Interpretive Explanation and Its Discontents: Author's Reply to Commentaries

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I can only begin by thanking the contributors to this symposium, and its editor, Marek Skovajsa. Particularly after the latest work speedup in academic production (circa 2008, for rather obvious reasons), it is gratifying in the extreme to see one's interlocutors and editor treat one's efforts with such care and attention, attention that goes far beyond any reasonable instrumental rationality. Whether or not it deserves it, my book has been gifted a set of critiques, some sympathetic, others deeply sceptical, that together are hermeneutic in the original sense of careful attention to (and attempts to correct) a text. This is wonderful stuff, indeed, and I only hope the following reply does justice to the essays to which it responds. Certainly, for me, reading the critiques and writing this reply has clarified the implications of the argument of *Interpretation and Social Knowledge* [Reed 2011] (hereafter *ISK*) for the practice of social research today.

Landscapes

Nelson Arteaga Botello and Eeva Luhtakallio both develop immanent critiques of the landscape metaphor, noting its relative underdevelopment in the text. Their arguments are evocative, and constitute creative interpretations of the original statement of the metaphor and thus conceptual innovations. I affirm these lines of development, and only wish to clarify what should be retained as a point of focus.

Luhtakallio notes that the metaphor is static and emphasises the visual. As a remedy to the first problem, she proposes 'sedimentation' as a way to think about shifting landscapes. As a remedy to the second, she proposes getting even further into the muck of social life, and thus pulls the metaphor in a specifically

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ethnographic direction. I certainly agree with the need for a temporal metaphor to accompany the landscape metaphor, and sedimentation is promising, though I would argue it should be paired with a metaphor that captures the potentially abrupt and perhaps violent transformation of certain landscapes of meaning. For example, beginning with the French Revolution and through Napoleon's reign, the people of France witnessed several attempts to radically overhaul—one might say terraform—certain meanings that attended everyday life, including religious symbols, calendars, and clocks. Whether or not such attempts at terraforming fail, they are surely part of social life we should examine.

Interestingly, however, Luhtakallio's second suggestion with regard to landscapes—that we get into smell and touch—is somewhat at odds with Arteaga's critique, since for him the metaphor is insufficiently discursive in its proposed reach and significance for social theory. For Arteaga, the potential utility of the metaphor rests with its ability to grasp how society as a whole is imagined, both by the actors we study and by sociologists themselves. He thus urges its use to transcend an opposition, in social theory, between systems theory and postmodernism. Arteaga wants to replace a notion of interlocking subsystems, for example, with overlapping landscapes, and, as such, his suggestion is at odds with a more strictly interactionist focus.

However the metaphor develops, I want to keep in mind that it was intended to enable the analyst to use the concept of subject position in a more precise and subtle way. We are exceedingly familiar, in social theory, with the idea that individual human actors confront a world that is both made and yet not up to them to remake; and that, in so far as they are formed by their social world, said world forms their perspective on that world as well. The long arc of social theory from Karl Marx to Pierre Bourdieu is the dominant trend here. The central goal of introducing the landscape metaphor was to suggest how this insight could be rendered in a way that allows the comprehension of the aesthetic, complex, and sometimes subtle ways in which the construed worlds into which actors walk *vary*, because I felt that, in the interests of parsimony, the sociology of subject position often sacrificed its ability to interpret complex variation.

In field theory, a subject position, and thus the view of the field contained in the subjectivity of the actor, is computed in terms of two or three axes—usually concerning autonomous and heteronomous poles, and different sources of capital. These are simultaneously symbolic and social, and thus Bourdieu's model is not acultural. But when I read empirical studies of fields, and more broadly arguments about subject-position and symbolic power, I am always struck by the way in which the principles of vision and division subscribed to by subjects seem to have much more to do with context-bound and content-full distinctions of meaning, and specifically the ambiguous meanings of certain signs, than they do with the positionings that so concerned Bourdieu. So, I thought that 'landscape' might be a good addition to the theoretical toolkit. The question, then, is what is fielded, and why, as a subset of a larger category of landscapes of meaning.

