



TRAJECTORIES

Summer 2013

Newsletter of the ASA Comparative and Historical Sociology Section

Vol. 24, No. 2

SECTION OFFICERS 2012-2013

Chair

Julian Go

Boston University

Chair-Elect

Andreas Wimmer

Princeton University

Past Chair

Neil Fligstein

University of California-Berkeley

Secretary-Treasurer

Ho-fung Hung

Johns Hopkins University

Council Members

Nina Bandelj, *UC Irvine* (2013)

Greta Krippner, *U Michigan* (2013)

Mounira Maya Charrad, *U Texas-Austin* (2014)

Dylan John Riley, *UC Berkeley* (2014)

Emily Erikson, *Yale University* (2015)

Isaac Reed, *U Colorado* (2015)

Elizabeth Pearson (Student, 2014)

Newsletter Editors

Ateş Altınordu, *Sabancı University*

Seio Nakajima, *U Hawaii-Manoa*

Webmaster

Kurtulus Gemici, *National U of Singapore*

CONTENTS

Book Symposium (Reed).....	1
Member Awards.....	17
Member Publications.....	17
Section Awards.....	20
ASA 2013 List of CHS Events.....	21
Symposium Announcement.....	23
2013 Section Election Results.....	24
Call for Member Information.....	24

Book Symposium (Reed)

Editors' Note: Isaac Ariail Reed's Interpretation and Social Knowledge: On the Use of Theory in the Human Sciences (Chicago 2011) was the subject of an Author Meets Critics session at the Social Science History Association meeting in Vancouver in November 2012. These are the revised comments of Richard Biernacki, Lyn Spillman, and Jim Livesey, with Isaac Reed's response. We would like to thank Mounira Maya Charrad for organizing the session and guest-editing the book symposium.

Interpretation and Social Knowledge: On the Use of Theory in the Human Sciences
by Isaac Ariail Reed

Chicago: University of Chicago Press,
2011

Reading Scholarship with Reed

Richard Biernacki, University of
California, San Diego

Since encountering Isaac Reed's pithy book in 2011, I have yet to find any release equivalent to it in breadth or importance. Scarcely longer than some extended essays, *Interpretation and Social Knowledge* pins down a dazzling variety of intellectual contenders swiftly. The book's deftness makes it a challenge to discuss, because there is scarcely wiggle room between summarizing versus over-simplifying. My aim is to map the argument's major turns while conceding, as an *American Journal of Sociology's* reviewer did, that the book's "complexity and ambition" defy rapid summary. For the book indeed scrambles for a new take-off virtually all the familiar philosophical debates in the social sciences: the standard positions taken by positivists versus critical realists, those adopted by scientific explainers versus interpreters, as well as other divides.

Reed pulls this feat off by excerpting and dissecting what the most accomplished social inquirers from competing genres actually *DO* in their texts as rhetorical and evidentiary masterpieces. He initiates you into his unique way of sympathetic yet critical reading by taking you word-by-word alongside to experience his intellectual retranslating of classic passages, whether from Skocpol's *States and Social Revolutions* or Jürgen Habermas' *Structural Transformation of the Public Sphere*.

The first chapter, with the dramatic one-word heading "Knowledge," starts from the unifying insight that social inquirers build distinctive kinds of research depending on how they bring two meaning systems together. The first meaning system is always that by which "facts of the matter" are represented via a signature language and narrative positioning. The signified facts deserve to be called a meaning system because of how the inquirer cannot avoid typifying evidence into characteristic kinds of elements. Sometimes this amounts to what Reed calls "minimal interpretation." That is to say, the inquirer is not

just chronicling one thing after another, but signals what the facts basically mean in a minimally coherent scenario and storyline. My example would be: the calling of the Estates-General leads to a crisis in royal authority, enabling the Third Estate to claim to represent the French nation, and so forth. Even this kind of story is a delicate signifying operation because it calls on all the accessories of the concept of royal authority or legitimate power, for example, and therefore draws on the resonances of a larger web of political terms. This too justifies use of the term "meaning system" to indicate how it is only as part of a system that any facts gain the privilege of popping up as the alleged referents of the signs of the text.

The second kind of meaning system is always that of a theory, which Reed defines somewhat pragmatically as a more transportable set of concepts that are more systematically interworked. Its purpose is to frame what generates the facts, what decides their consequences more generally or their implications for the future. Colloquially, Reed suggests, theory digs out what is essential, "underneath the facts" (17). This kind of meaning system derives more of its import from internal logic or contrasts of terms, and if it has a referent it would be a model, wherever you wish to locate that model—in our heads or out there as a realized social form.

How these two meaning systems intersect establishes Reed's threshold criteria for social scientific excellence. Humdrum social science just uses facts as object-tokens for the theory. As Reed says, "facts provide an 'example of' a theory, theory provides a new way to view the facts" (22). But fact and theory remain separate in that the theory is just a commentary on the facts and the facts subsist by themselves in "well-colligated meaning systems" (22). It is implicitly subversive for Reed to propose that inquiry capable of producing compelling "new social knowledge" characteristically makes these two meaning systems, in Reed's phrasing, "fuse." Taking Marx's descriptions of the bourgeois use of heroic Roman symbols as an example, Reed suggests that the facts intrinsically take on deeper theoretical weight and the theory itself is enriched by new factual allusions such that it is more difficult to disentangle the two. That is what transpires in

Marx's *Eighteenth Brumaire of Louis Bonaparte*. Reed calls this effect "maximal interpretation," and he is the ultimate subversive because he convinces you that *when we cannot much tell the difference between theory and fact*, at least on first inspection, we are encountering social science worth talking about. For me, this counterintuitive insight might account for why *great* social inquiry also tends to invite the most disagreement about what the researcher is most basically up to, which I think is what social investigators actually experience but which is also scandalous for believers in normal hard science. It is a sterling event to invent such a suggestive criterion of explanatory excellence, but I would prefer to underscore its status as a promising *hypothesis*. More specifically, I cannot see how an analyst can logically establish this fusing of systems as a *necessary* criterion for important social inquiry. It is easy to sympathize with the idea that fusing is a true criterion *de facto* or that it is valid *de jure* in the best of academies. Indeed, it feels instinctively correct to envision masterful, canonical research as the stuff with which you want to engage whether you agree with it or not, or more particularly, the stuff about which you cannot make up your mind because it is not patently comprehensible. Conversely many would define pedestrian research as the stuff one does not enjoy reading even when one senses all too completely its message and verisimilitude.

Reed's second independent criterion for adequacy of research is also a kind of fusing. Reed insists that we hold onto the wealth of particular meanings which constitute historical agents' equipment and motives but that we integrate those meanings with the practice of explanation, severing neither explanation off nor hermeneutics off as enterprises of their own. In Chapter One Reed goes for broke when he proposes that "explanation can only function as a subcategory of the larger category of understanding," which for him means understanding human subjectivity as thickly constructed. It is not interpretation *versus* explanation, but how to relate the two that ought to preoccupy investigators. Reed astutely does not want interpretivists to quarantine the scientific explainers as engaging in a different sort of enterprise. For dividing the universe in this manner only lets the naïve realists carry on with their

business undisturbed—even preeminent—as before.

With his envisioning of two meaning systems, Reed proceeds to dissect three kinds of "maximal interpretation," which he calls the three major epistemic modes. He lends canonical significance to two of them, but recommends only the third. Readers will appreciate this strategy because it does not start by asking dismissively why an epistemic mode fails. Through something closer to an immanent critique Reed asks more sympathetically *why* well-known masterpieces operating in the first two epistemic modes *succeed*—only in a slightly illicit way.

The first epistemic mode is the realist one that characterizes key works of Theda Skocpol, Barrington Moore, and Marx, at least as explicated via Roy Bhaskar's critical realist presentation of Marx. Through careful verbal reanalysis, Reed brings to life the following circle: these investigators' narratives presume what they seek to prove through historical demonstration. "The claim is, implicitly or explicitly, that the theoretical signifiers used by a researcher point to an essential aspect of the social as such, and that this world exists underneath the time-space patch [in other words, the historical case] of social life to which their evidence refers" (49). To show how we readers so easily assume that the general theory refers to preexisting general entities, Reed reminds us how Barrington Moore can talk of "the relation between town and country" as something that actually exists as a thing given in advance as comparable across England, China, India, or anywhere else (49). When such theoretical terms as "class relations" intersect with the reportage through such verbs as "drive forward" or "bring about," it looks like we are referencing real causal joints with a general, coherent theory. Why are we not? Reed initially, in Chapter Two, shows nothing more, perhaps, than that there is a "semiotic circuit" that manufactures a misleading "reality effect." *Because* the social theory is assumed to reference social reality directly, it looks like an outcome in a particular case, once it is resignified by the theory, is being causally explained. The commensurability of diverse historical cases is guaranteed only because, circularly, "they have the same object as defined by the general theory" (54).

