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ABSTRACTS

Elizabeth R. Blum and Rajesh K. Kana (University of Alabama at Birmingham): Conceptual Issues in Psychiatric Disorders: The Curious Case of Autism
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According to the DSM-IV, autism is a developmental disorder characterized by impairments in reciprocal social interaction, language, and communication; restricted interests; and repetitive behaviors. Despite being the subject of active scientific investigation (including behavioral, clinical, genetic and neuroimaging approaches), autism remains enigmatic to researchers and families, and has consequently generated intense public interest. While much progress has been made in the characterization of autism since Kanner's (1943) initial description, conceptual issues remain. In this paper, we investigate how the problems associated with the conceptualization of autism may affect the nature of continuing empirical research. We will discuss several issues that are relevant to questions of measurement and other areas of philosophy of social science. The argument, which is informed by empirical research from our group, is motivated by a desire to generate a conceptual framework that may be more useful in scientific practice.

Spectrum: A fundamental tension can be seen in the question of whether autism ought to be characterized as a set of categories or as a continuum. On the one hand, the DSM-IV places the disorder in a categorical format under the umbrella *Pervasive Developmental Disorders (PDD)*. On the other hand, PDD encompasses several disorders with overlapping symptoms and characteristics. For this reason, PDD disorders are often discussed as "Autism Spectrum Disorder" (ASD), which suggests a range of conditions falling on a continuum. In addition, ASD comprises disorders with huge variation in abilities and impairments, which further supports the idea of "continuum". This tension between the "continuum" and the categories is problematic in terms of diagnosis, intervention, education and treatment, and intersects with the issues outlined below.

Variability: Although there are sound reasons, including much clinical and experimental research, for treating autism spectrum disorder as a homogeneous category, autism differs from other developmental disorders in that its presentation is highly variable across individuals. This heterogeneity is surprising given the fairly concrete evidence for both a common developmental course and a strong genetic component to the disorder. Clarity in the neurobiological profile of autism has also been elusive, especially the efforts to track autism down to a modular brain abnormality. The variability is implicitly acknowledged by the highly individualized treatment approaches in autism. Yet, the variability in the manifestation of the disorder is challenging since clear definitions and categories are required when diagnosing and classifying (for instance, the absence of definitions might lead to mis-, under- or over-diagnosis), and ultimately treating a patient. The variability also creates difficulty for research studies (whether behavioral or neuroimaging) that focus on data averaged across a sample. This may lead researchers to commit the "ecological fallacy": a mistaken inference from properties of the population to properties of the individuals in it. In addition, for the obvious practical reasons, many research studies focus on high-functioning individuals, which may lead to biased results when the findings are assumed to be applicable to the entire autism population. Another problem relates to the tendency to generalize brain results across individuals, groups, or populations, despite our knowledge that brain structure is highly variable. In other words, the focus when reporting results is always toward the forest with little attention to the trees.

In sum, autism spectrum disorder presents a fine case for examining conceptual questions in the philosophy of the social sciences, in particular as it applies to psychology and psychiatry. We will discuss these issues in the context of our neuroimaging and behavioral studies of autism, with the hope that doing so will ultimately help researchers, patients, and families.

Michael Brownstein (Pennsylvania State University): Conceptuality and Practical Action:
A Critique of Charles Taylor's *Verstehen* Social Theory
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"Between ethos and logos, practical mastery and verbal mastery, there is a radical discontinuity." –Pierre Bourdieu, *Distinction*, 461

John McDowell and Hubert Dreyfus' recent debate about the conceptual content of ordinary coping has clarified the meaning of an engaged conception of agency, but not enough attention has been paid to the debate's consequences for normative social theory. In his critique of McDowell's position that ordinary coping is "permeated with mindedness," Dreyfus argues that ordinary embodied skills – like throwing a ball – are often transformed for the worst when we consciously attend to our activities. Dreyfus' counterexample to McDowell recounts the story of Chuck Knoblauch, a professional baseball player who purportedly could not make a throw to first base whenever he thought too much about making the throw. The story is meant to suggest that *adding* higher-order thought to ordinary skillful activities has potentially deleterious effects on those activities. In Dreyfus' words, "the enemy of expertise is thought." If correct, Dreyfus' claim has far-reaching implications for those normative social theories aimed at clarifying, interpreting or articulating the meaning of social practices.

