

The distinguished concepts of repertoire and institution support multi-level analysis of both particular situations and their participants on the one hand, and the development, diffusion, and impact of generalized forms of conduct on the other. They thus promise to support an improved understanding of institutional formation and change.

REFERENCE: Hughes, E. C. 1942. "The study of institutions". *Social Forces* 20: 307-310. Reprinted in Hughes, E. C. 1970. *The Sociological Eye: Selected Papers*. Chicago: Aldine.

Author(s): Matt Heinonen
Title: Minimalism and Maximalism in the Study of Shared Agency
Affiliation(s): University of Helsinki and CUNY Graduate Center
Email: matti.heinonen@helsinki.fi

Abstract: In recent years, philosophers such as Stephen Butterfill (2012), Elisabeth Pacherie (2013) and Deborah Tollefsen (2005) have argued that standard philosophical accounts of shared agency cannot be used to provide an adequate account of the shared activities of young human infants, because infants do not have the cognitive resources that are needed for framing shared intentions. In particular, infants do not seem to have the meta-representational capacities that are needed for formulating beliefs and intentions about the intentional states of other agents. Accordingly, several philosophers have set out in search of minimalist accounts of shared agency, which impose more modest cognitive demands on agents, who act together, than standard philosophical accounts. Minimalist philosophers of shared agency have often contrasted their views with maximalist accounts, which presuppose that the involved agents have developed mindreading skills, and which in some instances also involve reference to irreducible group agents (Gilbert 2013; Tuomela 2013) or intentional state types (Searle 1990). In my paper, I will concentrate on the contrast between minimalists and Michael Bratman's account of shared agency—which is at the less demanding end of the maximalist spectrum, and has been discussed in detail by several philosophers of shared agency, who have themselves put forth minimalist accounts.

While I agree that the psychological data that minimalists about shared agency appeal to is relatively compelling, I argue that their challenge against the adequacy of Bratman's account is based on a misunderstanding of the meta-theoretical status of his account. To counter their criticisms, I will argue that Bratman's account of shared agency is best understood as a functional model of shared agency, which involves very few or no specific commitments about the psychological mechanisms that are operative in bringing about shared behavior. By contrast, some of the models that minimalists about shared agency have put forth can be understood as mechanistic models of the psychological processes that fill out the functional roles that Bratman's account specifies on a greater level of abstraction. Seen in this light, the two kinds of models can be regarded as complementary, rather than as antagonistic to one another. Furthermore, this distinction provides us with new directions on how to proceed from the present standstill in the philosophical debate on shared agency by providing a feasible platform for interdisciplinary research on the nature of shared agency.

To elaborate on the contrast between functional and mechanistic models of shared agency, I will distinguish between model templates, model profiles and ontological construals. A model template (in the study of shared agency) consists of an abstract description of a set of (psychological) states, their relations and properties. Mechanistic models characteristically involve more fine-grained model templates than functional models, and they are defined in relation to particular functional patterns of behavior. A model profile consists of an ascription of a set of particular psychological states to particular agents, conforming to the structure specified in the model template. Specifying suitable model profiles is an indispensable intermediary stage in applying models of shared agency to the explanation and understanding of particular behavioral occurrences. However, model templates can also be studied (qua theoretical structures) in isolation from particular model profiles—as most philosophers of shared agency have done. An ontological construal is a set of theoretical hypotheses providing a mapping from some elements in a model profile to features of the real world. When fully spelled out, mechanistic models of shared agency are often intended to involve relatively complete ontological construals, mapping all or most elements in the model profile to aspects of the psychological behavior of agents acting together in the real world. By contrast, functional models of shared agency are often intended to involve only a partial mapping from some elements of these models to the overt behavior of actual agents—often focusing on the outputs of the model, which correspond to predicted behavioral consequences.

The distinction between mechanistic and functional models allows us to reconcile the debate over minimalism and maximalism in the study of shared agency by imposing on them different roles in our overall quest for greater understanding of the social world that we live in. Given that mechanistic models characteristically involve more detail and a fuller mapping from elements in the model to features of the real world, they can be used to complement the functional models, which more traditional philosophers of shared agency have put forth. Furthermore, the three-fold distinction between model templates, model profiles, and ontological construals can be used to carve out a systematic division of labor between theoreticians and empirical researchers in the social and cognitive sciences, who are interested in formulating empirical hypotheses on the basis of philosophical models of shared agency. However, it is an important open question whether the mechanistic and functional models of shared agency that philosophers have developed can in practice be used for formulating informative theoretical hypotheses about the social world. An important aim of this paper is to provide an enhanced meta-theoretical framework for adjudicating debates about the nature of shared agency, and thereby to facilitate new interdisciplinary research, which aims to use philosophical models of shared agency in the pursuit of a concrete understanding of the actual social world that we live in.

Author(s): Philippe Huneman and Isabelle Drouet
Title: But who are these agents? Investigating what agent-based modeling does in social science
Affiliation(s): CNRS, IHPST and Université Paris Sorbonne
Email: philippe.huneman@gmail.com and isabelle.drouet@paris-sorbonne.fr

Abstract: Agent-based models and agent-based simulations have become widespread tools in empirical social science. One specific use, on which the present paper focuses, consists in explaining token macro-phenomena by reproducing them. It is often argued that such an explanation is causal. In this paper we will ask what kind of causal knowledge can be gained from these models – if any -, and what are the proper epistemic conditions and explanatory strategies in which relying on them is legitimate. To address this question we will review various epistemic distinctions that are used to specify and classify agent-based modeling as a kind of explanation, and consider both of the two main concepts of causation that philosophers have defended.

The first section of the paper will review distinctions classically made in order to conceptualize explanations or types of models: equations vs. algorithms (e.g. Gaucherel et al. 2011; Lethinen & Kurikovski 2007), top-down vs. bottom-up, analytic vs. generative (Epstein 1999), patterns vs. processes. It is usually claimed that agent-based models in social sciences are defined by the second members of these conceptual pairs: they are algorithmic, bottom-up, generative, and aim at capturing the processes leading to known patterns. However, if we refer to the triadic conception of model-building according to their explanatory aims - realism, genericity and precision - put forth by Levins (1966), things appear less straightforward. Their position relatively to the triad genericity/realism/prediction is less clear, since some agent-based models, like Sugarscape models of the distribution of wealth (Axtell and Epstein 1996) or Hedström's model of unemployment in the Stockholm area in the 1990 (Hedström 2005), are more realistic as compared to others, like reaction-diffusion used in epidemiology (Colizza et al. 2008) or models of spread of fashion (Tassier et al. 2004), which are more generic.

Such nuances cast a doubt on the common view that in general agent-based models are explaining, in a bottom-up way, and especially when no equation is given or tractable that would represent the processes that leading to detected or specified patterns. When they are very generic, such as the famous flocking behaviour models initiated by Reynolds (1987), agent-based models may not realistically capture processes. This lack of realism, also argued for by Küppers & Lehnhard (2007) and Grüne-Yanoff (2009), appears problematic for the view that agent-based models represent causal processes in social sciences.
To substantiate this criticism, we will show that aiming at processes which yield patterns is not the only objective of agent-based models in social science. Even though in many cases such as Schelling's segregation models, one intends to explore processes likely to produce some attested patterns – here, social segregation – there are other uses of agent-based models, in which the explanandum is precisely the rules themselves according to which some agents may ultimately behave. This will be shown by considering models of the evolution of norms, which intend to unravel the conditions under which some kinds of norms can evolve and be implemented by agents (e.g. egalitarian norms, Gavrilets 2012).

The last section will question on this basis the kind of causation generally involved in these models. We will situate causality in agent-based modelling in relation to process views of causation (Salmon 1984, and their reappraisal in the current neomechanicist view of explanation, e.g. Darden and Craver 2013) and to counterfactual views of causation – including the influential type of manipulationnist views of causation (e.g. Woodward 2005). Since the above sections emphasized the importance of genericity in many agent-based models, we will argue that, as such, these models do not capture the causality in the sense of a process leading to an explanandum. The agents in generic agent-based models, like reaction-diffusion models, correspond to human agents as well as to particles in a magnetic field or biological species in ecological communities studied by invasion ecology: this shows that the agents in agent-based models have rules that do not necessarily correspond to (or are not actually instantiated by) any actual entity, and therefore do not necessarily obey any constraint regarding conserved quantities (a major requisite for causal processes sensu Salmon). And for the same reason the neomechanicist view of explanations as unraveling mechanisms made up of entities with proper activities in a given setting, whose combination produces the explanandum, is also inaccurate to make sense of what is going on in these simulations. Therefore counterfactual concepts of causation seem more accurate. Agent-based simulations do not explain causally by establishing the causal nature of the histories they represent; their ability to explain causally rests on the possibility to manipulate their inputs and thereby identify causes. Elaborating this view can make sense of both the agent-based models intended at unraveling processes that underlie social patterns, and the agent-based models aiming at explaining why some agents behave according to the kinds of rules we may legitimately infer.