Reed suggests that since social reality is “transitive,” always churning under the influence of ongoing human interpretation, it is equally plausible that “cases could be *incommensurable* in the sense that that they do not have, underneath them, different arrangements of the same basic social forces, or mechanisms, or relational entities” (62). Notice that this critique extends to sociologists such as Tilly or to critical realists who are ready to admit that varying conjunctures of potentially universal mechanisms must be called upon to explain historically unique processes. These researchers still seem to assume that there is traceable in reality an existent toolkit of *general* mechanisms.

The second major epistemic mode for fusing the meaning systems of evidence and of theory Reed calls normative. His dissection of it will surprise many. The realist and normative modes do *not* line up with the belabored divide between facts and values. Instead they are twins. The normative mode works similarly to the realist mode in that it, too, posits a master referent underneath cultural particulars, no longer social reality but a utopia or an ideal that bridges peculiar narratives of the past to ideals made alive in the present. Habermas in the *Transformation of the Public Sphere* is interested primarily in how, where, and when universalistic debate between sheer humans was in part actual. “Actual,” as Reed qualifies it, means either in practice or in a circulating ideology. Although the tension between an inclusive public sphere and its very partial realization in early bourgeois society is never overcome even in Habermas’ own account, the narrative of the historically particular appears as the unfolding of a theory. The genealogy fuses evidence and theory because it looks to nearly all as if it accesses something essential and real, an ideal that unites the inquirer with the agents being depicted. There seem to be more positives in this epistemic mode of social research than in the realist one. My sense is that Reed’s complaint with the normative mode has two facets: the normative epistemic begs the question of how one chooses the theory or ideal that accesses something fundamental; then, too, it exercises preemptive tunnel vision when it isolates meanings from the larger cultural landscape.

Finally now, the solution, the third epistemic mode, interpretive understanding: um, why not just advise us to read Weber? Why does Reed compose it afresh? How does this mode carry out the task of explaining? In Reed’s telling, the key to the interpretive approach is that theory is whatever collection of models you happen to “pluck” that imparts novel sense of the meanings implicit in cultural artifacts. Consider Geertz’s study of the Balinese cockfight. It draws eclectically on Freudian word play on the Balinese term for being “cock crazy;” on Bentham’s utilitarian theory of how despite favorable odds of winning, the stakes of a game can be so high it is irrational to play; and on the Durkheimian distinction between a profane nature and the sacred human. These theories and many affiliates can remain disunited in Geertz’s treatment, because each singly draws out the features of masculinity, social status, or animality yet the coherence they reveal, such as it is, is lodged in the cultural landscape, not in the theory or in the head of investigators (102). The partial coherence that is constructed comprises the ultimate deep referent that lies beneath the superficial particulars. In my view, the theories can remain metaphors or tools for “seeing as.” Each therefore helps us put together an underlying sense to the evidence, but none of them indexes by itself an essential or necessary feature of human societies shifting through history. Geertz’s multiple theories are necessary for “maximal interpretation,” because they fuse our appreciation of what the cockfight descriptively is with our deepened understanding of the stakes of the game and the consequences it triggers. A researcher can “pluck” theories and remain “empirically responsible” (116) if each reading is minimally plausible and if they in toto add up to an “overarching interpretation.” Overarching does not mean the cultural landscape is clear and monolithic. A culture may be somewhat messy by functioning by the very tensions it sets in motion. In all likelihood Reed sees this interpretive plurality as a logical condition for avoiding the assumption that there is a necessarily an easily theorizable coherence to any culture, which would unfortunately mimic the assumption of theory in the realist mode. Reed is interested neither in interpreting unit acts; nor necessarily in contrasting conduct to a norm of

means-end rationality; nor finally in interpreting each case as an artifact for a research program, such as one about rationalization processes—whereas exactly those three features arguably characterize Weber.

Reed does resemble Weber by insisting that interpretation is the necessary foundation of explanation. Positivist or realist approaches survive only by making up their own in-house rules. The major forcing causes of social life, motivation specifically and mechanism more generally, are constituted by the landscape of meaning in a case. To demonstrate such forcings for motivation, Reed explicates George Steinmetz's *The Devil's Own Handwriting*. Some German colonial administrators may have been moved by general dynamics of psychic identification, by Steinmetz's evidence by identifying with the natives in particular. But the concrete function and direction this general process assumed depended on the peculiar anthropological discourses in place that set up images of the native in the first place (149). Furthermore, mechanisms, as well as motivations, are subject to this forming. Mechanisms include "powerful, repeated social processes that help us explain so many social outcomes" (159). For example, the institution of the confession by Foucault's rendering regularly produces a certain kind of self-objectifying human subject (154). But without understanding particular theological variants of sin and salvation, and their historical morphing into discursive norms for sexuality, we cannot explicate what form of culturally recognizable human subject will be produced (154).

Explanation is the explication of the historically specific mechanisms that entail certain consequences, and these mechanisms cannot be located outside the culturally specific landscapes that interpretation discloses. Reed's argument that interpretation is the necessary foundation flows through an assumption similar to that of the realists, namely, that explanation takes the form of the specification of mechanisms. But it also breaks with the realist explanation of human action for reasons similar to Weber's. Weber assumes that the scientific realist who aims to explain behavior without reference to specific subjective meanings relies on non-explanatory generalities. Knowing

that, say, people eat when they are hungry is non-explanatory. Only after you interpretively understand more specifically what they identify as food or whether they think they are fasting and their other cultural formations can you locate the motives and course of conduct. Thus in his concluding chapters, Reed's articulation of new objections to realism finally merges with that of Weber. But Reed's presentation is strengthened by how it demonstrates more concretely that postulating mechanisms without depth interpretations of particular cultures is non-explanatory.

Historical researchers and cultural sociologists who wish to endorse interpretation as a rigorous enterprise might well pull *Interpretation and Social Knowledge* from the shelf to justify to someone what they are already up to in their own research. But as a self-doubter, I have to admit that I do not always appreciate the rationales and full implications of my own procedures! The following four issues convey some of my self-doubting and therefore my questioning of *Interpretation and Social Knowledge* as well.

Question One: On what grounds can one mandate pluralism and plucking from multiple schemas to arrive at the best understanding of meanings in a cultural landscape? Why not acknowledge that our comprehension of meaning is always incomplete and selective, therefore constructed in the final instance by a line of questioning alone? If one follows Marx to suggest how we might interpret the construction of labor as a string of practices on the shopfloor, the depth and adequacy of the reading may derive from the parsimony of theory. At least that was the hope behind my book *The Fabrication of Labor* (University of California Press, 1995). Analogously, if researchers use the dynamics of Freud's scenario in *Totem and Taboo* to reconstruct in eerie detail the reechoing dynamics of emotion in the French Revolution, they may build a masterpiece such as Lynn Hunt's *The Family Romance and the French Revolution* (University of California Press, 1993). I self-consciously single out books that deploy perhaps the two greatest of all unified theories, those of Marx and Freud, yet each research exemplar tries to let the grand theory do the unifying only as a

nominal “seeing as.” Everyone who accumulates primary documents knows that a landscape of meaning is too vast to be painted as a landscape, or anyway too vast to let completeness and extension serve as criteria of interpretive adequacy. Might parsimony serve as a criterion of authentic local knowledge in addition to the pluralism exemplified in Geertz’s legendary cockfight?

Question Two: Is coming to understand a landscape really a process different from (therefore potentially “prior to”) the citing of causal mechanisms? Or, if these research processes logically interpenetrate, why not provocatively assert other way round that explanation based on mechanisms is prior to interpretation? For example, if we want to understand what the symbolic design of a cathedral or residence means, we have to know the mechanisms by which the edifice was made and the somewhat objective constraints on the building’s creation, from the weights of stones to the pressures of the market. Or when we decide into which genre we will classify a particular verbal artifact for interpretation, are we not likely assume to something about the social mechanisms of the artifact’s making, as Roger Chartier demonstrated by resignifying of Darnton’s canonical interpretation of “The Great Cat Massacre”? (Roger Chartier, “Texts, Symbols, Frenchness,” *The Journal of Modern History*, 57, 1985, 682-695).

Question Three: a potential contradiction appears in postulating simultaneously, as Reed does, that a deep web of significations as necessary for social explanation *and* that explanation considers counterfactual what if’s (156): The effects of meanings are, “like other causes, drawn out through comparison and/or counterfactual reasoning [like] (‘it could have gone differently’).” If meanings are thick and constitutive, it is difficult to formulate counterfactuals that retain the basic identities of historical agents, as Geoffrey Hawthorn demonstrates in *Plausible Worlds* (Cambridge University Press, 1993). For example, in *The Disciplinary Revolution* (University of Chicago Press, 2003), Philip Gorski makes a case for the causal effects of Calvinism by asking if Venice would not have built a more competitively efficient state had it enjoyed the disciplining effect of Calvinism (76). But this is to unwind a very

considerable part of the culture, military geography, and intra-Italian patronage that constituted an entity like Venice in the first place. The penetration of culture can make it look implausible to substitute even a single politician acting at a critical juncture. In his book *Injustice: The Social Bases of Obedience and Revolt* (M.E. Sharpe, 1978), Barrington Moore suggests that we cannot counterfactually put at the helm of state a more activist socialist politician than Friedrich Ebert in the Germany of 1919, making it less realistic to imagine an alternative to Germany’s incomplete revolution (392), and making it less feasible to see an escape from the tragedy of 1933. Moore judges that the over-determined bureaucratic culture of Germany’s socialist party made it nearly unthinkable that a leading socialist personality could have emerged who was consequentially different from Friedrich Ebert in 1919.