Charles Taylor's *Verstehen* thesis is one of contemporary philosophy's most celebrated normative social theories, but it partakes of a long-standing and problematic prejudice in Western culture and philosophy which the debate between Dreyfus and McDowell clarifies. That prejudice assumes that our lives and our practices are always improved when their meaning is made explicit. Taylor's overarching claim – that working towards maximal interpretive clarity of the "common meanings" and "motivating ideals" which purportedly make social practices what they are – problematically obscures the class of social skills which are distorted and perhaps even disabled by the activity of interpretation and higher-order reflection. Taylor recognizes that many common meanings and ideas are indeterminate or opaque, but he does not recognize that they may be – to borrow a phrase from Merleau-Ponty – *positively* indeterminate.

Taylor's failure to recognize the positive indeterminacy of ordinary skillful activity suggests that normative social theories in general must look beyond interpretation in order to clarify their purpose and their methods. They must consider both the gains *and* losses of theoretical activity itself.

Naomi Choi (University of California, Berkeley): “Is Moral Ontology Necessary for Moral Philosophy?: Evaluating ‘Strong Evaluation’”
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Charles Taylor’s defense of anti-naturalism in the philosophy of social sciences is based on his ontological claim that human beings *are* self-interpreting ‘strong evaluators’ – the actions/meanings of whom only an interpretive human sciences could possibly elucidate. In his arguments about the double hermeneutic in the human sciences, Taylor asserts that strong evaluation is undeniable about persons, and tries to convince us that any view of selfhood– even utilitarian or existentialist varieties of weak evaluation – presupposes it. We are always already strong evaluators such that even the disengaged view of selfhood has something that is necessarily strongly valued on some deeper level that it outwardly denies, be it instrumentality, efficiency, or a zeal for absolute freedom. But Taylor’s ontological claim of strong evaluation, the basis for his anti-naturalism in the human sciences, has equally deep and critical implications for moral philosophy as for philosophy of social sciences, the most significant of which is that the two are inextricably intertwined.

This paper examines the relationship between Taylor’s ‘strong evaluation’ as a claim about explanation in the human sciences and one about moral philosophy, understood as the “background languages” or “inescapable frameworks” in which we live, reflect on, and debate morality. Taylor’s latter claim is that our moral descriptions, reactions, questions, issues, in short - morality - involves discriminations of right or wrong, better or worse, higher or lower, which are not rendered valid by our own desires, inclinations, or choices, but rather stand independent of these, offering standards by which they can be judged. Taylor takes from Hegel’s conception of reason that the only kind of argument that can be offered for our moral ontology as strong evaluators is historical: an argument for or against a particular ontology of selfhood cannot be made merely from philosophical theory but must come from a broader historical articulation. Taylor’s *Sources of the Self*¹ is an attempt to do just this, by narrating the historical changes in conceptions of selfhood from antiquity to the present that explains how each historically new understanding of selfhood derives from and underpins a particular view of human agency along with a new conception of society and understanding of social bonds, a vision of the good, and new forms of narrative in which people understand their lives. But how can Taylor’s historicized ontological argument for the essentially moral attributes of personhood convince us of the moral outlooks behind our own moral preferences? What exactly can be said to be constant about human agency and social practices in light of the historical, changing aspects of being? And what, if anything, do such ontological arguments entail for how we should debate morality and politics?

¹ Taylor, Charles. *Sources of the Self: the Making of the Modern Identity*. Cambridge: Harvard UP, 1989.

Gerard Delanty (University of Sussex) and **Piet Strydom** (University College, Cork): The Challenge of Methodology for Critical Social Theory

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The central challenge of critical social theory for the social sciences is to offer a new methodology. The tradition of critical social theory associated with Hegelian Marxism and represented by the Frankfurt School and Habermas has lost its capacity to offer a methodological framework for social scientists and has been overtaken by more successful philosophical approaches, such as critical realism, which have articulated a more robust integration of epistemology and research. While Habermas's concerns have shifted towards normative political philosophy, his later work - despite the apparent abandonment of sociological research that was an integral to the older critical theory tradition - offers a good basis to rethink the methodological implications of the Hegelian Marxist philosophy of social science. In this paper we argue that despite the apparent retreat of critical theory into normative philosophy there is a promising opening to empirical social research. This is made possible by the confluence of critical social theory with the American tradition of pragmatism, the interaction of which has been integral to the work of Apel and Habermas over the past two decades or so. We argue that the core of the general methodological framework of critical theory is the Hegelian Marxist notion of immanent transcendence and, moreover, that this is shared with pragmatism in the common concern with linking the normative and empirical dimensions. Although pragmatism had a methodological impact of its own, in the interpretation of that tradition (in particular work of Peirce by Apel and Habermas) a new and more compelling methodological relevance can be discerned in the epistemology of social science outlined especially by Apel, who has drawn attention to the sign mediated nature of social reality as proposed by Peirce: all knowledge production starts from some (i) sign (e.g. a problem, crisis) which requires (ii) interpretation in order to arrive at (iii) the reality to which it refers. Pragmatism, like critical theory, places a strong emphasis on reality as something that social actors have to engage with and knowledge of which must be abductive as opposed to either deductive or inductive. Immanent transcendence is the master theoretical concept which concretises the relation between the epistemology and the methodology of critical theory to include both the immanent (empirical, grounded) and the transcendent (normative, counterfactual). It thus stands for the way in which the epistemology and theory can be brought in a methodological direction, i.e. in a way that lends itself to social research. The details of this will be explicated in the paper.