Author(s): Harold Kincaid
Title: Social Classes: Real, Nominal or Bogus?
Affiliation: UCT
Email: kincaidharold592@gmail.com

Abstract: This paper is a discussion of the empirical and conceptual issues involved in deciding whether social classes are real. The topic is of interest for multiple reasons that are addressed in the discussion. One reason the issue deserves scrutiny comes from interest in the individualism-holism debate. Some versions of individualism are eliminativist about social entities--witness Thatcher's slogan that there is no such thing as society, only individuals--and doubts that any good social science invokes them. Classical social theory invoked classes in some form as central to explanation, so the status of social classes are a natural battle ground for the individualism-holism debate. They are also are natural touchstone for debates over pluralism in social categorization. One worry about appeal to social classes is that there seem to be many ways to divide individuals into social classes. So does that provide evidence for pluralism about social categorization even if it does not provide evidence for individualism. If it does support pluralism, how do we tell the real from the merely nominal? The reality of classes also raises issues about whether we should take classes to be types--set of individual with common properties--or as concrete particulars, in parallel with the debate over the debates over the status of species (kinds vs. historical individuals). Finally debates over classes also raise fact/value issues, since Marxist conceptions of class often invoke notions of exploitation and domination, notions certainly at first glance that seem to involve moral judgements. Thus there is need to explain the extent to which such value claims are involved in class analysis and what effects that has on the ideal of objective social science. The paper explores these issues and argues that there are good reasons for being a realist and monist about ruling elite classes in Western capitalist societies and for being a pluralist and in a sense nonrealist about classes otherwise.

The paper is organized as follows. Section 1 sorts out issues. There is a fair amount of work to be done here. First issues are philosophical ones about how to do social ontology. I reject the Searle approach which thinks that philosophers should first get clear on ontology and then the social research can begin for a naturalist view which sees the conceptual and ontological issues as continuous with the empirical social science issues. We can argue that classes are real if 1.) we have independent ways to measure them and 2.) show that they stand in causal relations (I assert this as a sufficient condition and am agnostic on it as a necessary one).This means I think that debates over the reality of classes have to be debates about specific social theories or accounts of classes and what they explain, not over some general conceptual arguments about what is real. Standing in causal relations I spell out in terms of there being what Dennett calls "real patterns"--relations that carry nonredundant information. Following Ross, there are good reasons to think that there can multiple ways of identifying such relations, which leaves room for pluralism of a certain (nonpromiscuous) sort. Related issues concern what kind of causal explanations classes are supposed to ground: large scale macrosociological explanations or explanations of individual behavior, especially individual "life chances." Finally, it is important to be clear whether we are thinking of classes as sets or types of individuals or as concrete entities. I will argue there is no reason class analysis in the social sciences should be restricted to one or the other.

Section 2 provides some background by briefly sketching Marxian and Weberian approaches to class. Marxian approaches are unreasonably ambitious I argue for various reasons, but their treating classes as concrete individuals is an important idea and defensible I argue later for the idea of ruling elites or classes. Weberian accounts of market determined positions and common life chances help identify room for pluralism about classes other than ruling elites. Thus I use these two approaches to set up the discussion that follows.

Section 3 argues that there is good evidence for classes in the sense of a ruling elite in advanced capitalist countries. This is class largely in the sense of a concrete particular. The evidence is both in the form of multiple measurements and identifying causal relations. But these two requirements in practice are a host of different pieces of evidence. Relative to issues of individualism, the notion of class defended here combines individual level, mesolevel information about the role of various kinds of organizations and institutions, and macrosociological factors in one potential overarching explanation, suggesting a fruitful nonreductionist picture of the relation of individual and social explanations. Relative to question of values in social explanation, I argue that the sense of class defended in this section is indeed close to value issues around such concepts of exploitation, but that the results do not require value judgements for their force though they certainly are amenable to making moral arguments given some plausible background assumptions. This section also argues that the defense of class provided need not be incompatible with some alternative classifications of elites and their roles, for these alternatives are orthogonal divisions of the causes in the spirit of Ross's Dennettian "rainforest realism."

Finally section 4 looks at prospects for other social classes in the strong form of concrete particulars defended in the previous section for ruling elites. I argue that the evidence mostly supports weaker type-style classes--classes more amenable to Weberian market position analyses--instead of class as full fledged social entities. I defend such approaches from criticisms that they are arbitrary because multiple class divisions are warranted and from criticisms from the individualist direction that the social is doing no work beyond the traits of individuals. A final upshot from sections 3 and 4 is the individualism-holism debate is better understood as asking how individualist can we be and how holist must we be, an understanding of the debate that I have defended in print elsewhere.

Author(s): Loren King, Brandon Morgan-Olsen, James Wong
Title: Virtues of Science and Citizenship: Against Two Orthodoxies
Affiliation(s): Wilfrid Laurier University, Loyola University Chicago, and Wilfrid Laurier University
Email: king.loren@gmail.com, bmorganolsen@luc.edu, and jwong@wlu.ca

Abstract: In reflecting on our responsibilities as members of a liberal democracy, one might intuitively consider there to be a significant division of epistemic labor between scientists, on one hand, and average citizens, on the other. That is, one might well consider the epistemic labor of scientists and citizens to be substantially different in kind. However, we argue here against any compelling distinction between the cognitive demands of good science and of good citizenship in a liberal democracy.

We make our case by way of two orthodoxies concerning the practice of science and the relationship between science and policy. A powerful criticism of one of these orthodoxies suggests a way of approaching the other, and that interpretive stance leads to, and ultimately sustains, our thesis. The justification for privileging any deliberative outcome, in science or public life, importantly presumes certain epistemic virtues for both science and citizenship.

The Promise and Peril of More Voices

First, the motivating puzzle. In science as in politics, it is reasonable to hope that more voices, and more meaningful opportunities for dissent, can liberate us from the tyranny of received wisdom and vested interests. In both science and politics, then, diversity and dissent are vital. Yet more voices – especially acrimonious dissenters – can distract and confuse, sometimes deliberately.

Given this situation, it would certainly do no harm if scientists, legislators, and citizens more generally were reliably able to chart a course between the dangerous shoals of complacent consensus and partisan distortion. At the very least we ideally want trusted elites – scientists, judges, journalists – to meet this burden, and we hope that citizens might typically do better than chance at identifying the epistemic competence of these elite actors.

Our motivating puzzle, in short, is the common question of finding privileged, reliable sources of epistemic authority. Our corresponding suspicion is that calls for pluralism and dissent in both science and politics seem to make comparable epistemic demands on participants. Can that suspicion be vindicated by argument?

Two Orthodoxies

We make our case by way of evaluating two orthodoxies about relations between science, citizens, and democratic policy-making. The first orthodoxy involves an epistemic privileging of scientific claims in public debate. The second involves an epistemic privileging of theory in scientific debate.

1. Science as a privileged institution
The first orthodoxy suggests that we ought generally, as a default epistemic posture, to privilege scientific knowledge claims in considerations of policy. On this view, scientists – working as researchers, distinct from their status as citizens – inform policy-making by playing a variety of roles in a complex institution. According to this orthodoxy, however, the relationship between science and policy is largely asymmetric. As Philip Kitcher puts the point, "scientific inquiry sets the standards for the acceptability of belief" (2008, 11), not the other way around.

2. The Primacy of theory
The second orthodoxy can be stated far more succinctly than the first. According to this view there is, within the sciences, a rough but useful distinction between theory and experiment. Theorists theorize (usually with mathematical formalization), building models that in turn generate testable hypotheses. Other scientists then craft experiments, collect specimens, observe phenomena, and scrutinize data.

Against Orthodoxy

These orthodoxies have faced daunting criticism from a variety of sources. Helpfully, for our purposes, a powerful response to the second orthodoxy buttresses complaints against the first, while also giving support to our virtue-theoretic ambitions.

(i) Public values in science
On the first orthodoxy: a range of powerful arguments have suggested that science is not (nor ought to be) insulated from the influence of everyday values. We say "everyday" values to highlight the fact that these theorists are not meaning merely to resist the characterization of science as value-free. Advocates of the first orthodoxy can easily admit their use of suitably scientific cum epistemic values, such as truth, empirical adequacy, predictive power, or simplicity. Those who challenge this orthodoxy counter that science is implicated in more than a narrow range of epistemic values, but also in the same sorts of value disputes that characterize our public lives.

(ii) Reflective equilibrium-seeking between fact and theory
Against the second orthodoxy: it is now almost a truism of philosophy of science to assert that 'facts' (and 'observation reports' about them) are 'theory-laden': they never simply speak for themselves. But it is also the case that theories are often 'fact-laden', insofar as they are deeply

implicated in our varied encounters with the world. Theories rarely 'cohere' or 'inform' without a dense background of reasonably settled facts. Crucially, then, the second orthodoxy fails to account for the intimate reciprocity between theory, empirics, and practice in so much of science. Much of science is not a process of axiomatic reasoning to generate testable hypotheses; it is, instead, a complex process of reflective equilibrium-seeking.

Concluding Thoughts

Here, then, is the crux of our argument: both scientists and citizens are engaged in a comparable process of wide reflective equilibrium-seeking. This is true in deliberating on the proper course of public policy, and is true as well in deliberating about reliable epistemic authority. The orthodox rejoinder would be to privilege the "fixed points" of scientific inquiry in order to distinguish the citizen's equilibrium-seeking from the scientist's. After all, considered judgements in matters of public value are not obviously like observation reports in science, which at least purport to be about an independently real external world. However, resisting these two orthodoxies undercuts such a response.

Thus, we suggest the same personal orientation toward evidence and argument gives us an account of privileged inputs in moral justification for political questions, as for properly generated claims of fact in science. In this way, the justification for privileging any deliberative outcome, in science as surely as in public life, importantly presumes these epistemic virtues, for both science and citizenship. Furthermore, these are the very virtues used to arrive at considered judgements about fact and value, which are in turn well-modeled as reasonably stable "provisional fixed points" generated through a process of wide reflective equilibrium-seeking.