Question Four: Finally, all the magic that the word mechanism effects for us nowadays is worrying. In my own writing it troubles me that my use of mechanism implicitly asserts forcing or necessity in principle, but I identify it via events both conjunctural and singular, rendering it a metaphor without much epistemic backup. Does Reed invoke it as comprising “explanation” as a work-around for all the debates about the proper criteria for explanation? Have we stretched mechanism too far and reified human intentionality if we say historically idiosyncratic motives, too, are analogous to mechanisms as explanatory “forces” (159)?

By creatively reframing the current state of play in social science, Reed has set our agenda for both practice and theory for quite some time to come.

Re-Interpretation of Social Knowledge Lyn Spillman, University of Notre Dame

This short, elegant book lays down new epistemological grounding every sociologist needs to know. Up until now, we haven’t had a widely shared reference articulating and analyzing the distinctive intertwining of interpretation and explanation evident in most exemplary

comparative-historical research (as well as many other streams of sociology). Certainly, contemporary methodological discussion among historical sociologists is vibrant and innovative. (Indeed, I suspect that even standard graduate methodology classes will look quite different in a few years because of our work). But until now, discussions first initiated in the “second wave” of comparative historical sociology mostly explored and developed sophisticated new ways of arguing that qualitative historical comparison could be just as explanatory as any positivist, large-N research design—if not more so. By contrast, our understanding of why interpretation is important has lagged. Now, Isaac Ariail Reed’s *Interpretation and Social Knowledge* provides a new methodological approach more appropriate to the sub-discipline’s “third wave,” indelibly marked, as it is, by the cultural turn.

Reed begins with the incontrovertible yet often neglected observation that the facts of our cases and the theories we bring to them are certainly meaning systems (whatever else they may also be), and explanation and interpretation bridge those meaning systems. Against this background, he analyzes, illustrates, and compares three distinct ways we might link theory and evidence—realist, normative, and interpretive. The book’s five chapters—smartly titled “Knowledge,” “Reality,” “Utopia,” “Meaning” and “Explanation”—examine the following questions. What does the knowledge we value look like, and why? How do realist claims to be discovering underlying ontological truths operate? How do normatively infused knowledge claims ground themselves? What further explanatory purchase is gained if we make the meanings of social life the central ground of our inquiry? And how could interpretation make causal explanation? Reed’s analysis and assessment of realism and normativism are balanced and sympathetic, illustrated with fresh readings of many classics well known to historical sociologists. But he ultimately makes a spirited and novel argument for the explanatory power of interpretivism, the “disclosure of a landscape or landscapes of meaning” (110). He moves beyond old antinomies between explanation and interpretation to show how interpretive analysis is crucial for causal explanation. As he ultimately

argues, when we bring theories to the interpretation of “landscapes of meaning” within which action takes place, interpretive epistemology “allows us to get causal explanation right” (161).

The reasoning throughout is deeply grounded and tightly woven, but the book’s engaging authorial voice guarantees that reading it will offer many pleasures. I collected a long list of fresh formulations, ironic asides, theory laden images, and epigrams that work like lightning to illuminate our own “landscapes of meaning.” Here are just a few, which can also serve as summary: “explanation as a goal for the study of human beings can only function as a subcategory of the larger category of understanding” (35); “many of the most cogent reflections on realism participate in the strange gambit of science or nothing at all” (63); “some agents, some of the time, are not motivated by the horror of death... but rather are motivated to make money via double entry book-keeping . . .” (103-104); the belief that “knowledge claims are *just* interpretations . . . is a terrible idea and should be resisted at all costs” (124); “the social explanations that causal claims help produce have to be reconsidered not rejected from within the interpretive epistemic mode” (130); and “the nature of the social as such is that it is impossible to theorize the nature of the social as such” (162). And I venture to predict that some of the basic conceptual building blocks of the argument—like “minimal and maximal interpretation,” and “landscapes of meaning”—will very quickly gain wide currency in our methodological discussions.

The inevitable problem with pithy, tightly woven arguments is that readers always want more. We are left with questions and puzzles about how to situate the argument, and about its broader implications and uses. I want to ask here about the contrast between minimal and maximal interpretation, about the range of positions which might be included in this typology of non-positivist knowledge, about whether explanation really needs two types of causes, and finally, how this reconfigured epistemology might influence how we think about research design, and thus how to take these ideas further.

First, minimal and maximal interpretation anchor a “spectrum” (23) from uncontroversial referential facts to relational, theory-laden

statements which simultaneously organize, explain, judge, and interpret those facts. This is a really useful clarification of a difference we notice all the time, in all sorts of analytic and evaluative contexts. But since, as Keynes famously observed, supposedly a-theoretical facts are more likely the residue of dead theories, the spectrum is relative. Even the basic facts of our work, like uncontroversial dates, are concept- if not theory-dependent and social theory is almost never an abstract, non-referential deductive system. Useful as it is, this distinction itself is liable to become messy, especially as “interpretivist” modes of linking theory and evidence are explored further.

This is, I think simply a friendly amendment, but it comes with a deeper worry attached. Maximalist interpretation, compelling as it is to many sociologists, doesn’t communicate very far. In my experience, even the pleasures of reading someone like Geertz seem lost on readers who are not attuned to the theory resonating beneath the stories. What engages most people are new “facts”—coherent, of course, with the implicit, dead, stable theory that’s commonsensical. I think a lot of sociology’s impact in the world, such as it is, actually comes from minimalist interpretations, such as information on correlates of gender differences in pay. Personally, I think it’s important to have intellectual spaces to generate new, maximalist interpretation remote from contemporary concerns, but that could be a problem if you want sociology to communicate broadly. So, I’d ask, when should we favor maximalism as deeper explanation? This question is only preliminary to the main argument of the book, but the distinction is so useful that it deserves some more reflection.

Second, who’s a realist, and who’s a normativist? Reed’s explanations and exemplars are clear, but categorizing other research seems more difficult. I want to be able to use this typology to communicate to people who don’t know, for instance, critical realism, or Habermas, but do know, for instance, Weber on interpretation, or public sociology. Can I do that? Does it translate? What’s the broader range of positions you could include in this anatomy of non-positivist knowledge? What are the intended uses of the typology? And do some works mix realist and

normative, normative and interpretive, realist and interpretive positions? If that’s possible, does it matter?

Third, does explanation really need “forcing” as well as “forming” causes? I’ve also argued for expanding our view of causality to include formal causes, or conditions, so I welcome Reed’s account of how interpretivism identifies what he calls forming causes, or “the way in which social life is arranged . . . by formations of meaning—which are the condition for mechanisms and motives” (146). On this view, we can’t do explanation with causal mechanisms and motives without also systematically understanding landscapes of meaning. Nevertheless, Reed still seems to believe that the more familiar “forcing” causes are necessary for explanation. I’d like to take this argument a step further, to say that creating interpretive understanding of conditions or “forming causes” may sometimes be all we need to look for in an explanation—especially in historical comparative sociology. The “forcing causes”—the mechanisms and motives—of many interesting outcomes are rather obvious, trivial, and particular, whereas the conditions which make them likely to be efficacious are more obscure. In Weber’s account, there’s little mystery about the business techniques Protestants adopted which supposedly gave them an advantage over others; what’s interesting is the new “landscape of meaning” which encouraged more Protestants than others to adopt those techniques. I don’t really think we care too much about the game theory of Balinese cockfight win; the sociological explanation lies in the shared meanings attached to that win. There’s little mystery about how social movements and interest groups put marriage equality on the agenda and then on the books; what’s more intriguing is the shifting idea of marriage that make those strategies newly meaningful. So, I’d like to say that we can make forming causal arguments even without attending to mechanisms and motives generating particular outcomes. Is that going too far?

Finally, what does this new view of post-positivist knowledge mean for how we think about research design? Does it suggest new theoretical rationales for casing and case selection? What about different purposes for comparison? Does it

shrink possibilities of generalization? Does it alter logics of inference? These are all big questions, too big to address here, but I do want to raise them for our collective future consideration. If this book shifts the grounds of our methodological reflection as much as I think it should, there's a lot more work to be done to understand its practical implications. It's possible that it simply helps us understand better the good sociology we do already. On the other hand, there's a danger that some of the carefully argued ideas in the book will be co-opted as buzz words for dubious methodological rationalizations. Most likely, we'll understand and explain some of good work we're already doing better, but also start to develop new criteria of assessment for casing, comparison, generalization, and inference. I look forward to some serious new threads of methodological discussion based on this work in future.

How Will I Know?