The primary way landscapes vary is in the meanings that objects, words, and bodies signify to humans who are in the (metaphorical) landscape, both in the sense of long chains of associated signifiers and signifieds, and in the sense of referents. What it means to occupy part of a landscape, as an actor, then—that is, to take up a subject position—is that one inherits both a viewpoint and a capacity for action that varies from that of others within the landscape, and also varies in terms of the subject's position on other landscapes. In this sense, Arteaga is exactly correct that I should have emphasised the way in which figures in the landscape express or embody 'the confluence of different creative, recreational and passive forces in different parts of the landscape'.

The underdevelopment of the landscape metaphor *qua metaphor* provided a meeting point for critics who, like Luhtakallio and Arteaga, were inclined to take the humanist-interpretive leap with me (and perhaps ended up feeling that I did not leap far enough), and those who were much more reticent to turn social science into human science, for a variety of well-articulated reasons. In Welch's frustration with the rendering of causal imagery in the text and in Vollmer's point that there are actual physical landscapes, we see a much more sceptical take on the riot of metaphor that makes up chapter five of *ISK*. In addition to providing a nice dovetail between the scientific and humanist critiques of the book, the metaphorical and literal meanings of the term landscape raise, in turn, the difficult issue of materiality.

Materiality and semiotics

'Material'—in the form of bodies, the natural environment, the built environment, and technology—appears in many of the critiques of the book. This indicates that the issue of materiality is central to understanding both the limitations of the text, but also the way in which it can open on to a more elaborated, subtle, and effective approach to the use of theory to construct interpretive explanations. How should interpretivism comprehend the material?

Iconicity is certainly an excellent possibility, but unfortunately, beyond this, Bartmański and Binder appear so outraged at the Saussureanism in *ISK* that they will throw anything at it (the real, the body, the spectre of Bishop Berkeley…). They thus miss an opportunity to engage the Peircean themes in the book, and, despite their ritualised citation of Judith Butler, do not articulate how key questions concerning bodies and social research intersect with the kind of historicised, contextually sensitive explanations advocated in *ISK*. After all, the text took Susan Bordo's [2003] classic interrogation of the female body, discourse, and anorexia as an exemplar of the interpretive epistemic mode. Bodies, and their movement in space, can and should be central for the interpretive epistemic mode precisely because of the way they are a crossroads for different dimensions of signification. First of all, bodies and the imagination of and desire for bodies, tend to enter the

world simultaneously as signifiers, signifieds, and referents, thus making them a semiotic hotbed for power and action. Second of all, the tendency of bodies to messily exceed signification is a source of constant dynamism in human life precisely because in so doing they spur people to impose interpretations upon them nonetheless; these interpretations can strive for singularity or work via multiplicity, as in Annemarie Mol's *The Body Multiple* [2002]. Thus bodies matter indeed, but Bartmański and Binder do not consider that this demands even more analysis of representation, not less.

Meanwhile, Luhtakallio and Vollmer raise a much more disturbing and complex issue for interpretivism when they discuss non-humans and the material more generally. Here the reference is clearly (for Luhtakallio, at least) Actor Network Theory, and in raising this issue, she has surely identified the central omission of the text. I can only briefly enact an engagement with ANT here, and beg the reader's indulgence with an issue that clearly needs a much longer treatment and awaits the completion of work in progress.

I am compelled to begin with the admission that *ISK* participates in and advances a format of humanistic discourse that is at odds with the *rhetoric* of ANT. This is evident in the book's use of Aristotle and the reliance upon Alastair MacIntyre and Michael Walzer; in its obsessive inquiry into subjectivity, motive, and reasons as causes; in its attempts to salvage certain aspects of Michel Foucault's work as 'interpretive' despite his attack on hermeneutics; in its use of certain insights from the American ethnographic tradition; and, most obviously, in the framing of its overall argument in terms of a distinction between human and natural science.

However, when we look closely at what is really at stake in ANT's engagement with and reformulations of social research (rather than its attacks on the philosophy of science), we find a clear alliance between the project of ANT and the interpretive epistemic mode as I explicated it. This is true, first, at the level of epistemic values: both are interested in careful, historically bounded investigations (with some dispute about 'interpretive explanation', 'thick description' and 'description' lingering), and both take the multiplicity of signification as primary to these investigations. Furthermore, both express scepticism about the ontological unity of objects that are posited, without much investigation, as parsimoniously explaining vast reaches of social life in a more social realist tradition. In *ISK*, I objected to the realist 'short circuit' of interpretation via ontological social theory; in many of its key statements, ANT erodes 'foundational distinctions' in remaking research and thus reassembling the social.