Author(s): Jo-Jo Koo
Title: Haslanger's Critical Social Theory of Gender and Race from the Point of View of the Philosophy of Social Science
Affiliation: Skidmore College
Email: jjkoo1999@gmail.com

Abstract: Sally Haslanger's Resisting Reality: Social Construction and Social Critique (2012) is a book that collects together her various papers in the past two decades about the philosophy of gender and race. When they are read together in close temporal succession, especially when this is done in light of her important and illuminating "Introduction" to these papers, what emerges is a vigorously argued, penetrating, and instructive critical social theory pertaining to the "social construction" (in one of the several senses of this slippery term that Haslanger helpfully distinguishes) of gender and race. My paper is in large sympathy with the theoretical sensibility, aims, and argumentative moves that Haslanger makes in her book, which articulate and defend a nuanced social-structural conception of gender and race. The main aim of my paper is to highlight and consider the implicit assumptions of her critical social theory of gender and race from the point of view of the philosophy of social science. More specifically, I aim to explore how her critical social theory is committed to (1) a mechanistic conception of social explanation and (2) a social ontology that emphasizes the analytic priority of what she calls the "constitutive construction" to the "causal construction" of gender and race (see below for her definitions of what these expressions mean), even though there are, to be sure, close connections between these two ways in which gender and race are socially constructed in terms of her theoretical framework (see Haslanger 2012: 8, diagrams).

Concerning (1), there are some interesting convergences between Haslanger's implicit commitment to mechanism-based explanation of social phenomena (her focus is obviously on those of gender and race in particular) and the account of this type of social explanation that Petri Ylikoski has put forward in a recent book chapter (2012). Ylikoski argues that mechanism-based explanations of social phenomena: are contextually and pragmatically conceived, supported, and used; are microfoundationalist but not reductionist in the sense that such explanations do not accept the view that macro level explanations are causally irrelevant unless they are given microfoundations at the level of intentional attitudes and agents; draw an important distinction between causation and constitution. It seems prima facie plausible that these points that Ylikoski makes about mechanism-based explanations of social phenomena should resonate with and be connected in some ways to Haslanger's implicit commitment to this type of social explanation in her critical social theory of gender and race. My paper tries to work out what these apparent resonances and connections are specifically. Although Haslanger and Ylikoski often use similar sounding terms (e.g., talk of causation vis-à-vis that of constitution, as these figure in their accounts or analyses of social phenomena), it is not easy to sort out how their uses of these terms converge or diverge (if they do so at all). The first part of my paper aims to make some headway in accomplishing this task.

Concerning (2), I discern that Haslanger prioritizes the significance and impact of "constitutive constructions" over "causal constructions" in her social ontology of gender and race. This stems from Haslanger's choice of giving what she calls a "focal analysis" of gender and race: "A focal analysis explains a variety of phenomena in terms of their relations to one that is theorized, for the purposes at hand [i.e., for the purposes of Haslanger's critical social theory], as the focus or core phenomenon. For my [i.e., Haslanger's] purpose, the core phenomenon is the pattern of social relations that constitute men as dominant and women as subordinate, of Whites as dominant and people of color as subordinate. An account of how norms, symbols, identities, and such are gendered or raced is then given by reference to the 'core' sense." (Haslanger 2012: 7) This seems to suggest, analytically speaking, that Haslanger gives theoretical priority to the constitution (or "constitutive construction" in her terminology) of gender and race over the social factors that cause human beings to become gendered or racialized (i.e., their "causal construction" as people who are classified as members of a gender or race). In her terminology (Haslanger 2012: 131):

X is socially constructed causally as an F iff social factors (i.e., X's participation in a social matrix) plays a significant role in *causing* [my emphasis] X to have those features by virtue of which it counts as an F.

X is socially constructed constitutively as an F iff X is of a *kind* or *sort* [my emphases] F such that in *defining* [my emphasis] what it is to be F, we must make reference to social factors (or: such that in order for X to be F, X must exist within a social matrix that *constitutes* [my emphasis] F's.).

If my discernment in Haslanger's framework of the priority of constitution (or "constitutive construction") over causation (or "causal construction") is right, there is a sense or way in which her social ontology of gender and race, or rather a certain conception of social ontology for the purposes of her critical social theory of gender and race, sets the basic constraints for her analyses of other kinds of social construction and social explanation pertaining to these social kinds. My aim in this part of the paper is to explore, quite tentatively, whether this move of Haslanger illustrates the general dependence of understanding the explanation of human behavior (obviously a key concern of the philosophy of social science) on properly understanding the human way of being in the world (philosophical anthropology).

Author(s): Miles MacLeod and Michiru Nagatsu
Title: Model coupling in resource economics: conditions for effective interdisciplinary collaboration
Affiliation: University of Helsinki
Email: miles.macleod@helsinki.fi and m.nagatsu@gmail.com

Abstract: Recent years have seen a burgeoning interest in promoting and analyzing interdisciplinary collaboration in the natural and social science by researchers and university administrators. There is now a substantial collection of academic work and policy documents available on the subject. Most interdisciplinarity studies so far have been the domain of science policy and science and technology studies research. This research has focused on the institutional, organizational and social dimensions of scientific research that promote or inhibit interdisciplinary interactions, while developing policy frameworks and guidelines for structuring scientific institutions and organization to promote interdisciplinary interactions (see e.g. Gibbons et al. 2004). What is largely missing however is actual case-based study of how the available cognitive resources of different scientific fields and disciplines – their extant theories, modeling templates, experimental and evidential resources – get combined to create functional collaborative platforms for investigation and problem-solving, as well as precise descriptions of what is gained from these combinations. Discussions have identified the need for researchers to integrate values, goals, methods and so on in order to collaborate, but with little concrete guidelines or case studies of how this can happen conceptually and methodologically (see Mattila 2005 as one of the few existing case studies of this kind).

We have here an opportunity for philosophers to actually contribute their expertise on the conceptual and methodological side of scientific processes, to formulate more informed policy criteria on how to construct effective interdisciplinary collaborations. While the philosophical literature studying the explanatory affordances of different types of conceptual and methodological integration is starting to grow, there are yet few philosophical investigations exploring how effective a strategy of integration might be in creating functional collaborative problem-solving platforms given the constraints and difficulties of interdisciplinary research. Our goal in this paper is to demonstrate that conceptual frameworks developed in order to integrate background models and model-building practices can be structured in ways that facilitate collaborative responses to problems. Such frameworks can thus be measured and analyzed according to their ability to facilitate effective and gainful collaborative responses.

We identify and examine one such relatively clear conceptual framework for integrating ecological and economic models developed through successful collaborative interactions between groups of resource economists and ecologists. Their interdisciplinary interaction relies upon what we call a coupled model framework. After a brief introduction to current interdisciplinary studies (section 2) and integration in economics and ecology (section 3), we show how various features of this framework serve to demarcate the nature and structure of the collaboration required between ecologists and economists (section 4). Further using this case we apply two informal measures for assessing the degree to which this conceptual framework generates effective collaboration in practice; by assessing 1) the features of the framework that facilitate efficient collaborative interaction in practice given the various constraints of working across disciplinary boundaries (collaborative affordances), and 2) the gain these approaches afford through the agency of collaboration in comparison with what could be achieved working purely with one's disciplinary resources and skills (collaborative gain) (section 5). From this information we draw several lessons on both the affordances of this kind of methodological set-up for interdisciplinary research in ecology and economics, and interdisciplinarity more broadly (section 6). We conclude by summarizing our arguments (section 7).

Author(s): Manuela Fernández Pinto
Title: Economics Imperialism in Social Epistemology
Affiliation: University of Helsinki
Email: manuferpi@gmail.com

Abstract: In the article "Against Scientific Imperialism" (1995), John Dupré argued for a horizontal pluralism of sciences against the imperialistic tendencies of some scientific disciplines such as economics and evolutionary biology, which aimed at a unified explanation of human behavior. According to Dupré, "devotees of these approaches are inclined to claim that they are in possession not just of one useful perspective on human behavior, but of the key that will open doors to the understanding of ever wider areas of human behavior" (374). Dupré warned us against these imperialistic tendencies, claiming first that "as scientific methodologies move further away from their central areas of application their abstractions become ever grosser, and their relevance to the phenomena become ever more distant," and second that "…alien intellectual strategies may import inappropriate and even dangerous assumptions into the colonized domains" (380).

Take the case of economics imperialism, one of Dupré's main examples. Unquestionably economics has broadened its scope well beyond "economic" phenomena to explain other "social" phenomena in the realms of political science, anthropology, sociology, and geography (Becker 1976; Stigler 1984). The incursion of economics into neighboring disciplines has been a controversial topic for social scientists, whose views range from an uncritical appraisal of economics' scientific methods (e.g., Lazear 2000), to an undecided attitude (e.g., Green & Shapiro 1994), to a radical rejection of the trend (e.g., Fine 2002). Some hence disagree with Dupré's blunt rejection of economics as a general methodology for the study of human behavior.

At a time when interdisciplinarity is highly praised in academic circles, Dupré's analysis of scientific imperialism raises important questions about the epistemic fruitfulness and appropriateness of the interaction among scientific disciplines that is being encouraged. Accordingly, philosophers of science have taken up the task of examining further the concept of scientific imperialism with the aim of understanding this particular interdisciplinary relation emerging from the incursion of one scientific discipline or domain into another. They have contributed to the clarification of the conceptual space in which scientific imperialism occurs as well as to the normative assessment of such practices (Dupré 1995, 2001; Clarke & Walsh 2009, 2013; Mäki 2009, 2013; Kidd 2013).