Jim Livesey, University of Dundee

Isaac Reed's fascinating and enlightening book is bewitched. The book could hardly be more ambitious as it proposes an original understanding of the possibility of systematic social knowledge. In pursuit of a new understanding of how authoritative social knowledge is created Reed does not perform any of the classic manoeuvres of clearing the ground, revealing the shared flawed assumption underlying all prior efforts to configure the field or vaulting out of established research traditions. The language of the text is resolutely unheroic. Instead the book is couched in a therapeutic voice. It acknowledges the differences between versions of social knowledge inspired by the reliability of statistical inference, those based on the telling case or deeply imagined particular experience, but orientates itself toward an account of social knowledge that can comprise these manifold strategies.

The issues that Reed confronts are as pertinent to historians, particularly historians working in intellectual or cultural history, as they are to sociologists and political scientists. Historians in graduate schools are fully appraised of the

reductive dangers inherent in the strong programme in the sociology of knowledge and the insecurity of the categories of the philosophy of history, but are also warned against antiquarianism and biography. So, as Reed wryly notes, just like sociologists we know what we are against, but what are we for? How are the historian coding the contents of the *cahiers de doléances* of peasant communities in 1789, the reader of denunciations from rural France to the National Assembly the following year and the interpreter of intercessionary prayers at Marian shrines after 1795 to find a vocabulary through which they can interrogate one another's claims to knowledge of French peasants' experience of the French Revolution, to take just one example. Witches turn out to be useful to think along with when confronting this kind of problem.

The witches turn up as early as page ten. The Salem witch trials are used on this page as a good example of the utility of interpretation as an explanatory strategy. In understanding how witches threatened the integrity of the order of the world of this community, the interpreter can in turn explain why these individuals became the object of murderous intent and collective violence. Reed offers an approach that can explain why people were killed in the town without appealing to ideas such as "pathology" or "irrationality", or without making the actors puppets of some determining structures. Meaning and explanation are harnessed together, the beliefs of the actors and their motivations are acknowledged as having causal but not determinative force. As other commentators have remarked, this integrative approach characterises the whole book.

Reed refuses to accept theoretically articulated antinomies and polarities at face value and always seeks to reveal hidden compatibilities. His most audacious move of this sort is the claim that explanation is a particular instance of understanding. This bold move is softened somewhat by Reed's neo-Aristotelian account of understanding. To understand a phenomenon is to comprehend its causes, to answer a "why" question, and a lot of the book is given over to a discussion of material, formal and efficient causes, under the vocabulary of forming and forcing causes (the parallel is explained between pages 142 and

145). Final causes are not entertained, unremarkably as they have been rejected as an element of scientific explanation since the scientific revolution. However the function of final causes is fulfilled by the idea of “utopia” that is central to the Reed’s account of the normative epistemic mode. Even though one of the chapters bears this title this idea of utopia is less well fleshed out than many of the other important concepts in this book, with significant consequences.

Of course in dissolving the tension between explanation and interpretation as species of understanding the problems in social epistemology move from methodology and the constitution of social facts to question formation. Just what kinds of “why” questions will be accepted by the relevant community as appropriate to the generation of social knowledge? Reed takes on this issue of research design and question formation in chapter five. Reed argues that a well-formed research question in the social sciences will characterise the ground of possibility of a phenomenon and account for its emergence by reconstructing the mechanisms that articulate the reasons that motivate actors to generate the phenomenon and the causes that force the phenomenon into being. A lot of explanatory weight is carried by the mechanisms that articulate grounds of possibility, reasons and causes. Mechanisms in this account seem to do the work of institutions in other accounts of the objects of social knowledge, or assemblages in the kinds of actor network theory inspired by Latour. Social scientists and historians recognise a lot of different kinds of exercises, from description to theoretical reflection, as valid forms of social science and Reed’s account of social epistemology accounts for this variety. Reed argues that particular instances of social science range from the minimal to maximal accounts; in maximal accounts the theoretical terms of explanation are at stake while minimal accounts sit within theoretically secure, or at least accepted, research paradigms. One of the virtues of this theoretical account of the relationships between different kinds of social science is that the accounts of the variety of practices ring true. However one may have doubts about the viability of a totally integrative account. Programmes such as the

interrogation of literature through particular readings of neuroscience and theories of natural selection explicitly turn their face away from the distinction between reasons and causes in favour of an explicit positivism. It is difficult to see how such programmes could be anything other than the object of critique from this point of view.

Ultimately Reed’s epistemology is pragmatic. He points out a set of mistakes that impede the understanding of social research as a unified practice, such as taking reasons as simply another species of cause, and so setting up a false dichotomy between explanation and meaning, or mistaking naturalism or positivism for realism. He is careful to avoid any ontological or methodological statements that smuggle in some set of a priori ideas about what constitutes the “social”. His idea of maximal interpretations as assemblages that perform and exemplify a vision of social science is credibly realist and non-foundationalist. There may be a hint of a Darwinist sense of selection between maximal interpretations, but there is no explicit commitment to any philosophy of history. He clearly favours the interpretative mode of conducting social research as the best way to construct “a compelling fusion of theory and evidence [that] creates a maximal interpretation that, by power of intellect, reconstructs the critical reader’s understanding of the social actions under scrutiny” (123), but fully accepts the credibility of the normative and realist modes. So what we are offered is a compelling account of how we can get past meta-theoretical debates about how to create social knowledge, and sceptical moves that deny the possibility of any knowledge, to actual debates about claims about society. The comprehensivity of the model of the well-formed research agenda creates confidence that it can orientate genuine debate.

At which point the witches reappear to raise some worries about the comprehensivity of this account even on its own terms. They pop up on page 60 in an approving account of Bhasker’s use of synthetic a priori argumentation to establish the reality of social noumena. Reed argues that the pragmatic grounds in social phenomena are not sufficient to ground transcendental arguments for social science, “for, if it were, then one could say, for example; in Salem, Massachusetts, in 1692,

there was a social conceptualization of “the witch” which led to a set of practical actions that were highly successful: witches were found and killed. Thus it must be the case that the category “witch”, and more broadly the theory of what witches do elaborated in witch-hunting manuals of the time, must refer to a general social reality; there were (and are?) witches.”(60) This, on the face of it, is an odd foundation from which to develop an argument that social and natural ontology are not quite the same things since there obviously are witches. The town in the south of England where I live has a thriving coven and one of its, self-declared, members has been elected to the local council. There is a thriving debate in the historical literature as to whether the contemporary Wiccan movement is in continuity with earlier forms of animistic, popular religion. Witches make up a historical reality and a social phenomenon. Why deny they exist?

Of course what is at stake here is not the banal fact that there are people who are accurately described under the term “witch”, the same might be said for “Jedi Knight”. What is at stake is what is signified by that identity. And for Reed the kind of violence associated with the witch trials, particularly violence toward women, cannot be normatively recognized, cannot be rationally integrated as a possible element in a maximal interpretation of social reality. What seems like an ontological moment in a discussion in the interpretative mode is in fact an ethical point in an explication of social rationality in the normative mode. The issue is not that belief in the premises of witchcraft is incompatible with the institutions of modern science, that would be an issue for a very crude variety of critical realist and Reed is clearly not that. The problem is that witchcraft has been theorized within a particularly rancid set of patriarchal institutions that cannot be normatively grounded.

Is this a big problem? The specific case of the witch trials of the seventeenth century really offers no challenge to Reed’s ideas at all, and in many ways probably reinforces some of his observations about the relationship between social theory and embedded social norms. Witchcraft trials ceased not because people ceased to believe in witches, but because participants in the trials increasingly

observed that the conditions under which secure convictions could be made could not be fulfilled. The Devil was always a co-accused but could never be called as a witness or brought to trial himself. The trials ended because they could not fulfill the normative demands of procedural justice, which clearly can be integrated in a maximal account of social knowledge. In more general terms the case calls for a more considered account of normative horizons, final causes, and a deeper consideration of dystopias, constraint, violence and the other products of the dialectic of enlightenment. Possibly a consideration of Stanley Cavell’s notion of responsibility for knowledge claims might flesh out the idealized version of utopia that is at play in this text. And it might save the text from the inevitable Wiccan critique.

Response to Critics of *Interpretation and Social Knowledge*¹

Isaac Ariail Reed, University of Colorado at Boulder

When I read the excellent comments of Professors Biernacki, Spillman and Livesey, I felt both pleasure and trepidation. Their acuity in reading the book is quite wonderful, but also of course troubling, because questions from those who understand a piece of academic work are always more piercing and more revealing of its weaknesses than questions from those who remain entirely outside of its logic. In what follows, I use Jim’s complaint about my treatment of witches to orient the reader to how the book tried to open a space for debate about the human sciences beyond realism, a space for the development of interpretivism that I think does ultimately speak to Jim’s concern with responsible knowledge. Then, I try to answer some of Rick and Lyn’s truly excellent (and difficult!) questions. Finally, I conclude by discussing what I view as the next step forward from the book, a step that is, in fact, called for in Lyn’s comments.

¹ The author thanks Dan Hirschman for comments on a previous draft of this reply.