This commonality becomes even more fulsome when we move to ANT's desire to avoid determining, in advance, the scope and scale of the heterogeneous networks and co-produced materiality and sociality that it studies. Instead, ANT wants to trace empirical variation in the spread of networks and their capacity to impose and enact their realities upon the rest of the world. If they are useful for anything, 'macro' and 'micro' are, for ANT, not to be understood as foundational

categories, but rather as the *effects* of getting something to hold together (the classic here is clearly Callon and Latour's 'Unscrewing the Big Leviathan' [1981]). *ISK* argues something similar about meaning and signification as a formal cause, in the sense that it too wants the socio-temporal expanse of discursive formations to be a point of empirical investigation, and thus suggests we write theory in such a way that we can allow this expanse to vary. Thus, *ISK* also resists the tendency of social theory to jam the world into a layer-cake ontology,[1] and argues instead that we should develop tools with which to *trace* the scope, depth, and variable power of different discursive formations, meaningful tropes, etc.

All of which leaves the controversy over ANT's treatment of non-humans. Although I suspect that my use of Aristotle would count as the kind of 'special' treatment of the human actor that ANT criticises,[2] I do not see the allowance of causality to non-humans as problematic to the interpretive epistemic mode; what is required is a reinterpretation of the category of material cause (as Luhtakallio argues in discussing the bronze in the statue). There is no reason an interpretive explanation cannot recognise the particular materiality of a newly designed prison wall as part of what holds together the state as a heterogeneous assemblage, and it seems equally clear that in creating a 'material semiotics', ANT has provided a corrective to, rather than a total overhaul of, the analysis of discourse, field, practice and landscape. In other words, humans indeed need tools and train tracks to hold together the Leviathan. But they also need fear in the population, the myth of the state, and the institutionalisation of different formats of legitimate domination. The comprehension of materiality pushed by ANT, then, can be thought of as a needed extension and revision of, rather than a fundamental challenge to, the Weberian semiotics at the core of the interpretive epistemic mode.

Modesty, comparison, and causality

ISK also attempted to apply a semiotic analysis to social research itself, and in so doing criticised the Kuhn-inspired notion of 'theory-laden' data and its opposite, the supposed empiricists that Kuhn supposedly unseated. The well-worn philosophical opposition between empiricist and post-Kuhnian approaches to science does not grasp what is going on in social research, which is in fact a spectrum of interpretation, neither positivist nor relativist. This is to say that—to put it in Kuhn's terms—the laden-ness of facts in concepts is quite different, in most instances, from the laden-ness of explanations, normative arguments, etc. in concepts, and it is usually the latter concepts that we identify as part of 'social

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[1] Bartmański and Binder note that a stratified ontology is 'conspicuously missing' from the book. I'm not sure if it was conspicuous, but it was definitely intentional.

[2] Law writes that, in ANT, 'as with Foucault, there is a powerful if controversial non-humanist relational and semiotic logic at work quite unlike that of humanist sociology' [2007: 8].

theory'. The difference between minimal and maximal interpretation is indeed a difference of degree in the indexicality and relationality of the signs involved, but the difference of degree matters a great deal. For, we are able to go on in social research, in the pursuit of truth, precisely because our always tenuous maximal interpretations can rely upon and marshal minimal ones in making their case to a given community of inquiry.

Thus my point in distinguishing minimal and maximal interpretation—and indeed in choosing language that indicates a spectrum and differences of degree (from 'min' to 'max')—was to suggest that the overlapping communities of inquiry in social research tend to have certain points of agreement, usually centred on thin descriptions of phenomena, and then more and more disagreement as theoretically-driven interpretations of the evidence emerge, offering causal explanations, normative critique, etc. To quote one sociologist who has grasped precisely what is at stake here, maximal interpretation is 'related to what empiricists tend to call "generalization," but without the epistemological baggage that allows a sample to represent (*darstellen*) the whole … The important move here is to argue that maximalist interpretation is *always* tenuous, that is, always requires the active interpretation of the analyst' [Perrin 2012].