For the most part, the recent literature on scientific imperialism examines cases in biology and the social sciences, in which philosophy as a discipline is not involved. However, with the emergence of social epistemology in the late 1980s, philosophers of science followed other social scientists in importing economic models to explain the epistemic success of scientific inquiry (e.g., Kitcher 1990; Goldman & Shaked 1991). Following Dupré, this move of economics into social epistemology could be seen as a case of economics imperialism, where a scientific methodology is used to explain phenomena beyond its original domain. And this of course brings back Dupré's worries: Is economics importing inappropriate assumptions to epistemology? Does the transfer require further abstraction and detachment from the phenomena to be explained? Is philosophy adopting economics' style of argument? Is economics interfering with epistemology's autonomy? Is it displacing other valuable explanations of cognitive phenomena?

The aim of this paper is to examine the case of economics imperialism in social epistemology and to determine whether economics explanatory expansionism appropriately contributes to this philosophical subfield or not. The paper is divided in five sections. The second section presents the conceptual and normative framework for examining cases of scientific imperialism that philosophers of science have worked so far. Here I follow Mäki's (2009) four normative constraints—ontological, epistemic, axiological, and institutional—for the assessment of cases of scientific imperialism. The third section examines social epistemology's interdisciplinary transfers with economics, especially through the development of the interdisciplinary approach known as the economics of scientific knowledge (ESK) (Hands 2001; Zamora Bonilla2012). I examine two representative approaches to ESK: Kitcher (1990) and Goldman & Shaked (1991). Finally, I give an evaluation of the appropriateness of ESK in terms of Mäki's four normative constraints. I argue that the ontological constraint imposes epistemological commitments that one might have reasons not to endorse and thus I left it aside. I then show that the epistemological and the institutional constraints are difficult to assess in the case of ESK. In part this is due to the fact that the constraints leave a gray area for assessment, where one can argue for or against the satisfaction of the constraint given the same evidence. Finally, I argue that the axiological constraint gives us good reasons to question economics imperialism in social epistemology. In particular, I show that ESK's attempt at explanatory unification under the rational choice framework fail to express significant human interests. In this sense, the axiological constraint gives us the strongest reason to be doubt the appropriateness of the incursion of economics into social epistemology.

Author(s): Ouzilou Olivier
Title: A dispositional account of collective beliefs: the case of political parties
Affiliation: Université de Lorraine
Email: olivier_ouzilou@yahoo.com

Abstract: In our everyday life, we sometimes attribute intentional states to social groups as a whole, we also understand, predict and evaluate their behavior and their intentional states in terms of their possessing mental states. For example, we can ask what will be the position of a syndicate as such toward a given economic problem. We can also try to predict, explain or evaluate such a position by reference to what we know or suppose to be its beliefs, intentions, hopes, etc. But these kinds of attribution and explanation occur also in scientific context. Indeed, social science studies often explain the behavior of collective entities by refering to their beliefs, intentions or desires: for example, a social scientist can say that such a government intends to undertake such an action because it believes that p and desires that p.

However, how must we understand the attribution of mental states to social groups ? I will focus here on the concept of 'collective belief'. According to Gilbert, what she calls the 'joint acceptance' model of group beliefs corresponds better than the summative account of group belief to our unexamined everyday and scientific ascriptions of beliefs to collective entities. Again following Gilbert, a group G believes that p “if and only if the members of G are jointly committed to believe that p as a body” (Gilbert, 2002). It is therefore not a necessary condition of a group's belief that p that each member of the group believes that p either that most group members believe that p, or that there be common knowledge within the group that most members believe that p.

A question arises: are there disanalogies between belief in the individual case and collective belief ? In other words, can we say that collective beliefs in a non-summative sense lack key features of belief in general ? I will try partly to answer this question by focusing on the dispositional property of belief. Can a collective belief in a non-summative sense be dispositional and, if the answer to this question is positive, in which sense can it be said so ? Moreover, if a dispositional characterization of collective belief is correct, we must ask: to what can a collective belief dispose ? According to Schwitzgebel (2002), the dispositional properties belonging to individual belief stereotypes fall into three main categories: behavioral dispositions, phenomenal dispositions and cognitive dispositions, that is to say dispositions to draw conclusions entailed by our belief or to acquire beliefs consonant with the belief in question.

First, I want to show that collective beliefs can be understood as dispositional when they are ascribed to what Pettit (2007) calls 'purposive collectivity'. In order to illustrate this point, I will take the example of political partis and try to provide an epistemic description of political partis. We can distinguish the kind of beliefs of a political party according to their importance or centrality in the set of beliefs of the party. Some beliefs (descriptive and normative), which can be called 'core beliefs', are constitutive properties of the party insofar as they contribute to define the party's identity. Therefore, the revision of these beliefs is very unlikely because it would weaken a part of the identity and the reason for being a given party. By contrast, other beliefs can be called 'peripheral': they are open to revision and a party can easily adjust when they seem to be false or when it is not strategic to adopt them. Moreover, political parties seem to be intentional subjects over and beyond its members, displaying intentional states, such as beliefs, intentions, desires, hopes, etc. and performing actions that such states rationalize. Therefore, to be an intentional subject, a party must display a rational unity.

We can say that there are two interlinked kinds of rational constraints, synchronic and diachronic: synchronic because a party will generally act in a manner that is rationalized by both its mental states – the core beliefs, in particular - and the evidence (in the social world) at its disposal. It is what Pettit (2007) calls the 'attitude-to-evidence standards' which requires that the system's belief be responsive to evidence and the 'attitude-to-attitude' standards, which require that, even as they adjust under evidential inputs, its beliefs and desires must be coherent ; diachronic because past judgments of the group will constraint the judgment that the group ought to make in various new cases: it must be coherent with the past judgments. In other terms, rational unity is a constraint that binds the attitudes of the collectivity at any time and across different times.

I would argue that this epistemic description of political party helps us to understand in which sense a collective belief of a purposive collectivity can be described as a set of cognitive and behavioral dispositions. In other terms, a belief can manifest itself, without being explicitly mentioned, in the way in which a party thinks and acts. If this analysis is correct, a collective belief in a non-summative sense is not reducible to a collective judgement but it can be implicitly expressed in collective judgements, decisions, intentions, etc. I want finally to show that there are nevertheless two main differences between the ways in which individual and collective belief are dispositional. The first difference concerns the categories which constitue, according to Schwitzgebel, the belief stereotypes: collective beliefs in a non-summative sense can not be described as phenomenal dispositions, that is to say as dispositions to have certain sorts of conscious experiences. The second difference is the following: in contrast with individual belief, the existence of the dispositional beliefs of purposive collectivity seem to be necessarily dependant on occurrent judgements.

Author(s): Gustav Ramström
Title: Mechanisms in social science – what are they and how do we model them?
Affiliation: Stockholm University
Email: gustav.ramstrom@statsvet.su.se

Abstract: The increasing use of and discussion on the concept "mechanism" in social science in recent years has been strongly associated with the idea of presenting micro foundations and Coleman's classical macro-micro-macro model (a.k.a. "Coleman's boat" or "Coleman's bathtub"). Coleman's model has become the main model used both in general theoretical discussions on mechanisms and in presentations of specific mechanisms in empirical studies. In this paper I argue that Coleman's model has a number of problems/weaknesses. To address these problems I present a modified version of the classical macro-micro-macro model. The modified model, I argue, constitutes a preferable tool both when trying to understand the general nature of mechanisms and when generating accounts of specific mechanisms in empirical work. Furthermore, the modified model illustrates better the full range of different forms/categories of mechanisms that occur in social scientific study.

The paper is divided into three parts. The first part consists of an attempt to clarify what we actually refer to when we talk about mechanisms in the social sciences. I argue that the tried and tested method of simply analyzing and comparing different, often quite abstract (one or two sentences) definitions of the concept don't get us very far, since there are about as many definitions as there are participants in the debate. Instead I focus on what participants in the discussion claim that mechanisms are good for, and the interconnected issue of to what "bad" they are a remedy. When analyzing the discussion on mechanisms with these questions in mind three main answers appear:

1.) Mechanisms open up the "black box", i.e. they explicate how dependent and independent variables are connected.
2.) Mechanisms are middle range patterns, as opposed to local or universal patterns/regularities.
3.) Mechanisms are probabilistic (statistical) rather than deterministic laws.

Specific uses of the concept might include one, two or all three of these meanings, however, it is argued that the first use of the concept, i.e. as an antonym to the black box, is the dominant meaning ascribed to mechanisms in the contemporary debate. It is therefore suggested that this sense of the concept (a mechanism is what goes on in-between X and Y, which makes Y change with X) could be used as a "minimalist" definition, onto which scholars consciously and explicitly can choose, or not choose, to add further attributes.

In part two, using this minimalist definition, I continue by discussing how mechanisms in this sense of the concept usually are described and modelled. The discussion shows that Coleman's model, sometimes with a few minor and more recently contributed conceptual "add-ons", still very much dominate the discussion.