Let's start with the witches. My point, in *Interpretation and Social Knowledge*, about "successful" witch hunts was part (and only a small part) of a critique of naturalism as a philosophy of social science. In its sophisticated form, naturalism adapts, for the human sciences, a longstanding argument about the natural sciences that connects "realism" to "pragmatism." That now-classic argument is that, for natural science, there is a connection between scientific theory, experimentation, and the human interest in predicting and controlling nature (see Habermas 1971). So, the epistemic grounding of science is guaranteed not only by the ambitions of theory to mirror reality, but also by the pragmatic connection between scientific explanations of the world and the way technology can meet our practical needs as human animals. This argument is given an important new articulation in Bhaskar's *A Realist Theory of Science*.

For Bhaskar and for others, the point is that flights of theoretical interpretation are limited, in natural science, by what does or does not work when the experiment ends, the steam engine is built, or the disease is (or is not) cured. Though there is important work in the sociology and philosophy of science that disputes whether this view is sustainable even for *natural* science, it does seem to function well as a kind of 'working epistemology' for practicing natural scientists. More important to the argument of *Interpretation and Social Knowledge*, however, is the way in which this argument has been transferred to the human sciences. For it is this "naturalist transfer" of realism and pragmatism that allows everyone, via Bhaskar, to have their Kuhn and eat it too.

In *The Possibility of Naturalism*, Bhaskar proposed that the existence of proto-theories of society *in* society provides the substitute for the fact that the human sciences (often) lack experiments. Without restating my entire critique, I would just point out that this argument of Bhaskar's is the latest iteration of a longstanding argument in social theory that provides limits to interpretation via some sort of *analogue* or *imitation* of the connection between knowledge of nature and powerful technology that underwrites the natural sciences. Here are three examples: To some degree in Marx, and especially in Lukacs, the

equivalent to the experiment working in natural science is the revolution occurring and succeeding in the social world; interpretation cannot become infinite, or really even multiple, in social theory because it is hooked into *what must be done* to bring about the revolution (and thus to the standpoint of the proletariat, see Jameson 2004). Similarly, in Freud, the correct interpretation of the dream is, in the end, that which cures the patient. Finally, note that this same pragmatic-realist grounding for interpretation is *also* embraced by the great and vicious opponent of Marxism and Freudianism Karl Popper. In *The Poverty of Historicism* Popper proposed to replace the "totalizing" historicism of Marx with his own pragmatics of "what works," namely, piecemeal social engineering.

What is so bizarre about the long life of this pragmatic-realist argument in social theory is that the schemas of interpretation that emerged out of this work derive more from the worldviews, grand theoretical schemas, and utopian hopes of the intellectuals that produced them than they do some quasi-experimental grasp on human nature achieved through the imitation of science. Moreover, the intellectual *success* of concepts derived from Marx, Freud etc. has hinged, not on the curing of patients or the leading of revolutions, but on the detailed, careful *departures and reinterpretations* that their theoretical language games have been subjected to by other scholars, as Rick notes in his comments (and which, I might add, Lyn, Rick and Jim all exemplify in their own work). And this leads me to my snarky comment about witches that so bothered Jim.

For, while it is indeed a realm of tremendous normative debate whether steam power has good or bad effects on society, it is nonetheless possible to set up relatively well-defined intellectual boundaries within which one can determine whether an experiment, re: steam power works or does not work, and thus judge the success of the experiment. "Success" in social engineering, social practicalities, or social change does *not* provide the same kind of epistemic grounding for human science, for a variety of different reasons, including the fact that the very "stuff" one is "experimenting on" is subject to tremendous ontological variability, *and* the fact that social "success" is

subject to massive political dispute and contention, to the point that the boundaries within which one judges something a “success” are not so easily determined. I am of course aware that work in the natural sciences is also subject to dispute and politics, and that advocates of Actor Network Theory will find in my argument a reiteration of the illusory modern ontology that Latour (1993) attacked. Nonetheless, I think that the distinction retains some validity.

Hunting witches is an example of the messiness of “successful” social engineering, though an all-too-easy and extreme one. To return to my own work on the Salem Witch Trials: Cotton Mather et. al. got their witches in 1692 (“success” for them, a grim “failure” for Rebecca Nurse), but lost control of the Massachusetts Bay Colony in the process (“failure,” though they may have been losing control of it anyway—see the point about counterfactuals, below). While not this extreme, many cases of “success” in the application of social knowledge also render somewhat questionable this whole analogy to natural science, which forms the core of what critical realists call “retroduction.” So, my remark in the text was designed to emphasize a crack in the foundations of the sophisticated realist argument, namely, that the transfer of “pragmatic realism” from natural to social science *does not work*. One cannot assign ontological validity to concepts based on the “success” of finding witches, making a revolution, or enacting a policy in the same way that, within certain intellectual boundaries, one *can* assign such validity to core concepts in the natural sciences (as Grover Maxwell (1962) famously argued). One has to pursue a different logic of, and different justifications for, the human sciences, rather than mimicking the realist philosophy of natural science.

Having said this, I should add that in the extended argument with Jim and members of the audience at the session on the book at the Social Science History Association conference, it became clear to me that something that I took for granted was not yet fully articulated as a workable position in social theory, namely that *interpretivism can be realist about natural processes*. So, I maintain: the social category of “witch” can have all sorts of denotations and connotations that give shape and

form the motivations of social actors, but one does not have to accept the idea that *spellcasting works* to build an interpretive sociological explanation for why certain people pursued those they identified as witches violently.² This is why I avoided the language of “social construction” in the text, because the connotations of that language suggest a suffocating blanket of relativism about knowledge, while in my view, interpretivism is the route to a more honest epistemic privilege for rigorous, expert knowledge in the human sciences, and does not, as an epistemological commitment for working social researchers, somehow inherently dispute the validity of scientific experiments that reveal the inefficacy of spellcasting.

It is in this sense, I suppose, that my view of interpretive sociology is grounded in the “ethos” of a truth-seeking science. If scientific knowledge is understood as making it possible to show that it is not witches making you sick, but viruses, then indeed sociology and biology share an underlying ethical project, because sociology will be able (one hopes!) to explain why people are so concerned with finding witches. But this shared ethos—what Jim might term the secret utopian grounding of interpretivism—should not cause us to conflate the analytic architectures and reflexive self-understandings of good practice in natural science with those to human science. Thus *Interpretation and Social Knowledge* set out to explore how we might proceed if we recognize that this specific, natural-science-influenced aspect of “realism” does not work, while taking on board many of the criticisms of positivist social science that are so well articulated by critical realism. Must the space of disputed interpretations in sociology be defined *either* by a realism where the best ontology wins *or* by a postmodern pragmatism *a la* Rorty where every knowledge claim is just another recontextualization? I say no.

So, the position offered instead is that of an explanatory or *causal hermeneutics*. I have come to view this position—which I refer to as interpretivism in the book—as more or less

² I want to thank Matthew Norton for his contributions to this discussion at the SSHA session, and for his suggestion of using the “does spellcasting work?” criterion for expressing this point.

compatible with the pursuit, via empirical research, of “historical ontology” (Hacking 2002). Let me, then, attempt to clarify a few points of contention about interpretivism raised by my critics. In discussing my argument for theoretical pluralism, Rick notes that I propose that interpretivists “pluck” theoretical concepts from various sources and use them to build up deep knowledge of cases. And he worries that this is a weird criterion or guide for knowledge-building—do researchers *have* to use multiple theories to construct their understanding of a case? After all, you can use Marx, as Rick himself did, without committing to Marxist ontology. Instead, it can be precisely the recognition of the partial use (and reinterpretation) of a concept from Marx that can lead to a good interpretive explanation. I think I agree with this point—explanations are partial explanations, and some partial explanations can use a single line of concepts. To clarify this further requires more work on “what is an explanation,” something I am currently pursuing. But in the text, I wanted to show how concepts taken from *ontologically incompatible theories* could be developed, via interpretive analysis, into a holistic comprehension of a case, and thus into a compelling interpretive explanation of a set of social actions. Thus I focused on the use of concepts taken from different theories to intensify the contrast with realism’s search for ontological coherence.

Interestingly, Rick’s question is the inverse of a more common question addressed to *Interpretation and Social Knowledge*: is this pragmatism of theory-use in the human sciences a form of “intellectual gerrymandering” which renders truth claims in sociology far too subject to the whims of the investigator, even if such theory-use is subject to a series of constraints vis-a-vis evidence marshaled (*ISK*, pp. 112-117, see Lizardo 2012)?

Here, then, is my overall response to both of these worries about the “use of multiple theories,” articulated in Chapter 4: If social theory is a jumble of abstract language games, sometimes organized (perhaps unfortunately) into schools, then “theory” exists, most of the time, one step removed from the social realities it is used to investigate. The location of rationality in sociological investigation, then, is in disputes over the maximal interpretation of minimally agreed upon, historically bounded

social phenomena, and *not* in the “triumph” of one school of theory over another, one “fundamental ontology” over another, or one “paradigm” or “research program” over another.