The results of looking at social research in this way seem to have unnerved a positivist (Welch), infuriated two realists (Bartmański and Binder), and disappointed a supporter of general theory (Vollmer). The clear point of contention is that I do not find, in social research today, anything that does, can or should look like normal science in the Kuhnian sense, or even a revolutionary science constituted by paradigm wars, whereas an image of normal science and paradigms grounded in ontology appears to unite my critics in their doubts, whatever their differences with each other may be. Welch suggests that I reject evidence-based practices in human science in the name of anti-imperialism, which is clearly not true, and that I unnecessarily limited the capacity of the text to address the theory-evidence nexus by choosing realism as an opponent, which is clearly true. Perhaps this latter point is what Bartmański and Binder mean by saying that realism is the constitutive other of interpretivism in the text.[3]

When Vollmer discusses commensurability and incommensurability, he identifies a key point, though the argument is unfortunately merged with his arguments about ontology. Nonetheless, an implication of his argument is both true and important as a description of a key epistemic problem in the human sciences: between the inviting commensurability of the world to general comprehension that he advocates, and the emphasis on particularity that I argued for, lies the vexing question of the comparison of cases. Finally, Lukes' reading

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[3] In this regard, perhaps a discussion of the Peircean dimensions of the text, and the development of a conformist theory of truth in the philosophy of knowledge, based in a reading of C. S. Peirce [Longino 2002], which informs both the text and its diagrams, could be an instructive way forward for the interpretivism/realism debate.

is in some ways the most sensitive of those that address the issue of naturalism/ science, for he identifies a diffuse but important feature of the text as a whole, namely the idea that we should continue to *develop* abstract, even grand theories, but should adopt a deep modesty and humility in how we *use* them. Modesty to me here signifies several different aspects of good practice: attention to the various meanings in a case, extreme care about the prospects for and conditions of generalisation, and a willingness to sacrifice adherence to a 'paradigm' (really, overvaluation of coherence in the abstract theoretical architecture brought to bear on the case) so as to make sense of the case itself. This last point is what Luhtakallio finds liberating about the book. Thus Lukes is correct to find common ground between *ISK* and Elster's 'substantive knowledge'. Lukes then asks: what understanding of causality we should develop?

Put together, Vollmer's and Lukes' critiques hit upon what is perhaps the central conceptual problem for the interpretive epistemic mode going forward: *what, given the emphasis on theoretical pluralism in the pursuit of in-depth case knowledge, is the role of comparison?* And thus: how does the problem of comparison relate to the reconceptualisation of causality proposed in *ISK*? For, note that Lukes gestures towards J.L. Mackie's *Cement of the Universe,* which has become the foundational philosophical tract for comparative-historical methodology as it has moved beyond the positivism and Millsianism of the opening chapter of Skocpol's *States and Social Revolutions.* Nowhere, in other words, are INUS causes taken more seriously in social science today than in my own subfield of comparative-historical sociology! And thus Lukes and Vollmer reveal the fundamental tension between *ISK* and the methodology debates in American comparative-historical work. For, the book's argument sits *outside of* the 'Tale of Two Cultures' told by Goertz and Mahoney [2012], wherein they distinguish social science concerned with average effects and grounded in the mathematics of regression ('quantitative') from social science concerned with comparative case studies grounded in the mathematics of set theory ('qualitative'). In other words, to answer Lukes' question, I am not happy with the Mackie-reliant rebuilding of social science constituted by fuzzy set qualitative comparative analysis (fsQCA); he may, however, have had a different route out from Mackie's text in mind.

Rather, in contrast to the 'age of regression', grounded theory ethnography, and the more recent fsQCA, the argument of *ISK* suggests that it is the complexity of theoretical interpretation that makes a case into a case. Nonetheless, Vollmer is right that I did not provide a sufficient account of comparison in my efforts to push hard (perhaps too hard) for historically-bound interpretive explanation. In particular, my exemplars are not comparisons, at least not explicitly so. Let me, then, try to say here how I imagine comparison working in the interpretive epistemic mode.

First of all, although I view abstract theoretical coherence as something that should be sacrificed to make sense of a case while retaining verifiability in the piece-by-piece interpretation of groups of evidentiary signs with theory [see

Reed 2011: chapter four, 114–115], I retain a commitment—as should be clear from the analyses of Bordo and Geertz—to case-based knowledge as deeply theoretically mediated. Indeed, it is this that marks the departure of the text from certain ethnographic traditions in the USA that eschew theory (which I would criticise as unwilling to take the risk of maximal interpretation), and from the deflationary rhetoric of ANT that rejects explanation for description. So, a case is a case because of theoretical interpretation.