Brian Epstein (Virginia Polytechnic Institute and University): History and the Critique of Social Concepts
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An obvious goal of social criticism is to correct erroneous beliefs and change the features of society that produce or reinforce them. A target of critique might be the mistaken belief that certain racial groups are innately more intelligent than others, or the plausible but incorrect idea that policies encouraging fiscal responsibility and increased household savings will pull us out of the looming depression. In recent years, it has become evident that social critique may take a different form as well. Instead of being directed at false belief, it may take the form of criticizing the possession or employment of certain social concepts or social terms. Sally Haslanger has recently discussed the example of a twelve-year-old telling her mother, “crop-tops are cute!” – where it is a matter of some importance to the girl that she be cute as opposed to dorky, and is concerned that if she fails to wear a short cut-off shirt, then she will not be cute. The mother may correctly reject the daughter’s judgment, in its tacit participation in a culture of sexualizing young girls. Yet at the same time the girl’s assertion is at least in some sense correct, a true belief she holds. Regardless of the truth or falsity of an assertion involving a concept such as *cute*, it is the uncritical possession or employment of the concept *cute* itself that is problematic.¹

Historical or genealogical investigations have long been regarded as well suited to the criticism of social concepts. It has remained a puzzle, however, what bearing an account of the historical development of a concept can have on criticizing the concept. While it may be interesting to study the factors motivating the development of a concept, unless it is shown that the unfortunate historical features of some concept’s development persist today, then it is unclear how these features can serve as the basis for criticism of the concept. And yet if the unfortunate features do persist in the contemporary practices, then it is unclear why genealogy should be an important tool for investigation, as opposed simply to putting the contemporary practices under close scrutiny.

My aim in this paper is to defend the unique suitability of genealogy for criticizing certain social concepts. And in defending this, I aim to make progress in clarifying some aspects of the individuation of social concepts and the semantics of social terms. In particular, I untangle how certain aspects of the development or generation of a concept may just be interesting factoids, while other historical factors may be constitutive of the concepts, figuring into the individuation of the concepts. These two aims – methodological and semantic – are connected, in that when historical factors are individuating, there may be no other way of describing or analyzing the concept they individuate except with a genealogy.

I do not argue that genealogies have just a single function. There are circumstances in which they are useful even when historical factors fail to figure into the individuation of some concept. For instance, historical investigation could demonstrate the likelihood that a concept is employed oppressively by showing that it is unlikely that we would have the concept were the oppressive practices not present. But this use of genealogy is arguable dispensable, in that the same conclusion could be drawn with non-genealogical methods. Where historical factors are individuating, on the other hand, it can be that satisfactory conceptual analysis *requires* empirical historical investigation. This means that genealogy may be essential to conceptual criticism, in that it is essential to conceptual analysis.

In approaching projects of conceptual criticism, Haslanger has argued for a number of useful distinctions. She advocates an externalist treatment of social concepts, and distinguishes the project of correctly *describing* social concepts from two other projects that are often conflated with it: the (confusingly named) *conceptual project*, which treats our internal understanding of social concepts, and the *ameliorative project*, which seeks to modify the concepts we possess for social improvement. She correctly notes that a proper approach to the descriptive project is essential for conceptual criticism. However, Haslanger (along with other theorists who stress the importance of including the “social matrix” in conceptual analysis) pays insufficient attention to the distinctions among various kinds of historical and other social factors, and thus fails to demonstrate that any are individuating. With an externalist treatment of social concepts, it is natural to suppose that there are historical factors in concept individuation. But the fact that the concept *cute* is employed in oppressive social contexts does not imply that those oppressive factors figure into the individuation of cuteness, any more than the fact that racists use paperweights implies that racist practices or ideologies figure into the individuation of *paperweight*, or any more than the fact that I wash my car with water implies that cars or suds figure into the individuation of the concept *water*. By untangling the nuanced distinctions between different roles that historical (and other) factors play in determining conceptual content or linguistic meaning, I aim to show how some social and historical factors come to figure into the individuation of social concepts, while others may be relevant for certain purposes but are not individuating. If we are to describe and criticize social concepts, genealogy is sometimes required, but care must be taken, since the history and social practices that surround a concept are often incidental or irrelevant to its description.