I go on to argue that Coleman's model has three main problems: 1.) It does not clearly separate actual causal processes from aggregation/summarization, which often leads to causal gaps in accounts of causal sequences between X and Y where large parts of causal chains are lumped together in so called "transitional mechanisms" (the micro-macro arrow in Coleman's model). 2.) The model only "aggregates" action whilst we in practical social science often deal with aggregates of mental states/properties (like for example the level of generalized trust in a certain country), or variables including both mental states and actions (such as certain cultural traits etc.) 3.) The model cannot deal with dependent variables which are not inherently social in a methodological individualist sense, i.e. variables which are not describable/reducible to actions, interactions etc. Social science, I argue, is strictly speaking not distinguished by the "socialness" of our X's and Y's but by the socialness of the mechanisms which are theorized to connect X and Y. We may thus share dependent variables with the natural sciences, for example in the sociology of health, if the causal sequence leading up to the outcome in question includes mental processes (decisions etc.) of individuals. Since such "non- social" phenomena can't be aggregated from actions, Coleman's model can't deal with them in a satisfactory manner.

After discussing these problems I move on to present my own modified model and explicate how it solves these issues, as well as improves our general understanding of what constitutes a causal mechanism in the social sciences.

In the third part of the paper I use the model to generate a typology of different kinds of mechanisms. Mental process based causal links are distinguished from non-mental process based casual links and it is shown, for example, how these different links may be combined to generate hybrid mechanisms (connecting X's to non-social Y's) or how "longer" causal mechanisms may contain several instances of the same kind of "mental process links" when it includes transference of causal forces from one (type of/set of) actor(s) to another. The differences between the typology and existing categorizations of mechanisms will be discussed.

Author(s): Kristina Rolin
Title: Values in Science: The Case of Feminist Standpoint Theory
Affiliation: University of Helsinki
Email: kristina.rolin@helsinki.fi

Abstract: Feminist standpoint theory is an attempt to analyze the proper role of social and political values in science (see e.g., Crasnow 2008, 2009, 2013, 2014; Harding 1991, 2004; Intemann 2010; Rolin 2006, 2009; Wylie 2004, 2012; Wylie and Nelson 2007). While the controversy over the proper role of social and political values in science has entered the mainstream in philosophy of science, there is very little uptake of feminist standpoint theory (see e.g., Carrier et al. 2008; Biddle 2013; Douglas 2009; Kitcher 2001, 2011; Kincaid et al. 2007; Machamer and Wolters 2004). My aim is to rectify this situation by arguing that feminist standpoint theory provides us with a unique model for understanding how social and political values can play not only a legitimate but also an epistemically productive role in the social sciences.

My main thesis is that feminist standpoint theory is a social epistemology of scientific/intellectual movements (SIMs). In feminist standpoint theory, social and political values are thought to lead social scientists and other scholars to build SIMs, and such movements are thought to play an epistemic role in two ways. First, they enable social scientists and scholars to generate evidence under conditions where relations of power tend to suppress or distort evidence. Second, they provide social scientists and scholars with an epistemic community where they can receive fruitful criticism for research which may be ignored in the larger scientific community. While an individual social scientist or scholar may work against predominant power relations on her own, her research is unlikely to lead to a scientific change unless it is part of a scientific and intellectual movement. Only a SIM can successfully overcome the methodological and epistemological obstacles that relations of power raise for scientific inquiry. It can do so by coupling the generation of evidence with empowerment and community building. This model of values/evidence interaction outlined in feminist standpoint theory is unique in that it cannot be found in the other arguments philosophers have advanced against the ideal of value-free science (see e.g., Douglas 2009; Kourany 2010; Lacey 1999; Longino 1990, 1995; Root 1993; Solomon 2001).

My presentation has three sections. In order to highlight the unique features of feminist standpoint theory, in Section 1 I present a brief review of three well-known arguments against the value-free ideal: (1) an argument from pluralism with respect to epistemic values (Elliott 2013; Elliott and McKaughan 2014; Kitcher 1993; Kuhn 1977; Longino 1995; Rooney 1992; Solomon 2001); (2) an argument from inductive risk (Biddle 2013; Brown 2013; Douglas 2000, 2007, 2009; Elliott 2011; Steel 2010, 2013; Wilholt 2009); and (3) an argument from value-laden background assumptions (Anderson 1995, 2004; Clough 2011; Hawthorne 2010; Intemann 2001, 2005; Longino 1990, 2002; de Melo-Martín and Intemann 2012; Richardson 2010). The arguments advanced against the value-free ideal (which will be defined in Section 1) are built on slightly different yet overlapping analyses of what it means for a scientist to accept a hypothesis or a theory, and how non-epistemic values can play a legitimate role in acceptance which is thought to be a core epistemic moment in scientific inquiry. While these arguments offer valuable insights into the role of values in science, they do not do justice to feminist standpoint theory because they focus on the role of values in individual scientists' decision making thereby ignoring scientific/intellectual movements (SIMs).

In Section 2 I introduce three theses associated with feminist standpoint theory: (1) the situated knowledge thesis, both generic and systemic (see e.g., Wylie 2012); (2) the thesis of epistemic advantage (see e.g., Wylie 2004); and (3) the achievement thesis (see e.g., Crasnow 2013). While I agree with Alison Wylie that the thesis of epistemic advantage is best understood as an empirical hypothesis suggesting that "contingently, with respect to particular epistemic projects, some social locations and standpoints confer epistemic advantage'' (2004, 346), I propose a novel interpretation of it. I argue that insofar as there is an epistemic advantage associated with unprivileged or marginal social positions, the advantage accrues to a SIM which is political in the sense that it aims to change relations of power. I build on Scott Frickel's and Neil Gross's (2005) sociological theory of SIMs to argue that feminist standpoint theory is the most sophisticated attempt to analyze the epistemic significance of SIMs developed in philosophy of science so far. According to Frickel and Gross, "SIMs are collective efforts to pursue research programs or projects for thought in the face of resistance from others in the scientific or intellectual community" (2005, 206).

In Section 3 I situate feminist standpoint theory in the field known as the social epistemology of scientific knowledge. Much of the literature in the social epistemology of scientific knowledge focuses either on scientific communities or on research groups thereby ignoring SIMs. For example, some social epistemologists propose norms which characterize ideal scientific communities (see e.g., Longino 1990, 2002; Zollman 2007). Some others are concerned with an ideal distribution of research efforts in scientific communities (see e.g., De Langhe 2010, 2014; Kitcher 1990, 1993; Solomon 2001; Weisberg 2013; Weisberg and Muldoon 2009; Zollman 2010). Some social epistemologists suggest that scientific knowledge produced by research groups involves collective beliefs or acceptances (Andersen 2010; Bouvier 2004; Cheon 2013; Gilbert 2000; Rolin 2010; Staley 2007; Wray 2006, 2007). Some others suggest that the epistemic structure of scientific collaboration is based on relations of trust and interactions among scientists (Andersen and Wagenknecht 2013; Fagan 2011, 2012; Frost-Arnold 2013; Hardwig 1991; Kusch 2002; de Ridder 2013; Thagard 2010; Wagenknecht 2013, 2014). Clearly, the term "social" in the social epistemology of scientific knowledge means that philosophers are concerned either with scientific communities or with research groups. After explaining how SIMs differ from scientific communities and research groups, I conclude that there is a need for a more systematic inquiry into the epistemic significance of SIMs.

Author(s): Rosa W Runhardt
Title: Causal Generalizations and Epistemic Homogeneity
Affiliation: London School of Economics
Email: r.w.runhardt@lse.ac.uk

Abstract: Social science researchers who build general theories have to abstract from particular cases. Doing this means the researcher conceptualizes the case as members of a class. For instance, one labels several conflicts as 'insurgencies', and makes general claims about 'insurgencies'. Generalization necessarily simplifies away some of the unique aspects of the cases in favour of broader applicability. In this paper, I show that generalization can follow a clear pattern: researchers conceptualize phenomena according to the best known information on the causal connections of these phenomena, attempting to form populations that are causally homogeneous, i.e. that are causally similar in the relevant respects. I also show how generalization is limited. Due to the fuzzy nature of social science phenomena (think of 'poverty', 'wellbeing' and the like), generalization is not entirely value-free; what is considered causally relevant, and thus what is considered causally homogeneous, depends on the aim of the researchers. This also implies that populations are not set in stone; if certain members of the population turn out to behave differently in some situations, further subdivision of the population may be necessary.

In the first part of my paper, I briefly outline the belief that the events or phenomena one generalizes over should be causally similar in the relevant respects, which can be found in the literature under the term 'unit homogeneity'. Paul Holland, for instance, argues that two phenomena are causally similar relative to a set of background conditions, if and only if the probability of the effect given the cause is the same for both events or phenomena given that set of background conditions (Holland 1986). Any population we wish to generalize over should be causally homogeneous in this way. In this first part I show that although the notion of unit homogeneity was taken up by methodologists like Gary King, Robert Keohane, and Sidney Verba (King, Keohane, and Verba 1994), unit homogeneity will not always be helpful when it comes to social scientific practice.

In the second part of the paper, I follow Dan Hausman's argument that we often lack the causal knowledge required to definitively partition a population into causally homogeneous subpopulations (Hausman 2010). I then present my own solution to the problem, an argument for the use of 'epistemic homogeneity': we should generalize over the narrowest populations which we believe are causally homogeneous with respect to the causal relations of interest to us, and be open to subsequent divisions of the population into subclasses. This argument relies on a distinction between ontological and epistemic homogeneity, which follows a move by Wesley Salmon (Salmon 1971, 1977), and which is nowadays employed in amongst others philosophy of medicine (Clarke 2011). I show how the notion of epistemic homogeneity can bring us closer to accounting for what constitutes an adequate justification for the scope conditions of a general theory about a class of social phenomena, emphasizing potential future partitioning of an epistemologically homogeneous class into causally dissimilar subclasses.