This means that a comparison of cases will, first and foremost, have theoretical interpretation as a mediating intellectual process. This will take place in relation to the concerns of a given community of inquiry grounded in certain clearly articulated questions that demand, as answers, maximal interpretations. This is already implicit in the subfield of comparative-historical sociology, wherein, for some questions, the French Revolution should be compared to the American, whereas for others it should be compared to the Iranian or the Nicaraguan. But then a difficult problem arises: what is the point of these comparisons, given that in interpretivism, explanation actually happens at the much more concrete, historically bounded level of the case itself?

In my view, *theoretically mediated interpretations of other cases provide a route to good counterfactuals in the interpretive analysis of a case*.[4] That is to say, if we have an explanatory problem about, say, the role of charismatic authority in the Bacon's Rebellion, then a complex, theory-laden interpretation of charismatic authority (or, perhaps, its non-importance) in the Whiskey Rebellion may provide an essential 'difference to be explained' in the first case.[5] That is, out of the infinity of counterfactuals that exist for a chain of occurrences under scrutiny, the 'good counterfactuals' are chosen, not only from detailed knowledge of the case itself— which is the route preferred by historians and certain ethnographers who eschew theory—but also via the theoretical interpretation of other cases, which provide maps of other ways things could have gone. Note, of course, that there is a great deal of fore-knowledge that has to be developed and reasonably established to set up a comparison in this way, including the meaning and appropriateness of a theoretical term like 'charismatic authority'. In other words, comparison exists within, and not as a brake on, the hermeneutic circle. In this sense, I imagine comparison as something that becomes possible neither because of a kind of immediate, de facto commensurability, nor because of a single theoretical language that unites all cases, but rather through the work of theoretical interpretation to link certain cases to one another, piece by piece, in relationship to a given research question. The result of this is a better specification of the explanatory problem that is defined and bounded historically in the original case study.

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[4] This was somewhat adumbrated, in *ISK,* in footnotes 37 and 77 in chapter five, which dealt with Fritz Ringer's work on Weber and made reference to Geoffrey Hawthorn's *Plausible Worlds* [1991] as inspiration.

[5] All of the usual caveats about cases not having to be defined nationally apply.

Clearly, this kind of intensive comparative work requires serious, in-depth, interpretive knowledge of the other case or cases as well, and thus I imagine this as a model for how and why we should pursue small-N comparison in historical sociology; in contrast, the formalisation of INUS causality via set theory cannot easily account for the massive theoretical relevance we frequently attribute to studies of just 2 or 3 social movements or revolutions. Indeed, I think that there is a deep continuity between INUS causality and the sorts of commensurable comparisons that Vollmer wants, which I would contrast with the route that we could take in the interpretive epistemic mode. For the latter, I would point to 'thick causal concepts' and the 'dappled world', both ideas drawn from the work of the philosopher Nancy Cartwright [1999, 2007]. For Cartwright, the world is dappled into overlapping zones wherein different causal concepts provide the torque necessary for explanation. Without explicating her perspective here,[6] let me just say her argument that we should *not* seek to make cause mean one and only one thing in a very precise, analytic sense certainly inspired my turn to Aristotle in *ISK*, even if my reading of Aristotle differs significantly from hers [see Reed 2011: 143, fn. 42]. Having arrived at this description of explanation and comparison as I see it, I can now return to the issue of naturalism.

Nature and society

Several commentators critique the anti-naturalism of *ISK*, but this is done via two different tendencies of argument. The first argues that the text underestimates the utility of general theory and, in a broad sense, positivism for moving social research forward. The second defends realism by arguing that sociological realism's strengths are the way it has been or can be amended or retooled to suit the human sciences (usually via engagement with, e.g., agency, concept-dependence, open systems and historical variation). So, in a certain way the objections, though they run together in many of the texts, are in fact mirror images of each other: for one, the science is there to be done, and *ISK* is to be grouped in with a larger set of distractions, including postmodernism and critical realism; for the other, the brilliance of realism in social science is precisely that it has already satisfactorily adjusted to all of the objections *ISK* raises. Somewhere in middle of this is the problem of 'ontology'.[7]

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[6] Cartwright's notion of the dappled world was originally developed in relationship to the natural sciences, and thus her causal pluralism rests on an Aristotelean concept of 'natures' which does not, in my view, work for social research. But the concept can be developed in a hermeneutic direction. See Nordman [2008].