¹ This is somewhat different from Haslanger’s point with the example, but it is a useful example to appropriate for these purposes as well as hers.

David Henderson (University of Nebraska, Lincoln): Rationalizing Explanation and Rationality Naturalized
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When explaining some action or thought of an agent, one often notes antecedent beliefs and desires that would make that action or thought rational. Some have suggested that finding agents rational provides the “only clear pattern of explanation that applies to the mental” (Davidson), and that this model is equally central within the human sciences generally—making for an explanatory practice that can neither be significantly refined nor transcended without loss of the subject matter of those sciences (Rosenberg). I have elsewhere argued that it is not findings of rationality per se that are explanatory; rather, what is explanatory are findings of ways of reasoning (cognitive processes) that conform to what we have come to know and expect about human cognition. However, there remains a matter that has not been adequately explored in discussions of rationalizing explanation. It has to do with whether it might be better understood when thinking of rationality in a naturalized fashion, rather than along more traditional lines.

Most writings on rationality and explanation have supposed that what makes for rationality is to be understood drawing from disciplines that can and do proceed independently of significant empirical information regarding humans as a class of limited cognitive systems. Commonly, the understanding of rationality has come from some combination of first-order logic, the probability calculus (including Bayes’ Theorem), and utility theory or decision theory. These (typically treated as the verdicts of a priori reflection) have been thought to provide some reasonable measure for what would be objectively rational. However, in epistemology, and increasingly in accounts of normative rationality more broadly, one finds the realization that fitting normative standards for epistemic and practical rationality need take into account the resources of real cognizers—typically humans as a class of cognitive systems (Quine, Harmon, Cherniak, Goldman, Henderson and Horgan). Does the new realism in accounts of rationality (associated with naturalized epistemology) itself significantly improve the prospects for unproblematic forms of rationalizing explanation? Do earlier misgivings about rationalizing explanation ring hollow when the rationality to be attributed is naturalized? I here argue that, while explanation in terms of naturalized rationality would be free of one fatal flaw had by explanation in terms of rationality understood in the traditional fashion, it would yet have parallel flaws.

Carole J. Lee (University of Washington): Methodological Rationalism as Galilean Idealization in Psychology
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Methods of Galilean idealization lie at the heart of modern scientific and social scientific modeling and theorizing. Within the social sciences, methodological debate about the epistemic and metaphysical features of Galilean idealization arise primarily in the context of economic theorizing, which adopts distorting idealizing assumptions about the nature of the market (e.g., that it is in perfect equilibrium) and about the nature of economic agents (e.g., that they are perfectly informed, self-interested, and utility-maximizing). However, less work has been done to articulate the methodological issues that arise with respect to Galilean idealization in the context of psychological theorizing. In this paper, I analyze methodological rationalism as a form of Galilean idealization in psychological theorizing. By analyzing methodological rationalism's power and limitations in capturing different types of psychological problems/phenomena, I aim to articulate its proper use in psychological interpretation and explanation more generally. Although some of my case studies are historical in nature, my analysis should be relevant to the body of contemporary work in psychology and cognitive science that models human judgment, learning, and decision-making in rationalizing and simplifying ways.

To begin, I recast Max Weber's approach to methodological rationalism as a form of Galilean idealization. Galilean idealization involves the deliberate simplification of the properties attributed to a phenomenon in order to gain a better understanding of the features of the real world or, less ambitiously, of the idealized model at hand.² From a methodological point of view, Weber's methodological rationalism works to simplify the factors and motivations considered in modeling and explaining rational and irrational action.³ Analogously, Davidson's Principles of Charity, invoked to address the problem of radical translation, simplify the domain of relevant factors in the attribution of belief and meaning.