In comparative-historical sociology, maximal interpretations tend to be *explanations* of a set of well-defined phenomena. And we can have (and do have!) highly productive, evidence-based disputations over who has the best explanation, whether this or that explanation of this or that phenomenon needs to be augmented or modified, etc. But these will *not* be disputes over whether “Marxist theory” is true that somehow mirror the use of a crucial experiment to engage in arguments about whether the special theory of relativity is true. I can imagine showing my colleagues, with various pieces of evidence, why the Marxist explanation of the French Revolution, or even the Marxist explanations of 18th century revolutions, are wanting, or are bested by other explanations that approximate truth better, and so on. But I cannot see how this work—which is, in my view, where the rationality of sociology resides—would disprove, or alternately “augment” the “research program” of, *Marxism* (Burawoy 1990). Rather, new and better explanations refine and improve the conceptual toolkit to which we have access, when we want to offer a new and better theoretical interpretation of another piece of human history. This toolkit is made up of the overlapping abstract language games of social theory.

This leads to Lyn’s question: Are maximal interpretations esoteric, academic, and thus difficult to communicate publically? Perhaps. Prima facie, Lyn is right—in the schema I have presented, one has to be inaugurated, in one way or another, into the discourse of social theory and comparative-historical sociology to be able to engage in the “maximal” debates I value so highly. And yet, I think that social theory, and in particular the theoretical schemas that inform comparative-historical explanation are, in complex ways, related to broader cultural shifts in the societies in which academics function. This is a relationship that is, in my view, due for a new look—a new sociology of sociology, or sociology of intellectuals, might give us some insights here (e.g. Frickel and Gross 2005; Steinmetz 2005; Reed and Zald forthcoming). Thus I think there can be broader communication,

though perhaps it is not easy. I also think that it is important not to *overvalue* immediate public relevance; as I said above, interpretivism is designed as a new route to the epistemic privilege we have long associated with various formats of expert knowledge—and especially with science—in the modern West. As such, we should expect some distance between maximal interpretations and popular formats of knowledge to serve as a kind of Archimedean lever vis-à-vis the pieces of the world we want to explain. The difficulty is in creating this Archimedean explanatory torque out of *understanding* and the kinds of hermeneutic operations Wilhelm Dilthey associated with the human sciences, rather than by imitating certain images and methodologies of natural science.

In considering such an interpretive human science, Rick asks whether explanation-via-webs-of-meaning disables counterfactual supports for causal claims. There is much to be worked out here, and Hawthorne's book can be read a few different ways (I think I read it differently than Rick does). But, as a preview to a paper in progress I am currently working on—and with reference to Chapter 9 of Goertz's and Mahoney's new book (2012: 115-124)—I can say that there are better and worse counterfactuals, and a variety of ways of judging whether counterfactuals are "good." My point, in the book, was that in interpretive studies, the "good" counterfactuals are often "bootstrapped" from the in-depth knowledge of the case. Rick's own example shows how this sort of interpretive bootstrapping could be used to reject a certain counterfactual as "bad." However, there is a great deal of debate on this issue, and what is not well enough articulated in the book is how interpretive comparison would work—that is, how other cases, as well as counterfactuals developed from the intensive study of one case, would contribute to a good interpretive explanation. However, I will say that, in contrast to some in social theory and the philosophy of social science, I *do* believe that careful counterfactual thinking is important to causal research in social science, and especially in historical sociology.

All of which leads, of course, to meaning and mechanisms, causal priority, and so on. Here too, there is much work to be done. What I wanted to make clear in the book is that, first, it is possible to

find that some of social life works mechanistically, some of the time, without committing "paradigmatically" to becoming one of the "mechanistas" (Gross 2013). One need not give into the blackmail of *either* mechanisms *or* meaningful social life, choosing a side and saying that there are, or are not, mechanisms. This sort of argumentation remains tied to the need to present a single foundational social ontology before one pursues explanation. And, second, I wanted to show that "locating mechanisms historically" might mean very different things. Locating a mechanism in a "historical context" can be done *without* investigating the meanings involved as *part of the explanation* of the social actions under study, and it is this move that I objected to in *Interpretation and Social Knowledge*. Thus I agree with Lyn that, in some scholarly contexts, identifying a forming cause can itself be a massively important contribution that qualifies as an "explanation." It is the *a priori* exclusion of such meanings from a potential explanation that is the problem. And, in this sense, Rick is right that I adopt a Weberian position of seeking interpretations that are adequate in terms of both meaning and cause.

Comparative-historical sociology, perhaps more than any other field, has long taken inspiration from the work of Max Weber. The second wave of CHS linked Weberian methodology to the Millsian method, small-N comparison, experimental understandings of time (Sewell 2005), and hence to the pursuit of a certain kind of scientific legitimacy. In recent years, this version of the comparative-historical project has received further support from the mechanisms compromise (Gorski 2009) and, more broadly, from a realist understanding of social science. And great advances have been made as a result, including advances in theory and epistemology without which *Interpretation and Social Knowledge* would not have even been possible. We all stand on the shoulders of the giants of the second wave, and work with the philosophy of social science handed to us by Bhaskar, Habermas, and Runciman, among others.

Nonetheless, it is worth asking what these advances have cost comparative-historical sociology. In my view, overdependence on

naturalism in its various incarnations has limited the repertoire of thick causal concepts at our disposal, and re-inscribed a certain, unfortunate opposition between nomothetic and ideographic work. In the present day, this opposition takes the form of mechanisms, explanations and “realism” versus meanings, understanding and “interpretation.” Thought structured by these dichotomies misunderstands what is actually happening in interpretive work. More controversially, it misunderstands what the central problems and the great potential of the human sciences are. At least some of this misunderstanding is the result of the naturalist imperative, and the consistent importing of constructs from the philosophy of natural science as ways to understand our own work. Some versions of critical realism have worked to trouble this dichotomy (Steinmetz 2004; Gorski 2004), but in my view, they do not go far enough.

All of which leads to the question of what these interpretive explanations look like, and how to do them. In the book, I tried to take apart some exemplars of interpretive work to show their inner textual workings, and to articulate what interpretivism means for how we think about action, structure, culture, meanings, motivations, and mechanisms. But it is clear there is much more to be done. In particular, as Lyn points out, what is needed is an account of how causal hermeneutics works at the level of research design. This is the subject of a series of papers I am working on now with co-authors, specifically designed to delineate how research on forming causes and forcing causes can work in the pursuit of sociological explanations. Whether the version of interpretivism I am working on is or is not “Weberian” will be for others to decide. But in the meantime, I think it approximates an appropriate understanding, in equal parts descriptive and prescriptive, of what we do when we use theory to build explanations in comparative-historical sociology, and in the human sciences more broadly understood.

REFERENCES

- Burawoy, Michael. 1990. “Marxism as Science: Historical Challenges and Theoretical Growth.” *American Sociological Review* 55: 775-793.
- Frickel, Scott and Neil Gross. 2005. “A General Theory of Scientific/Intellectual Movements.” *American Sociological Review* 70: 204-232.
- Goertz, Gary and James Mahoney. 2012. *A Tale of Two Cultures: Qualitative and Quantitative Research Cultures in the Social Sciences*. Princeton: Princeton University Press.
- Gorski, Philip. 2004. “The Poverty of Deductivism: A Constructivist Realist Model of Sociological Explanation.” *Sociological Methodology* 34: 1-33.
- Gorski, Philip. 2009. “Social ‘Mechanisms’ and Comparative-Historical Sociology: A Critical Realist Proposal.” Pp. 147–94 in *Frontiers of Sociology*, edited by Peter Hedström and Björn Wittrock. Leiden and Boston: Brill.
- Gross, Neil. 2013. “The Mechanistas.” *Contemporary Sociology* 42: 368-370.
- Habermas, Jürgen. 1971. *Knowledge and Human Interests*. Boston: Beacon Press.
- Hacking, Ian. 2002. *Historical Ontology*. Cambridge, MA: Harvard University Press.
- Jameson, Frederic. 2004. “History and Class Consciousness as an ‘Unfinished Project’.” Pp. 143-152 in *The Feminist Standpoint Theory Reader*, edited by Sandra Harding. New York: Routledge.
- Latour, Bruno. 1993. *We Have Never Been Modern*. Cambridge, MA: Harvard University Press.
- Lizardo, Omar. 2012. Review of *Interpretation and Social Knowledge* by Isaac Ariail Reed. *American Journal of Sociology* 118: 497-499.
- Maxwell, Grover. 1962. “The Ontological Status of Theoretical Entities.” *Minnesota Studies in the Philosophy of Science* 3: 3-27.
- Reed, Isaac Ariail and Mayer N. Zald. Forthcoming. “Theorizing and the Unsettling of Communities of Inquiry in the Social Sciences.” In *Theorizing in the Social Sciences*, edited by Richard Swedberg. Stanford, CA: Stanford University Press.

Sewell, William, Jr. 2005. "Three Temporalities: Toward an Eventful Sociology." Pp. 81-123 in *Logics of History: Social Theory and Social Transformation*. Chicago: University of Chicago Press.

Steinmetz, George. 2005. "Scientific Authority and the Transition to Post-Fordism: The

Plausibility of Positivism in U.S. Sociology since 1945." Pp. 275-326 in *The Politics of Method in the Human Sciences: Positivism and Its Others*, edited by George Steinmetz. Durham, NC: Duke University Press.