[7] Note, however, that the way in which these arguments develop—'we need to leave hermeneutics behind and build a general positive science already' versus 'we need to be hermeneutic, and, voilà, critical realism already is'—implicitly vindicates the utility of explicating an interpretive epistemic mode separate from either realism or positivism.

Bartmański and Binder write that I fail to recognise the way in which critical realism (CR) articulates important differences between the natural and social sciences, citing the usual litany of CR terms including open systems and existential intransitivity. The only thing missing is the ritualised invocation of the story that Roy Bhaskar could have named his second book *The Impossibility of Naturalism*. However, in their hurry to defend CR and attack the semiotics in *ISK*, they missed entirely the essence of my critique of CR (and they missed, indeed, my careful consideration of precisely the adjustments they laud in Bhaskar). My critique has little to do with the existence or non-existence of human bodies (I think they exist and should be part of explanation), or the existence of the state and the utility of having a passport (I agree territorially-defined states structure flows of human travel, in many or perhaps most places in the globe).

Rather than going down the rabbit hole of trying to determine what really, really, really exists, in *ISK* I tried to analyse the structure of arguments in social science, and this led me to a criticism of a fundamental mistake made by Bhaskar concerning how the use of theory in explanation works in social research. That mistake was to suggest that the *inference to structure* that enables sociological explanation of a certain sort—Marx's explanation of action within and actors' understandings of the dynamic socio-economics of capitalism, for example—is a *transcendental* argument, equivalent to the classic transcendental arguments of Immanuel Kant, and Bhaskar's own earlier 'derivation' of the necessary existence of a natural world and the rationality of science from the existence and success of scientific experiments. My contention is that to suggest that Marx's argument is transcendental is to reveal a deep misunderstanding of the nexus of theory and evidence that produces explanations, and more broadly, maximal interpretations, in the human sciences.

This mistake is important beyond CR and debates in the philosophy of social science because it is an incorrect philosophical justification for, and I would hazard a symptom of, an unfortunate tendency that is quite common in social theory and sociological research, whether those who make it are proponents of CR or not. That is the tendency to *short-circuit* the pursuit of explanations, or more generally, maximal interpretations, via the following manoeuvre: elaborate a theory of how society or the social works, make sure it is internally consistent, then take the theory as general and as directly referential (though perhaps in need of revisions at its edges), and thus finally posit those theoretical workings as the underlying explanation of an incredibly wide variety of minimally investigated actions. (Side note: this critique resonates deeply with Elster's critique of rational choice theory, as explicated by Lukes in his comment). This tendency misrecognises itself as providing a ground for science and rationality, whereas in fact what it does is conflate ontology and explanation into an elaborate theoretical architecture that serves as both the premise and the outcome of analysis. Another way of saying this is that misrecognising the hermeneutic nature of social science leads CR to pursue vicious as opposed to virtuous hermeneutic circles or spirals.

And this is further evidenced by the ambiguity in the critical realism around 'ontology', captured so well in the criticisms of CR written by Justin Cruickshank [2004] and Anthony King [2004].

Vollmer is right that *ISK* has an ontology, and it is one that features meaning prominently. Perhaps nowhere is this clearer than in the Aristotelean schema presented in chapter five, which was motivated by a desire to escape the dominance of certain causal images in social research, while retaining *ratio via machina* as one potential route to explanation. What is Aristotle's schema of four causes if not an ontology, and thus a primer for thinking about action? Vollmer thus points out that the book's argument for particularity is underwritten by 'the implication that meaning in an important sense is itself a truly general aspect of the social'. For Vollmer this is a performative contradiction, and thus indicates that, in criticising general theory, I have thrown myself unknowingly into the arms of postmodernism. This latter claim is not correct.

To see why, I insist that we hesitate before breezily moving from 'ontology' to 'theory' to 'explanation'. In my view, ontology is preliminary, and exists at some distance from concrete explanatory problems and their solution. When we do get into ontology, we are confronted with the radical malleability of human social forms by the construal of meaning. In reaction to this, we should intentionally design our ontology as *weak* in the sense of opening conceptually onto the vast variety of human experience and construals of the world (including the way in which these construals can harden into institutional routines). As such, ontology is necessary and debating it is useful, but it is a mistake to think that it *contains within itself* answers to well-defined explanatory questions, whether those questions concern the origins of revolutions, the origins of the French Revolution, the influence of imperial ventures on state formation, gaps in educational achievement between different demographically defined groups, or myriad other problems in sociology or other affiliated human sciences.