I then compare how Davidson's rationalizing assumptions work in the context of two very different problems of interest: namely, radical interpretation versus Subjectively Expected Utility (SEU) Theory.⁴ Unlike his *a priori* thought experiment about radical translation, Davidson's SEU Theory was an experimentally-oriented project developed in conjunction with Patrick Suppes for psychologists researching choice under uncertainty. In light of my analyses of these projects, I argue that, although methodological rationalism can help to motivate answers to the "how possibly" questions raised by radical translation, empirically naïve approaches to methodological rationalism face predictable limitations when it comes to "how actually" explanations (illustrated most directly by Kahneman and Tversky's research on framing effects). I argue that, in the case of "how actually" styles of explanation, rationality assumptions should be empirically-informed and thereby "de-idealized." Such "de-idealization" suggests the possibility of achieving realist, albeit modest, rationalizing models of human action.⁵ In contrast, I argue that our interests in answering "how possibly" questions need not call for de-idealized assumptions, as rationalizing models in cognitive science and artificial intelligence suggest. Whether these types of methodological rationalist models should be considered real or fictional remains open to further investigation.

² Cartwright, Nancy (1989). Nature's Capacities and their Measurement. Oxford, U.K., Oxford University Press.

³ Weber, Max (1947). The Theory of Social and Economic Organization. New York, NY, Oxford University Press.

⁴ Davidson, Donald (2000). Psychology as Philosophy (1974). Essays on Actions and Events. Oxford, UK, Oxford University Press: 229-38. Davidson & Patrick Suppes (1957). Decision Making: An Experimental Approach. Stanford CA, Stanford University Press.

⁵ Rationalizing models of human action and decision are, in the Philosophy of Mind, characteristically associated with anti-realist approaches to mental states and explanations. For example, see Botterill, G. and P. Carruthers (1999). The Philosophy of Psychology. Cambridge, UK, Cambridge University Press. For a de-idealizing, realist approach to Galilean idealizations in the context of Physics, see McMullin, E. (1985). "Galilean Idealization." Studies in History and Philosophy of Science **16**(3): 247-273.

Daniel Little (University of Michigan-Dearborn): Thinking social ontology
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The subject of social ontology has played second fiddle to epistemology and the theory of explanation in the philosophy of social science. However, the time is ripe for philosophers to think more seriously about the nature of the social world – to think through a better social ontology. The social world has characteristics that fundamentally distinguish it from the natural world – heterogeneity, plasticity, and contingency, to name several. We need to ask fundamental questions about what social phenomena are composed of and what kinds of causation are at work within social processes.

Why are these questions important to the philosophy of social science? And how could they contribute to better research and theory in the social sciences? One answer is that it isn't really possible to investigate any domain without having some idea of what sorts of things the domain consists of. So attempting to arrive at more perspicuous models of what the social world is made up of is a necessary step on the way to more specific forms of empirical and causal research. A second answer derives from recognition of the harm that has been done to the cause of knowledge by misconceived ontologies in the history of science. This has been especially true in areas of knowledge adjoining human life and activity -- radical behaviorism in psychology, naturalism and positivism in sociology, and what Andrew Abbott calls the "variables paradigm" in quantitative social science. Better science will result from more propitious ontology, because we won't be in the situation of trying to force the social world onto the wrong sorts of boxes. Researchers will be encouraged to explore multiple methodologies and theories when a more satisfactory social ontology guides them. Theoretical pluralism finds strong support in the ontology explored here, given the emphasis offered to social heterogeneity and contingency.

The paper will sketch a view of the compositional nature of social phenomena. It begins with the theory of "methodological localism" as the most fundamental level of social stuff. It represents higher-level social entities as being composed of a heterogeneous mix of mechanisms and processes. It finds a place for a variety of forms of social causation, resulting in a view of social wholes that leads us to expect heterogeneity, plasticity, contingency, and path-dependence in social organization and development.

So, to return to the original question: it is in fact very appropriate and potentially helpful for philosophy of social science to give more attention to social ontology than it has traditionally done. The social world of the twenty-first century is chaotic and rapidly changing, and we need better mental frameworks within which to attempt to make sense of this complexity and change.

Brandon Morgan-Olsen (University of Washington): Epistemic Injustice and Public Reason
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Deliberative democratic theorists typically use accounts of public reason – i.e., constraints on the types of reasons one can invoke in public, political discourse – as a tool to resist political exclusion; at its most basic level, the aim of a theory of public reason is to prevent situations in which powerful majority groups are able to justify policy choices based on reasons that are not even assessable by minority groups. Using Miranda Fricker’s recent analysis of epistemic injustice⁶ as a point of departure, I argue here that – contrary to the standard view – the invocation of a public reason constraint has the potential to further exclude already marginalized groups. Thus, taking epistemic injustice seriously requires us to revise the very idea of public and non-public reason.