Member Awards

Nancy Davis and Rob Robinson's book, *Claiming Society for God: Religious Movements and Social Welfare in Egypt, Israel, Italy, and the United States* (Indiana University Press, 2012) is the co-winner of the gold medal in the Religion category of the 2013 Independent Publisher Book Awards. The "IPPY" awards recognize books published by university and independent presses. The book also received the 2013 Scholarly Achievement Award of the North Central Sociological Association.

Robert D. Woodberry, National University of Singapore, received four outstanding article awards for "The Missionary Roots of Liberal Democracy" (2012, *American Political Science Review* 106: 244-274): three from the American Political Science Association (the Luebbert Award for Best Article in Comparative Politics; Best Article in Comparative Democratization; and runner-up for the Wallerstein Award for Best Published Article in Political Economy) and one from the American Sociological Association (Distinguished Article in the Sociology of Religion).

Enrique Pumar, Associate Professor and Chair of the Department of Sociology at the Catholic University of America, has been chosen as an Outstanding Author Contribution Award Winner for the Literati Network Awards for Excellence 2013 by Emerald Publishing. Emerald is a global publisher of more than 290 journals and more than 2,000 books and book series volumes. Pumar's article "National Development, Capability, and the Segmented Assimilation of Hispanics in Washington, D.C.," was part of a larger volume, *Hispanic Migration and Urban Development: Studies from Washington, DC*, that he published in October 2012. Editors of the book series Research in Race and Ethnic Relations (of which this book was one part) nominated his essay.

Member Publications

Books

Hall, Peter and Michele Lamont, eds. 2013. *Social Resilience in the Neoliberal Era*. New York: Cambridge University Press.

What is the impact of three decades of neoliberal narratives and policies on communities and individual lives? What are the sources of social resilience? This book offers a sweeping assessment of the effects of neoliberalism, the dominant feature of our times. It analyzes the ideology in unusually wide-ranging terms as a movement that not only opened markets but also introduced new logics into social life, integrating macro-level analyses of the ways in which neoliberal narratives made their way into international policy regimes with micro-level analyses of the ways in which individuals responded to the challenges of the neoliberal era. The book introduces the concept of social resilience and explores how communities, social groups, and nations sustain their well-being in the face of such challenges. The product of ten years of collaboration among a distinguished group of scholars, it integrates institutional and cultural analysis in new ways to understand neoliberalism as a syncretic social process and to explore the sources of social resilience across communities in the developed and developing worlds.

Contributors: Peter A. Hall and Michèle Lamont; Peter Evans and William H. Sewell, Jr.; Jane Jenson and Ron Levi; Will Kymlicka; Michèle Lamont, Jessica S. Welburn and Crystal Fleming; Leanne S. Son Hing; James Dunn; Lucy Barnes and Peter A. Hall; Daniel Keating, Arjumand Siddiqi, and Quynh Nguyen; Gérard Bouchard; Clyde Hertzman and Arjumand Siddiqi; Ann Swidler; Marcos Ancelovici.

Lewis, Penny. 2013. *Hardhats, Hippies and Hawks: The Vietnam Antiwar Movement as Myth and Memory*. Ithaca: Cornell University Press.

In the popular imagination, opposition to the Vietnam War was driven largely by college students and elite intellectuals, while supposedly reactionary blue-collar workers largely supported the war effort. In *Hardhats, Hippies, and Hawks*, Penny Lewis challenges this collective memory of class polarization. Through close readings of archival documents, popular culture, and media accounts at the time, she offers a more accurate "counter-memory" of a diverse, cross-class opposition to the war in Southeast Asia that included the labor movement, working-class students, soldiers and veterans, and Black Power, civil rights, and Chicano activists.

Lewis investigates why the image of antiwar class division gained such traction at the time and has maintained such a hold on popular memory since. Identifying the primarily middle-class culture of the early antiwar movement, she traces how the class interests of its first organizers were reflected in its subsequent forms. The founding narratives of class-based political behavior, Lewis shows, were amplified in the late 1960s and early 1970s because the working class, in particular, lacked a voice in the public sphere, a problem that only increased in the subsequent period, even as working-class opposition to the war grew. By exposing as false the popular image of conservative workers and liberal elites separated by an unbridgeable gulf, Lewis suggests that shared political attitudes and actions are, in fact, possible between these two groups.

An essay adapted from the book is also available in the May 13 edition of The Chronicle Review, in *The Chronicle of Higher Education*, entitled "Hard Hats, Hippies, and the Real Antiwar Movement" (<http://chronicle.com/article/Hard-Hats-Hippiesthe/139125/>).

Polillo, Simone. 2013. *Conservatives Versus Wildcats: A Sociology of Financial Conflict*. Stanford, CA: Stanford University Press.

For decades, the banking industry seemed to be a Swiss watch, quietly ticking along. But the recent financial crisis hints at the true nature of this sector. As Simone Polillo reveals in *Conservatives Versus Wildcats*, conflict is a driving force.

Conservative bankers strive to control money by allying themselves with political elites to restrict access to credit. They create new financial instruments in order to consolidate and reproduce their wealth over time, turning money into an instrument of exclusion, and couching their practices in ideologies of sound banking. Barriers to credit, however, create social resistance, so rival bankers—wildcats—attempt to subvert the status quo by using money as a tool for breaking existing boundaries. For instance, wildcats may increase the circulation of existing currencies, incorporate new actors in financial markets, or produce altogether new financial instruments to create change.

Using examples from the economic and social histories of 19th-century America and Italy, two decentralized polities where challenges to sound banking originated from above and below, this book reveals the collective tactics that conservative bankers devise to legitimize strict boundaries around credit—and the transgressive strategies that wildcat bankers employ in their challenge to this restrictive stance.

Ren, Xuefei. 2013. *Urban China*. Cambridge: Polity Press.

Currently there are more than 125 Chinese cities with a population exceeding one million. The unprecedented urban growth in China presents a crucial development for studies on globalization and urban transformation. This concise and engaging book examines the past trajectories, present conditions, and future prospects of Chinese urbanization, by investigating five key themes - governance, migration, landscape, inequality, and cultural economy.

Based on a comprehensive evaluation of the literature and original research materials, Ren offers a critical account of the Chinese urban condition after the first decade of the twenty-first century. She argues that the urban-rural dichotomy that was artificially constructed under socialism is no longer a meaningful lens for analyses and that Chinese cities have become strategic sites for reassembling citizenship rights for both urban residents and rural migrants.

The book is essential reading for students and scholars of urban and development studies with a focus on China, and all interested in understanding the relationship between state, capitalism, and urbanization in the global context.

Stampnitzky, Lisa. 2013. *Disciplining Terror: How Experts Invented Terrorism*. Cambridge and New York: Cambridge University Press.

Since 9/11 we have been told that terrorists are pathological evildoers, beyond our comprehension. Before the 1970s, however, hijackings, assassinations, and other acts we now call 'terrorism' were considered the work of rational strategic actors. 'Disciplining Terror' examines how political violence became 'terrorism,' and how this transformation ultimately led to the current 'war on terror.' Drawing upon archival research and interviews with terrorism experts, Lisa Stampnitzky traces the political and academic struggles through which experts made terrorism, and terrorism made experts. She argues that the expert discourse on terrorism operates at the boundary - itself increasingly contested - between science and politics, and between academic expertise and the state. Despite terrorism now being central to contemporary political discourse, there have been few empirical studies of

terrorism experts. This book investigates how the concept of terrorism has been developed and used over recent decades.

Sun, Anna. 2013. *Confucianism as a World Religion: Contested Histories and Contemporary Realities*. Princeton: Princeton University Press.

Is Confucianism a religion? If so, why do most Chinese think it isn't? From ancient Confucian temples, to nineteenth-century archives, to the testimony of people interviewed by the author throughout China over a period of more than a decade, this book traces the birth and growth of the idea of Confucianism as a world religion.

The book begins at Oxford, in the late nineteenth century, when Friedrich Max Müller and James Legge classified Confucianism as a world religion in the new discourse of "world religions" and the emerging discipline of comparative religion. Anna Sun shows how that decisive moment continues to influence the understanding of Confucianism in the contemporary world, not only in the West but also in China, where the politics of Confucianism has become important to the present regime in a time of transition. Contested histories of Confucianism are vital signs of social and political change.

Sun also examines the revival of Confucianism in China today and the social significance of the ritual practice of Confucian temples. While the Chinese government turns to Confucianism to justify its political agenda, Confucian activists have started a movement to turn Confucianism into a religion. Confucianism as a world religion might have begun as a scholarly construction, but are we witnessing its transformation into a social and political reality?

With historical analysis, extensive research, and thoughtful reflection, *Confucianism as a World Religion* will engage all those interested in religion and global politics at the beginning of the Chinese century.

Articles

Aminzade, Ronald. 2013. "The Dialectic of Nation-Building in Post-Colonial Tanzania." *The Sociological Quarterly* 54: 335-366.

Bortoluci, José and Robert S. Jansen. 2013. "Toward a Postcolonial Sociology: The View from Latin America." *Political Power and Social Theory* 24: 199-229.