Next in the paper, to illustrate my claims, I discuss the development of Nicholas Sambanis' theory on ethnic civil wars (Sambanis 2001) as a case of partitioning a class previously thought to be epistemically homogeneous. I show that although social science researchers before Sambanis had reasons to believe that all civil wars are slightly different, nevertheless they attempted to formulate general theories about this class (such as, for instance, in the debate on whether financial incentives for rebels are or are not one of the main causes of the breaking out of civil war). I will show that the class of civil wars was treated as an epistemically homogeneous reference class until Sambanis showed this class could be fruitfully partitioned into two causally dissimilar subclasses: ethnic and nonethnic civil wars.

To conclude my paper, I consider ways of calculating the relevance of a cause C on effect E for a particular population, even when we are not able to refer to the relevance of the cause on the effect for any particular subpopulations. I critically assess Hausman's suggestion of using the 'average effect'. A common method of calculating the average effect is through RCTs; we can calculate the average effect by holding fixed the frequencies of all causally relevant background factors at their frequency in the population under study and see whether, against this background, the conditional probability of the effect, given the cause, is larger than the conditional probability of the effect, given the absence of the cause. In this concluding part of the paper, I discuss the applicability of such an approach to social science, illustrating my claim with Nicholas Sambanis's study. I conclude that though using RCTs may lead us to find the average effect, it cannot help us with further subdividing a class previously considered epistemically homogeneous. Thus, I conclude that using a mixed-method approach, which combines the use of the average effect with the study of individual cases, is necessary. This may lead to the pinpointing of previously unconsidered causally relevant differences as the epistemic homogeneity notion requires.

Author(s): Matthew Sample
Title: Social Science as the Measure of All Things: Prospects of a Sellarsian Perspective
Affiliation: University of Washington
Email: mssample@gmail.com

Abstract: Wilfrid Sellars, in Empiricism and the Philosophy of Mind (1956), caused no small amount of debate by simultaneously suggesting both that the ``space of reasons'' is not reducible to scientific or purely naturalistic explanations and that ``science is the measure of all things.'' For many readers, there appears to be a tension between these two statements. On the one hand, science is unable to fully explain the epistemic or moral behavior of humans. Yet, on the other hand, science is hailed as our best source of descriptions and explanations of the world, humans included. O'Shea (2010) notes that, while it might be tempting to ignore this second thesis as merely an echo of positivist scientism, we should consider the possibility of reconciling these two theses. I will argue that this possibility of reconciliation, a middle way between normativism and anti-normativism, provides some philosophical insight into scholarship in the social sciences, particularly in ``co-productionist'' science and technology studies (STS).

Recently, Christias (2014) has argued for a way out of the stalemate between normativism and anti-normativism. He asserts that a careful reading of Sellars provides us with a much needed diagnostic, clarifying an ambiguity shared by those on both sides of the debate. Specifically, both normativists and anti-normativists conflate the ``conceptual'' reduction of normative facts to non-normative facts with ``causal'' reduction. Focusing on Stephen Turner's analysis in Explaining the Normative (2010), Christias shows how a Sellarsian perspective vindicates the core of the normativists' position; complete ``conceptual'' reduction to subpersonal facts or causal regularities is not possible, since such an explanation would simply ``change the subject'' away from normative content and its objective purport. Nevertheless, the normativist must admit that ``causal'' explanations of normativity are indeed possible and (contra the anti-normativist pretense) do not compromise the objective purport of normative commitments.

In this way, Sellars provides us with an alternative, often unacknowledged attitude towards the study of normativity, which O'Shea (2010) dubs ``Janus-faced.'' It is thus possible to causally describe the regularities associated with human normative behavior---we can do cognitive science or social science, etc---so long as we don't presume to have reduced ought-laden sentences to ought-free sentences or presume to have ourselves escaped normativity altogether. To continue the metaphor, we can describe one face while acknowledging the other. What is the result of this moderately normativist attitude? Christias (2014) claims that understanding the material and social conditions of our normative responses, far from undermining normativity, ``paves the way for its potential material realization, by liberating persons from unforeseen, uncontrollable, and hitherto completely unknown practical restrictions and impediments.'' As does O'Shea, Christias uses Sellars to hint at a mode of scholarship that uses science (perhaps social science) to understand normativity in a careful non-reductionist, and possibly emancipatory, mode. The potential of such Janus-faced research, however, need not be an object of mere speculation.

I suggest that much scholarship conducting social analyses of rationality, power, and technoscience already serves as proof of concept for Sellars' (and Christias') vision. Here, I mention only a few notable examples. Jasanoff (2004) has called attention to scholarly work that embodies ``the idiom of co-production.'' She explains that co-productionist analyses are those which do not presume that knowledge of natural world can be conceptually reduced to the effect of social norms or that the social world is just the product of natural causes; rather, they understand that ``lived `reality' is made up of complex linkages among the cognitive, the material, the normative, and the social.'' (p274) In short, they take nature (as we know it) and social order to be co-produced.

We can see this loosely Sellarsian perspective active in Ezrahi's Descent of Icarus (1990), when he reveals interdependency between the ``visual attestative'' norms of science and the legitimation of public action in liberal democracies. Or equally, we can see it in Visvanathan (2005), which also describes how norms of knowledge production and rationality are inevitably tied to political structures; he brings our attention to the way in which positivist epistemology has been associated with abuses of state power in India, with erasures of traditional agriculture and the creation of technological refugees. But these accounts do not aim to conceptually reduce the normative to the causal or to say that epistemology can be replaced with political science. Instead, they describe the social and political correlates that are left out of internalist accounts of normativity (of scientific knowledge practices, in this example).

Fitting with Christias' hint at liberation, Jasanoff (2004) asserts that co-productionist scholarship can actively feed back into society, enabling us to better understand our current situation and the possibilities for our collective future. As we endeavor to describe the way ought-laden sentences and normative facts travel together, in particular scientific rationalities, social and political structures, and local conceptions of human nature, we can do far more than just radical deconstruction and reduction. By maintaining an open mind towards the normativist side of Janus, our own commitments to robust knowledge or right behavior, we can use social scientific explanations to revise and further our own collective normative commitments. Co-productionist analyses, thus, can be seen as just one productive instantiation of a Sellarsian attitude towards social science. They show us how it is possible to study the external, causal side of normativity without giving up on normativity ourselves.

Author(s): Hardy Schilgen
Title: How to render theories of explanatory pluralism useful for scientific practice
Affiliation: Department of History and Philosophy of Science, University of Cambridge
Email: hschilgen@gmail.com

Abstract: Recently, the quest for various kinds of pluralism in the social sciences has increased significantly. This paper focuses on explanatory pluralism. More specifically it raises the following question: How to formulate a notion of explanatory pluralism in a way that best respects current social scientific practices? For even though explanatory pluralism is a normative (affirmative) statement about the plural status quo in the social sciences, it has to fit existing scientific practice to a considerable degree, in order to be of any practical relevance. I want to argue that most conceptions of explanatory pluralism put forward by philosophers of science to date fail to do precisely this. Their current treatments are based on a too narrow ('competitive') conception of pluralism, which cannot accommodate instances of ('complementary') pluralism common to existing scientific practice. This is problematic in that it prevents philosophers from raising philosophically interesting questions – questions that they need to answer in order to give sufficient guidance as to what qualifies as a pluralistic explanation in the first place and as to how exactly they are to be constructed.

Now, what are competitive and complementary pluralism? Competitive Pluralism holds that explanations at different organizational levels (e.g. individual micro- and institutional macro-level) are only weakly complementary "in virtue of possessing different explanatory virtues" (Marchionni, 2008: 315). While micro-explanations provide explanatory depth, macro-explanations are commonly held to satisfy explanatory breadth (e.g. Sober, 1999; Kitcher, 1991; Weber & Van Bouwel, 2002; Steel, 2006). Explanations at different levels, while not seen as mutually exclusive, are treated as separate and autonomous. Philosophers predominantly subscribe to such a competitive notion of pluralism (Marchionni, 2008; Mitchell, 2002), which admittedly correctly captures some scientific practice but fails to capture all of it. This is why I argue for a broader notion of explanatory pluralism that includes complementary instances of pluralism.

Complementary pluralism, instances of which are common in scientific practice, holds that different explanatory kinds complement each other in the satisfaction of one single epistemic virtue. Micro- and macro-explanations are not treated as separate and autonomous, but rather as entangled. As soon as they are integrated into one another, they lead to a better explanation. For now, let me just give one example. Social scientists have long studied the aggregate macro-relations between unemployment-levels and crime-rates. The causal direction is normally said to run from unemployment to delinquency. More recent theoretical models, however, suggest the opposite: Calvó-Armengol & Zenou (2003), for instance, explain the relationship between the two macro-variables 'crime-rate' and 'unemployment-level' through micro-determinants like 'job-search', which, in turn, depend on social-structural relations that individuals are embedded in. In other words, they improve a shallow macro-only-explanation by integrating it with a micro-explanation ending up with an explanatory broad and at the same time deep overall explanation of one phenomenon.