Fricker describes structural inequities that negatively impact one’s abilities and activities *as a knower*, but does not explore how these limitations on one’s knowing can affect one’s political agency. Accordingly, I demonstrate that the structure of a society’s conceptual framework can have far-reaching political implications, implications which extend beyond the injustices Fricker identifies. Specifically, epistemic injustice – and related phenomena – can work to politically exclude citizens both by inhibiting their ability to express certain political claims and by reducing the likelihood that their political claims will be easily assessable by the public at large. I take this latter point to be of particular interest, because it is what raises complications for the formulation of a workable public reason standard.

⁶ Fricker, Miranda (2007). *Epistemic Injustice: Power and the Ethics of Knowing*. New York: Oxford University Press.

Isaac Reed (University of Colorado, Boulder): Ontology and Anti-Ontology in Social Explanations
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When we examine how theoretical terms are used in actual research texts that make truth-claims about the social world, a distinction emerges that is not well-captured by the oft-repeated dichotomies deductive-inductive, scientific-humanistic, or even explanatory-interpretive. Rather, the key distinction is whether or not theory is used ontologically.

“Using theory ontologically” or “theory as social ontology” can be defined as the expectation that a group of theoretical terms sets out the nature, dynamics, or essential entities and structures of the social as *such*. In so doing, ontological theory tends to create, in the abstract, a picture of the social world that is expected to apply widely (generality), be consistent with itself (coherence), and describe directly social reality (reference).

Many social researchers whose substantive theories of the social differ considerably can be said to share the epistemic mode wherein explanations are built from theory-as-ontology. Some well-known examples would be Randall Collins, Theda Skocpol, and Claude Levi-Strauss. Ontological explanation is also the basis for the common understanding of the comparative-historical sociological tradition as a cumulative exercise in theory development and explanatory knowledge, and the basis for the increasing acceptance, among many theoretically informed sociologists, of some form of scientific or critical *realism* as the philosophy of social science that describes and justifies what they are doing. Ontological explanation is realism in action. In this paper, I briefly examine Barrington Moore’s *Social Origins of Dictatorship and Democracy* as a classic example of this.

However, there is another way of using theory in research that is explicitly anti-ontological, but which nonetheless makes truth claims, and perhaps even explanatory truth claims. In this epistemic mode, abstract theoretical terms feature prominently in the construction of social knowledge, but these terms do not, when taken together, offer a coherent, easily generalizable, or directly referential picture of the social as such. Taken in the abstract, in fact, the theories used in a given knowledge claim may be highly *inconsistent*. And it may not be clear at all that the explanation of the social actions at hand is generalizable to other cases. And what of underlying reality? In this anti-ontological mode, the intensive study of a case reveals an immanent unity underlying the social actions at hand, whose participation in this unity was not initially understood. This understanding is produced by a procedure that looks very odd by ontological standards: different—and often inconsistent—theoretical terms are attached to different pieces of evidence, creating compound “theory-fact” signs. Then, these compound signs are brought together to form the underlying unity which explains the case at hand. Two well-known practitioners of this way of using theory were Clifford Geertz and Michel Foucault. The cries of their critics reveal the key to their conceptual method: both were criticized extensively for proposing that an underlying, immanent unity (for Geertz, “the Balinese” or “Balinese culture”, for Foucault, the “dispositif” or “classical episteme”) explained their case.

Much work remains to be done on what makes this anti-ontological mode a justifiable way to pursue social knowledge. But I hope to make clear in the course of the paper that anti-ontological social research need not be dismissed as “merely interpretive” or as “anti-theoretical.” It may instead be the case that anti-ontology is a radically different means used in pursuit of a shared end—the explanation of social action.

Bill Rehg (St. Louis University): Inquiry and Solidarity: Implications of Tuomela's Philosophy of Sociality for Scientific Collaboration
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The need for collaborative and interdisciplinary modes of inquiry is apparent not only in areas of "big science," but also in science-intensive policy arenas that draw on multiple forms of expertise. But cooperation across disciplines presents distinct challenges, which arise in large measure from differences in disciplinary practices: in coming to understand oneself as a competent member of a discipline, one masters a distinct disciplinary ethos, a set of constitutive goals, beliefs, standards, values, etc., that guide the practice of inquiry. To illuminate the role of disciplinary ethos in collaboration, I draw (primarily) on Raimo Tuomela's *Philosophy of Sociality* (2007), which steers a middle path between individualistic and holistic theories of social action. The key question is whether disciplines count as "I-mode" or "we-mode" groups. That is, do members of a discipline cooperate simply as a collection of individuals, each of whom is privately committed to the group ethos? Or do they rather act precisely *as a group*, whose practice is based on the members' joint acceptance of "group reasons" that satisfy Tuomela's "Collectivity Condition," such that R is a group reason for me iff R is a group reason for every other member of the discipline? After introducing the problem of collaboration and relating it to Tuomela's framework, I argue that epistemic authority in the sciences requires we-mode group commitments, at least to some extent—although I-mode defections can be fruitful for innovation. Drawing on case studies, I then assess the importance of we-mode attitudes for two kinds of collaborative inquiry: research teams within a discipline, and interdisciplinary collaborations. In the former case, I-mode attitudes can destroy collaboration. In the latter case, the importance of the we-mode depends on the kind of interdisciplinarity. However, robust interdisciplinary collaboration requires participants to forge a new we-mode group identity based on a heterogeneous intellectual solidarity.