Section Awards

BARRINGTON MOORE BOOK AWARD

Committee: Richard Lachmann (Chair), Stefan Bargheer, Gurminder Bhambra

Co-Winners: Michael Mann (UCLA). 2012. *The Sources of Social Power, volume 3: Global Empires and Revolution 1890-1945*. Cambridge: Cambridge University Press; Monica Prasad (Northwestern). 2012. *The Land of*

Too Much: American Abundance and the Paradox of Poverty. Cambridge, MA: Harvard University Press.

Honorable Mention: Andreas Wimmer (Princeton). 2012. *Waves of War: Nationalism, State Formation, and Ethnic Exclusion in the Modern World*. Cambridge: Cambridge University Press.

CHARLES TILLY BEST ARTICLE AWARD

Committee: Isaac Reed (Chair), Krishan Kumar, Aaron Major, Alvaro Santana-Acuña

Winner: Elisabeth Anderson (Northwestern). 2013. "Ideas in Action: The Politics of Prussian Child Labor Reform, 1817-1839." *Theory and Society* 42: 81-119.

REINHARD BENDIX STUDENT PAPER AWARD

Committee: Nina Bandelj (Chair), Claire Decoteau, Matthew Norton, Nicholas Hoover Wilson

Winner: Yael Berda (Princeton). "The Peculiar Persistence of Colonial Legacies: Why New Nations Reproduce State Practices against which their Founders Struggled."

Honorable Mention: Deirdre Bloome and Christopher Muller (Harvard). "Slavery and African-American Marriage in the Postbellum South, 1860-1880."

THEDA SKOCPOL DISSERTATION AWARD

Committee: Colin Beck (Chair), Erin Murphy, Ben Herzog

Winner: Jaeun Kim (Stanford). "Colonial Migration and Transborder Membership Politics in Twentieth-Century Korea."

Honorable Mention: Kevan Harris (Princeton). "The Martyrs Welfare State: Politics of Social Policy in the Islamic Republic of Iran."

List of ASA 2013 CHS Events

MONDAY, AUG 12

The Return of the Revolution?

2:30 - 4:10 pm

Session Organizer: Hazem Kandil (University of California-Los Angeles)

Containing Nationalism in World-History: A Longue Durée Analysis of State-Seeking Nationalist Movements

*Sahan Savas Karatasli (Johns Hopkins University)

Cuba and Venezuela: Revolution and Reform

*Silvia Pedraza (University of Michigan), Carlos A. Romero (Universidad Central de Venezuela)

The Revolution Might Be Televised: The Arab Spring as the Future of Revolutions?

*Daniel P. Ritter (Stockholm University), Alexander H Trechsel (European University Institute)

Reflections on a Failed Revolution “from the Middle”: Middle-Class Breakdown and Political Hyperpolarization in Venezuela, 1970s-2008

*Celso M. Villegas (Kenyon College)

Discussant: Hazem Kandil (University of California-Los Angeles)

Author Meets Critics Session: Michael Mann’s *Sources of Social Power*, volumes 3 and 4
4:30 - 6:30 pm

Author: Michael Mann

Critics: Edgar Kiser, Miguel Centeno, Liliana Riga, Philip S. Gorski

TUESDAY, AUG 13

Section on Comparative-Historical Sociology Council Meeting
8:30 - 9:00 am

Section on Comparative-Historical Sociology Business Meeting
9:00 - 10:00 am

All are welcome! Awards distributed at this meeting.

Section on Comparative-Historical Sociology Invited Session. Debate: How to do Comparative Historical Research

10:30 am - 12:10 pm

Organizers: Andreas Wimmer, Emily Erikson

Panelists: Charles C. Ragin, Ivan Ermakoff, Julia Adams, John F. Padgett

Paper Session: Historical Sociology and the History of Sociology
12:30 - 2:10 pm

Session Organizer: George Steinmetz (University of Michigan)

Emancipation and the Sociological Tradition: The African American Contribution

*Gurminder K. Bhambra (University of Warwick)

General Equilibrium Theory Traveling Behind the Iron Curtain

*Olessia I. Kirtchik (Higher School of Economics), Ivan Boldyrev (Higher School of Economics and Humboldt University)

The Impact Factor Fetishism

*Christian Fleck (University of Graz)

Universities, Law, Jurisprudence, and Sociology: A History

*Eric Lybeck (University of Cambridge)

Discussant: Henrika Kuklick (University of Pennsylvania)

Paper Session: Global and Historical Perspectives on Policy Diffusion: Mechanisms, Directions, and Levels of Analysis

2:30 - 4:10 pm

Session Organizer: David Scott FitzGerald, David A. Cook-Martin

Categorizing Populations: How Bureaucracy Shaped Citizenship in the Former British Empire - India, Israel and Cyprus

*Yael H. Berda (Princeton)

Complexities in Global Diffusion of Abortion Liberalization: Abortion Policy in China, Russia, and Nicaragua

*Elizabeth Heger Boyle (University of Minnesota), Wenjie Liao (University of Minnesota), Jasmine Trang Ha (University of Minnesota), Lisa Gulya (University of Minnesota)

How their Law Affects our Law: Mechanisms of Immigration Policy Diffusion

*David Scott FitzGerald (University of California-San Diego), David A. Cook-Martin (Grinnell College)

Producing Global Health: Global Policies and Local Actors in Gujarat State, India

*Peggy Levitt (Wellesley College), Jennifer A. Holdaway (Social Science Research Council)

Unpacking Policy Diffusion: A Textual Analysis of Refugee and Population Policies across African Countries

*Rachel Sullivan Robinson (American University), Katherine Tennis (American University)

WEDNESDAY, AUG 14

Comparative-Historical Sociology and Political Sociology Section Joint Mini-Conference:

“CAPITALISM, THE POLITICS OF INEQUALITY, AND HISTORICAL CHANGE”

Columbia University. For more details, please visit: <http://capitalisminequalitychange.publishpath.com>.

Symposium Announcement

**Reenvisioning the History of Sociology:
Recognizing Social Theorists, Reconceptualizing the Social World**

A Symposium featuring Doctoral Students & Early Career Sociologists

August 10, 2013
The New School for Social Research, New York, New York
Wolff Conference Room

*Sponsored by the History of Sociology Section of the
American Sociological Association*

We are very pleased to announce the preliminary program for a History of Sociology Symposium to be held in conjunction with the American Sociological Association's Annual Meeting in New York City. In the Fall, we issued a Call for Papers, seeking contributions to a Symposium discussion about the role of sociology's history, in relation to its present and future. In response to our call, we received 23 excellent paper submissions from graduate students and early career sociologists. Our submitters hail from Latin America, Europe, and Asia, as well as North America. We are now in the process of finalizing what promises to be a very exciting and dynamic program.

The Symposium will be held on August 10, 2013 in the Wolff Conference Room at The New School for Social Research, which is located in Union Square. We are particularly grateful to Jeffrey Goldfarb and Vera Zolberg, as well as the New School administration, for all their help in obtaining space at the New School. Given the quality of our paper submissions, we are planning for a full-day symposium, with breaks for coffee and lunch. Coffee and bagels will be available at 8:30 a.m., and the Symposium will begin at 9.

Our tentative program will include three paper panels, featuring Martin Bulmer, Jeffrey Goldfarb, and Jeffrey Olick as discussants. We are also considering possibilities for a concluding discussion panel that will feature the perspectives of more experienced sociologists. Emerging themes for the panels include the following:

- *Recognizing Social Theorists and Methodologists* – a panel focusing on ways that the history of sociology helps us see a place in sociology's canon for previously-marginalized groups, figures, and perspectives;
- *Reconceptualizing the Social World* – a panel focusing on ways that the history of sociology helps us to theorize the social world anew;
- *Reframing the Sociological Field* – a panel focusing on ways that the history of sociology helps us to see new possibilities for the discipline of sociology;
- *Reenvisioning the History of Sociology* – a reflective discussion, focusing on common themes in the Symposium, and drawing conclusions about the ways that the history of sociology can be most effectively deployed in supporting new developments in sociological theory and methods.

Please join us at the New School for what we hope will be an engaging and thought-provoking discussion!

Co-Organizers: Michael Bare (University of Chicago) & Laura R. Ford (Cornell University)

2013 Section Election Results

Chair-Elect: Bruce Carruthers (Northwestern)

Council Members: Sarah Quinn (Washington), Nitsan Chorev (Brown)

Student Representative: Laura R. Ford (Cornell)

Call for Member Information

Let's make sure that the website of the Comparative and Historical Sociology section remains a vibrant hub of intellectual exchange! Please keep the Web Editor updated with your latest information, including: (1) the current link to your professional webpage; (2) citation information and links to your latest article and book publications; (3) announcements and calls for upcoming jobs, conferences, and publications pertaining to comparative and historical sociology. And be sure to visit the website (<http://www2.asanet.org/sectionchs/>) to learn about recent and upcoming section activities – and to browse current and back issues of the newsletter.

Please email your information to Kurtulus Gemici, CHS Web Editor:
kgemici@nus.edu.sg

Contributions to *Trajectories* are always welcome. Please contact the editors at
atesaltinordu@sabanciuniv.edu and seio@hawaii.edu.