Now some might claim that the outcome is one single, unified mixed explanation that accordingly does not qualify as pluralist any more. However, I want to argue that this depends on the precise nature of integration and that not all mixed explanations are explanatorily monist. The kind of integration I subscribe to has been defended earlier by Mitchell (2002) and Bechtel & Hamilton (2007): while the micro-components of a mixed explanation (e.g., individual-level mechanisms) are contextualized by the macro-structure, the macro-components (e.g., social-structural terms) are complemented by the 'stories' that micro-mechanisms tell. The explanatory levels are neither reducible to one another, nor can they be separated. The kind of integration one ends up with amounts to something that Kincaid referred to as 'unity without reduction' (Kincaid, 1996). As a result, mixed explanations that are complementary in virtue of this very inter-level kind of integration keep two explanatory kinds (micro & macro) within the explanation of one single phenomenon and thus qualify as explanatory pluralist. However, in order to even recognize this, philosophers need to broaden their conception of pluralism by a complementary element.

Marchionni (2008) correctly notes that the literature on explanatory pluralism insufficiently deals with mixed explanations (325). My paper attempts to spell out some specific issues arising from this large-scale omission, including the following questions: How does the integration of explanations work? Can we stick to the traditional level-of-analysis conception, given that levels are often significantly entangled with one another? Philosophers need to tackle these questions in order to ensure sufficient guidance about the exact construction of (pluralistic) mixed explanations.

Beyond that, by their renewed acknowledgment of mixed explanations, philosophers of science may be able to finally recognize the empirical nature of the long-led methodological individualism-holism debate. For in mixed explanations, individualist and holist components are not mutually exclusive but complement each other within one explanatory account and are thus a matter of degree. Philosophers, however, have mainly (unsuccessfully) attempted to tackle the individualism-holism-debate by means of conceptual analysis aiming for a winner-takes-it-all solution in favour of either of the two (Kincaid, 2014). Instead, they should attempt non-general empirical answers

Author(s): David Sherman
Title: Elster and the Scope of Ambition in the Social Sciences
Affiliation: University of Montana
Email: david.sherman@umontana.edu

Abstract: Jon Elster's commitment to rational choice theory and to game theory as ways to illuminate both conceptual and empirical issues in the social sciences is too well known to need rehearsing, but in recent years his position has shifted rather markedly. As early as 2007, Elster downplayed the explanatory power of rational choice theory but still claimed that, if retained in a common sense way, it has the capacity to explain much of everyday behavior, has a significant conceptual value even when it does not explain very much, and reflects the human aspiration to be rational. His most recent project—"Excessive Ambitions"—seems to constitute another turn of the screw. In the two articles published under this title, Elster contends that the explanatory power of much of the social sciences and the normative power of all of ideal political theory are characterized by an unjustified hubris, and he aims to cut both down to size. Instead of prediction and explanation based on the uncovering of general laws, he claims that the social sciences should be contented with retrodiction and the establishment of mechanisms. And, instead of ideal political theory, which furnishes positive institutional designs, he argues for a negative institutional design that consists largely in the implementation of guardrails that are so commonsensical that opponents (if there are any) dare not publicly quibble. Although Elster's positions here do not suffer from the insufficient ambition that he finds in anthropology and other disciplines that embrace some variant of either poststructuralism or hermeneutics, they still suffer from an insufficiency in their own right.

In this paper, I shall primarily be concerned with Elster's first article, which deals with the social sciences, but I shall also consider his second article to the degree that it has a bearing on the first. In particular, I shall explore the upshot of Elster's social scientific approach, both in terms of its methodological assumptions and its substantive effects. I shall argue that his methodological assumptions, not the least of which is an individualism that seems to have become even more severe over time, are problematical; that the substantive outcomes that flow from his approach tend to have a strong conservative slant, as he concedes (albeit in a qualified way); and that the interaction between these assumptions and outcomes, which mutually inform one another, leave us with a social scientific approach that perpetrates the kinds of harms that he discerns in what he takes to be excessively ambitious social scientific approaches. I shall proceed as follows:

In the first section, I shall critically set forth Elster's position. According to Elster, when rational choice theory fails to adequately explain behavior, which he believes often occurs in important cases, it is because one or both of its basic assumptions, that the theory must yield a determinate prediction and that the agent is rational, do not hold. After briefly considering the problems of indeterminacy and irrationality as Elster advances them, I shall consider them within the context of what I take to be his overweening individualism, which in my opinion exacerbates the very problem that he diagnoses. I do not disagree with methodological individualism per se, and shall offer a "concessive" individualism that makes better sense of the various problems that Elster diagnoses than his more stringent variety. My own approach does not sacrifice Elster's demand for "microfoundations," but it does thicken up his rational agent. This enables the social scientist to make better sense of the agent's "irrationalities," and therefore supports better predictions.

In the second section, I shall consider these problems within the framework of economics. As an initial matter, I shall argue that Elster is right to be wary of extrapolating from the experimental findings of behavioral economics and applying them to real life contexts, and that he is definitely right to call into question the reliability of the data that the discipline generates and relies on in making its predictions and justifying its theories. Indeed, in regard to the latter, he understates the problem. The basic problem that Elster himself confronts is that, given his own assumptions, he is left with very little to work with: unable to start from the idealized assumption that is homo oeconomicus or from the less idealized assumption that is homo oeconomicus as modified by the various sorts of irrationality generating mechanisms that the behavioral economics literature has discovered, his agent is radically underdetermined, and this necessarily has serious implications for his theory.

In the third section, I shall pick up this theme, tie it back to Elster's severe individualism, which I take to be the source of the problem, and then discuss his concept of society, which inexorably follows from this view. Drawing on the work of Martin Hollis, I shall argue that because Elster fails to consider the "rational" agent in a more nuanced way, which is to say not just as homo oeconomicus but also as homo sociologicus, he is forced to restrict the scope of ambition in the social sciences unduly. Although homo oeconomicus and homo sociologicus do not seamlessly dovetail, and mediating them does not surmount the problems of indeterminacy or irrationality, it does restrict their play, and therefore constitutes theoretical progress.

Finally, in the fourth section, I shall consider the relationship between the two types of excessive ambition that Elster articulates, social scientific and political theoretic. Elster claims that these two types of excessive ambitions parallel one another but in fact they do far more: they mutually reinforce one another. Restricting political theory to a wholly negative institutional design has a conservative slant that enforces the sorts of methodological strictures that he brings to bear in the social sciences. Elster fails to recognize, as certain thinkers that he calls "soft obscurantists" do, that the facts of the social sciences and the values of philosophy interpenetrate one another to a far greater degree than he allows.

SYMPOSIUM
Contributors: Karsten Stueber, Paul Roth, and Jouni-Matti Kuukkanen
Title: The Nature and Scope of Narrative Explanations
Affiliation(s): College of the Holy Cross., UC Santa Cruz, and University of Oulu
Email: kstueber@holycross.edu , paroth@ucsc.edu, and jouni-matti.kuukkanen@oulu.fi

Abstract: In the last decade, narrative has become a rather popular topic for researchers from a variety of disciplines. It seems that a narrativist account for or approach to almost anything seems to exist. Surprisingly however, the contemporary debate about narratives still has to come to terms with some of the insights from the prolonged discussion in the philosophy of history about the nature of narratives, their explanatory power, and their epistemic contribution to an exploration of the world. Our symposium hopes to address this lacuna by exploring the question of whether (and if so, how exactly) we should understand the explanatory power of narratives and their supposedly unique epistemic contributions. The panel will consist of the following three papers.

Paul Roth will start the panel with his paper "Reviving the Philosophy of History. His call for such a revival will address the following two questions: first, what exactly would this revival revive; and, second, why bother? Those skeptically inclined might counsel indefinite postponement, inasmuch as this subfield has remained mostly deserted since the 1970s. Roth's primary concern will be to outline the current status of key issues raised by the first question, for the purpose of identifying those aspects within philosophy of history that both merit and demand renewed philosophical consideration. In particular, his paper reconsiders questions tied to the use of narrative as a form of explanation. Specifically, Roth focuses on those features that make historical explanation distinctive and yet belong on any satisfactory catalogue of explanatory strategies. He directly addresses an epistemic question that he takes to be of central philosophical concern, viz., in what respects explanations in narrative form can be said to offer credible justifications. Answering this requires a turn away from narrative theory and back to neglected works by Arthur Danto and Louis Mink, since, as Roth will show, their work provides important and still crucial insights that can be deployed to fashion answers to philosophical concerns about narrative explanation. Roth will conclude with two examples of what he claims to be exemplary explanations in narrative form.

In his talk on "The Cognitive Function of Narratives," Karsten Stueber will continue the exploration of the uniquely epistemic contribution of narratives. He will show that it is a mistake to focus only on narratives about human agency if one intends to grasp a narrative's specific explanatory power. Nevertheless, Stueber maintains that we have to utilize the central historicist insight about the nature of the historical world and historical writing in articulating the cognitive function of narratives. Stueber will argue that full-blown narratives are best understood as developmental portraits of a chosen entity/ unit in respect to its individuality. The argument will proceed through a critical analysis of the debate between Noel Carroll and David Velleman about the nature of the narrative connection and the question of whether the explanatory force of a narrative has to be understood in causal or emotional terms. Stueber will side with the causalist in this respect but will also show that we need to be very careful in distinguishing between causal explanations underwritten by a theory and the use made of such causal accounts within the context of narratives concerned with explicating individuality. Accordingly, Stueber agrees with Mink that narratives are special cognitive instruments. Yet Mink's characterization of narrative understanding as a "configurational mode of comprehension" that is strictly distinguished from the theoretical mode needs to be amended. Narrative understanding should be conceived as an autonomous and irreducible mode of comprehension. At the same time, it should be viewed as being dependent on a variety of theoretical perspectives it intricately uses. Stueber will conclude his paper by pointing to a number of distinguishing marks of narratives about human agency.