Objectivity without Value Neutrality: The Precautionary Principle and Anthropology

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There is a longstanding debate in the philosophy of social science about the feasibility and desirability of value neutrality, that is, of insulating social science research from political and moral values. One familiar position insists that value neutrality is necessary for scientific objectivity and appeals to a distinction between the contexts of discovery and justification to explain how value neutrality can be maintained. However, the context-of-discovery-versus-justification distinction has come under a considerable degree of criticism in the philosophy of science literature within the past twenty to thirty years, thereby necessitating new approaches to this topic. In this essay, I develop a proposal about how non-epistemic values might enter into assessments of evidence without compromising objectivity. In particular, I consider the role of the precautionary principle in assessments of environmental risks that tend to be borne most heavily by poor and politically marginalized social groups, which I illustrate with a case study drawn from the anthropologist Melissa Checker's (2005) book, *Polluted Promises: Environmental Racism and the Search for Justice in a Southern Town*. I propose that the precautionary principle can be given an epistemic interpretation by means of what is known in the confirmation theory literature as cognitive utilities. I explain how considerations concerning environmental justice can impact those cognitive utilities and how my proposal applies to the case described by Checker. Finally, I defend my proposal from the objection that epistemic interpretations of the precautionary principle allow for an unacceptable role of bias in the assessment of scientific evidence and hence are incompatible with scientific objectivity.

Emma Tieffenbach (University of Geneva): What Money Is : Neither Menger nor Searle, But Somewhere in Between

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In *The Origin of Money* (1892), Carl Menger describes the emergence of the money system as the unintended consequence of actions directed toward another end. No agreement is required for money to replace the barter system, he claims. Such view is strikingly at odd with that of John Searle (1995, 2008) who, more recently, argue that something can not be money unless it is collectively recognized as such. As any institutional facts, money is observer-dependent in the following sense: something could not be money unless we collectively intended to assign it the function of being a medium of exchange.

First, I review two ways of reconciling these two apparently conflicting views. The first suggests that while Searle explains how money emerges among a small unit of agents, Menger explains how the money system spontaneously spreads itself among the entire group. Call it the space reconciliation. According to the second, Menger ends his description of the emergence of money just before Searle begins his. Call it the timing reconciliation. I argue that none are entirely convincing

Second, I show how confronting these two opposite views illuminates one another's respective weakness. On one side, what Searle shows is that, *pace* Menger, something cannot be money unless its users *intends*, and correlatively consciously accept, to use it as money. This is because, unlike screw-driver for instance, something cannot play the function of money in virtue of its intrinsic physical features. Therefore, the function must be imposed on by external agents and money is not an *unintended effect*, strictly speaking.

On the other side, what Menger correctly shows is that, *pace* Searle, *no collective agreement* for counting something as money is necessary for that thing to be money. The only mental state that needs to be present in each individual is a personal intention to use something as a medium of exchange. Although such intention needs to be widely shared, it needs not be the object of a mutual knowledge, as Menger correctly indicates. Therefore, being a medium of exchange is a function that needs not be *collectively* imposed on an object by agents for that object to count as money.

Malcolm Williams (University of Plymouth): Realism, Causal Analysis and Dispositions
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This paper asks the question, can causal powers or dispositions provide any methodological added value in social science? The answer to this question depends firstly on whether we can make the reasonable inference that there are such things and secondly, if there are would this make any difference to our methodological reasoning?

In recent years realists (Pawson 1989, 2006; Sayer 2000) have claimed that (at least) post hoc evidence for causal powers or dispositions can be demonstrated and this gives us license to propose theoretical models, which if true, would directly reveal the real structures of the social world. With some exceptions realism does not propose new methods, but rather different methodological assumptions prior to research and different assumptions about conclusions. Thus, realist research commonly uses the same battery of quantitative data collection and analysis methods that have been traditionally used in empiricist social science.