In the last talk of the symposium, "The Narrativist Insight and Postnarrativism in the Philosophy of Historiography," Jouni-Matti Kukkanen will be a bit more circumspect about the explanatory power of narratives and their assumed ubiquity in historical writing. As he argues, holism of narratives or representations implied by narrativism has a number of problematic practical consequences in historiography. These consequences are studied by examining E. P. Thompson's The Making of the English Working Class and Christopher Clark's The Sleepwalkers: How Europe Went to War in 1914. When all the statements or details of a book define a narrative or a representation, it is impossible to understand the central thesis of the book without having mental access to all these definitional components. It follows that, in practice, it is impossible to communicate a historiographical thesis to others. Further, no one can understand or memorize what historians argue for. Kuukkanen suggests that it is better to make a distinction between the meaning of a thesis and evidence for the thesis. Another problem of narrativism is that a commitment to narratives implies that historians necessarily describe events in temporal succession. Kuukkanen proposes that it is argumentativity rather than narrativity that characterizes historiography. Historians use different forms of reasoning in their attempts to persuade readers to accept a certain view of the past. In sum, while Kuukkanen does not want to deny completely that narratives may be a specific form of explanation in historiography he questions whether it is the kind of explanation that characterizes historiography in general. At most, narrative and narrative explanation may add further force to the theses argued for in a work of history.

Author(s): Johanna Thoma
Title: Credible Worlds and the Hidden Thought Experiments of Economic Theory
Affiliation: University of Toronto
Email: johanna.thoma@mail.utoronto.ca

Abstract: Most papers in theoretical economics contain thought experiments. They take the form of more informal bits of reasoning that precede the presentation of the formal, mathematical models these papers are known for. These thought experiments differ from the formal models in various ways. In particular, they do not invoke idealised assumptions about the rationality, knowledge and preferences of agents. The presence of thought experiments in papers that present formal models, and the fact that they differ from the formal models in this way are often ignored in debates on what, if anything, we can learn from formal models in theoretical economics. I show that paying due attention to thought experiments in theoretical economics has serious implications for this debate. Differences between thought experiments and formal models are especially problematic for Sugden's 'credible worlds' account.

The paper proceeds as follows. After introducing a paradigmatic paper from theoretical economics that does consist of a thought experiment - formal model pair, I compare the two and also explain why the former counts as a thought experiment while the latter does not. The formal model and the informal story share the following standard features of (thought) experiments: They start by describing a setup that is to some extent idealised or hypothetical. There is then a manipulation, after which we observe a thought experimental/model phenomenon. Lastly, in both cases, if our aim is to explain a real world phenomenon, there is still an inductive leap to be made from the thought experimental/model phenomenon to the real world phenomenon.

However, as Brown (1991, chapters 1 and 2) highlights, another important feature of thought experiments is that the thought experimental phenomenon is established purely in the imagination, and that the mental manipulation involved in establishing it is not theory-based. I argue that this condition is not met by the formal modals of economic theory, insofar as they feature perfectly rational agents. The agents featured in economic models are unlike real world agents in that they abide by the axioms of decision theory, are usually assumed to have perfect knowledge of most factors relevant to their decisions, and are typically greedy and self-interested. Because of these differences, I argue that we do not have pre-theoretical intuitive access to how these idealized agents behave. Trained economists can work out in their imagination how such agents behave, but this is only due to their experience in applying economic theory. In contrast, the informal bits of reasoning at the beginning of papers in theoretical economics don't usually make idealising assumptions about agents, and instead appeal to our intuitions about the behaviour of real world agents in order to establish a phenomenon. I argue that they thus count as thought experiments.

The paper next draws out the implications of this difference for the debate on the epistemic status of 'unrealistic' models in economics. First, the fact that thought experiments are presented in the exposition of formal models tells us something about the aspirations economists have when they construct models. The thought experiment concerns a choice situation that could be real, and employs the reasoning patterns real agents may use. It thus seems like economists think they are in the business of directly explaining real world phenomena, contrary to some accounts of the purpose of models in theoretical economics. At the same time, the differences between the thought experiment and the formal model cast doubt on at least one recent and influential account of how we learn from economic models that does take seriously the ambitions revealed in the thought experiments that precede formal models. Robert Sugden's 'credible worlds' account (see Sugden 2000, 2009) claims that economists construct alternative worlds that act as analogies to various real world phenomena. We are licensed to make inferences from these analogous worlds because we take them to be 'credible'. Sugden thinks that the thought experimental stories that economists tell around their models establish this credibility. Since he assumes that thought experiment and model are just variants of the same thing, the thought experiment's credibility transfers onto the formal model.

I argue that this assumption is not warranted. The thought experiment and the formal model are different, and they differ precisely in their credibility. The thought experiment does not establish the credibility of the model, and it is not clear what else could. In a final section, I suggest that a more plausible role for the thought experiment is twofold: It acts as independent evidence for the hypothesis the model wants to establish, and it acts as an example of a more realistic case the formal model does apply to, making it more convincing that the model applies to real world phenomena. But what makes it the case that the formal model applies to the thought experiment is still an open question, and one that is no easier to answer than the question of how the model applies to the 'real world'.

Author(s): Jack Wright
Title: Is mathematical modelling inherently unsuitable to social science?
Affiliation: University of Cambridge
Email: jw675@cam.ac.uk

Abstract: What, if anything, is wrong with economic modelling? A number of authors have been critical of the use of mathematical models in economics, and some have even rejected their use outright. These criticisms have been motivated by (among other things) a perception that economics is too narrow in its methodology, a failure of economics to predict a number of major economic events, and a supposed explanatory paradox (e.g. Lawson (2006) and Reiss (2012)). Yet, in order to avoid the rejection of mathematical modelling in other sciences, many of these critics have suggested reasons why modelling in economics stands out as particularly problematic. This paper will examine a central proponent of this form of reasoning: Tony Lawson and his arguments from Ontology (2006, 2003). Considering economists and their mathematical tools as coupled systems will allow me to question this reasoning by highlighting the importance of analysing the use of mathematical modelling, as opposed to the features of mathematical models in isolation.

Lawson starts form the simple idea that successful knowledge practices use tools and methods that are appropriate for the nature of what they are studying. He combines this with the argument that the nature of social reality is such that mathematical modelling is not a suitable tool of analysis for it, and concludes that the failures of economics are partly down to its methodological choices (the use of mathematical modelling). The intuition behind Lawson's simple starting point seems hard to argue with, so many critics of his position have focused on the natures of social reality and mathematical modelling. I will follow them in doing so, but the conclusion of my argument will also highlight a serious problem for Lawson's starting point.

Lawson's argument that the nature of social reality does not fit with mathematical modelling is based on the dual premises that (a) social reality is inherently open, i.e. it is not atomistic, and that (b) mathematical modelling presumes a closed ontology — i.e. has event regularities of the sort 'if x then y' — in its target domain. By treating economists and their tools of analysis as singular coupled systems I will challenge (b) — and implicitly challenge the reasoning involved in (a). The coupled systems approach assigns ontological unity to agents and the tools they use frequently in order to understand the properties and capabilities they have together as a virtue of their repeated interaction. I will argue for such an approach by repeated use of examples that show that Lawson's description of mathematics assumes a more static picture of its use than is fair, and that the coupled systems approach provides a more active, use-based, and realistic way to analyse mathematical modelling (and other sciences). This will align me with a large body of literature on the philosophy of science in practice (Chang, 2012) and with embedded/extended mind accounts of agency, action and tool use (Clark & Chalmers, 1998).

The main thrust of the argument will be that Lawson's account artificially separates models from those that use them, and that when considered together the use of such modelling does not need to assume a closed ontology. By arguing that mathematical reasoning can do more than Lawson suggests, the coupled systems approach partially supports others that challenge (b) (e.g. Vromer and Caldwell's pieces in Fullbrook (2008)). It goes beyond them, however, by emphasising that it is in the interaction between economists and their models that this can be the case. Thus, we can buy Lawson's argument that mathematics assumes a closed ontology in isolation, without needing to say that it does so in use. To conclude, I will suggest that the coupled system approach signals trouble for any position that assumes that the successful use of modelling is a function of the right relationship between the intrinsic features of said models and the intrinsic features of their domain of application. The multitude of coupled systems scientists can make with mathematical models suggests that such an assumption should be treated as, at the very best, an open question.

This paper should not be taken as a wholesale rejection of Lawson's ideas, I share his intuition that mathematics need not always be the best tool for analysing features of the social world. But, this depends on the specificities and goals of the analysis, and it is an open question as to whether rules regarding such context dependence can be determined by prior reasoning. Furthermore, a use based and pragmatic approach to knowledge would suggest that even in the specific contexts for which it can be determined that mathematics is not the best tool, it might still be beneficial (relative to the context dependent goals) to use some mathematical analysis. In many cases using multiple tools simultaneously (or at least in parallel) might best attain the goals of the analysis (Chang, 2012; Kellert et. al., 2006). Part of the motivation for Lawson's argument, the perception of narrowness in economics, might well be correct, but Lawson ends up assuming a position that supports a component of that narrowness. The benefits of employing pluralist ontologies and methodologies are more readily available if one takes a pragmatic approach to tool use.