In recent decades the latter tradition has moved a long way from the denial of the possibility of causal inference, exhibited in the work of Dodd or Lazarsfeld to embrace 'causal analysis' (Winship and Sobel 2004). The causal analysis programme makes no ontological assumptions about causes, but statistically infers causal relationships between variables in multivariate models. The variables themselves are often 'proxies'. We can identify two kinds of proxy, where one variable stands in for another when the second has not been measured e.g comparative wealth can be inferred from proxies such as occupation, qualification, area of residence etc. The second type of proxy, where the term is not commonly used, is where the operationalisation of a concept is that which we are able to measure, rather than the 'concept' itself. We can informally talk about social class, but to measure it must be operationalised into indicators. This second kind of 'proxy' is ubiquitous in social science and it encompasses virtually everything that is not an indicator of a physical or environmental characteristic (Williams 2003). No one claims that when they measure social class, cultural capital, occupational mobility that they are measuring something essential, but rather that they are measuring phenomena that might indicate the causal patterns of social life.

Whilst realists regard dispositions as necessary properties of an object they claim they 'denote what an object is like and what it can do and only derivatively what it *will* do in any particular situation' (Sayer 1992: 105). No one claims that dispositions can simply be read off from data, but require us to reason their relationship and causal potential from the evidence of indirect perception. Whilst the metaphysical starting points may differ, methodologically causal analysis and dispositional analysis amount to much the same thing. Both infer causes and both must use proxies of both kinds to do so.

In this paper I will nevertheless argue that the case for dispositional properties can be made if ontologically we regard them as contingent, rather than necessary properties. Following this I will argue they have methodological value in two respects¹) if regarded as contingent they may provide the starting point for a realist version of probabilistic causal analysis that begins from the properties of the case, rather than inferring proxy values to a variable. However the potential of this approach, though novel and valuable is limited to analysis at micro and meso level. 2) They may be treated as heuristics, which might be the basis of testable theoretical models, but this too requires a further modification of realism i.e. that our models can only be approximations of social reality.

The conclusion of this paper is that whilst the methodological practices of realism and empiricist causal analysis are not so different, a modified version of dispositions has potential for added value in analysis and perhaps more generally provides a 'moderate realist' bridge between the two traditions.

Kei Yoshida (University of Tokyo): Altruism, Trust, Strong Reciprocity: Lessons from Neuroeconomics to Philosophy of the Social Sciences
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The aim of this presentation is to investigate what philosophy of the social sciences can learn from neuroeconomic studies of cooperative behavior. First, I shall explain why neuroeconomics emerged and how it differs from standard economics. In standard economics, the idea of homo economicus is the fundamental assumption. Its basic ideas are that an agent is rational, self-interested, and emotionless, and that the agent works for maximizing his/her utility. Although there are many criticisms of homo economicus, it has been accepted and employed in economics for many years. Yet recent studies in behavioral economics and neuroeconomics suggest that we need to reconsider the idea of homo economicus. Second, I shall examine major neuroeconomic studies of altruistic behavior. Scholars such as James K. Rilling, Alan G. Sanfey, and Dominique J.-F. de Quervain respectively employed game-theoretic situations and examined which part of the brain was activated in the cases of cooperating with others, receiving unfair offers, or punishing defectors. These studies suggest the following things: (1) human beings derive pleasure from cooperative interactions, (2) there may be a conflict between cognition and emotions when receiving unfair offers, and (3) the punisher derives satisfaction from punishing the defector. These neuroeconomic studies implies that there is some correlation between the brain and our social behavior. Third, I shall discuss neuroeconomic studies of the relation between trust and the neuropeptide oxytocin. Neuroeconomists such as Michael Kosfeld and Thomas Baumgartner investigate how oxytocin influences our trusting behavior in game-theoretic settings. According to their studies, oxytocin reduces fear by reducing the activity of the amygdala and midbrain regions. Interestingly, oxytocin affects individuals' willingness to take social risks in interpersonal interactions. Fourth, to understand what we can learn from these neuroeconomic studies, I shall scrutinize Ernst Fehr and others' idea of strong reciprocity and argue for the need of social institutions that can promote cooperative behavior. Based on their neuroeconomic studies, Fehr and his collaborators argue that human beings are strong reciprocators. But as they admit, even if we are strong reciprocators, it does not follow that our behavior is always altruistic. Whether we behave altruistically turns on our social institutions. Thus our organization of social institutions is crucial to promote cooperative behavior and thus to make our society better.