If we want, we can include the work of 'ontology' in the conceptual work we term theory. But it is a terrible mistake to either *reduce* theory to the work of ontology, or to *misrecognise* theoretical work as always already and only ontological debate. For when we look at the vast world of concept development in the human sciences, we see constructs designed to colligate evidence, render coherent models, and enable interpretation in response to well-defined why-questions concerning substantive topics. Much of this conceptual work relies, perhaps, on ontology in a weak sense of the term 'relies', but it is not ontology itself. It is, rather, concept formation at varying levels of generality.

Vollmer wants to defend general theory; it may surprise him that I do too. But in the human sciences, I see theoretical constructs—some of them, indeed, developed in extremely abstract intellectual settings and with an eye towards highly general formulations—being used to construct answers to explanatory questions that are not at the level of generality that Vollmer wants, and not ontological in their implications. Do we really want to say that a typology of different

kinds of postcolonial states, a categorisation and conceptual model of three different causal pathways to open rebellion, and the specification of the mechanism of the self-fulfilling prophecy need, somehow, to come together to be ordered in perfect coherence to constitute *the* ontology of the social? My point is that not only should we avoid this, but that in avoiding it, we do not magically become postmodernists with no standards of evidence, no way to tell a better explanation from a worse one, etc.

This, then, connects to my specific interest, in *ISK,* in historically bounded, contextually-sensitive, and meaning-dependent explanation, which I differentiate from theory. For, interpretive explanation, as a kind of maximal interpretation, results from the successful *fusion* of theoretical signs and evidentiary signs*.* Thus, my argument is that, in imitating various images of natural science—from Popper, from Lakatos, or even from Kuhn—we have, in social research, missed something fundamental about how theory works to help us build good interpretations. For the Lakatosians, for example, there is a hard core of general theoretical axioms, and an outer belt of auxiliary concepts, which are then revised and developed in the theory's encounters with various empirical cases (the French revolution, say, or the industrial development of a postcolonial African country). But to me this model of a research program, made famous for sociology via two brilliant papers written by Michael Burawoy [1989, 1990], has misconstrued the location of rationality in the human sciences. To me, it seems very difficult to answer questions such as: Is Marxist theory true? Is Marxism a progressive or degenerative research programme? And yet, it seems quite clear that we *are* good at answering questions such as: Is the Marxist explanation of the French Revolution the best one? Hence, for me, theory has to be *used* in a particular way to get at explanation, and our judgmental rationality in the human sciences will be at this, more concrete, level.

There is perhaps no better example of this than in Stephen Welch's own excellent work on political culture, which both shows the utility of working on preliminary ontology to get things clear and the relative distance of ontology from explanation. Welch [2013] articulates an ontology of culture as dualistic—that is, both discursive and practical—based on a careful reading of Wittgenstein and a simultaneous critique of both practice theory and its opposite (those for whom culture is propositional). Culture, for Welch, is bifurcated into public/social discourse, on the one hand, and embodied practical skills, on the other. But, given this ontology, I would hazard that we are still a long way from compelling explanations in response to well-defined research questions. Welch wants to say that skills are involved in the adaptive inertia of culture, and that discourse, as public signs, is subject to wild market-like fluctuations. Yet when we actually look at any of the explanations that he proposes embody his ontology of culture, what we find is that the cultural difference that makes the difference concerns the *content of the meanings* that emerge in either the 'skills' or the 'discourse' or both, not the ontological distinction between skills and discourse itself. The dualistic ontology

does not really do the work of comparison, explanation, causal difference making, etc. So, for example, Welch reconstructs a paper that skilfully offers a partial account for the differential fates of Chinese and Russian capitalism by pointing to inertial, implicit, adaptive skills around social network formation and the subtle meanings that differentiate two versions of these skills: that is, the difference between Russian *blat* and Chinese *guanxi* [2013: 176–177; see Hsu 2005]. The torque, in such an explanation, comes not from the ontology that underwrites it, but from the key difference in *meaning* that shapes and guides the development of social relations.

In so far as explanation, in the human sciences, requires this sort of ferreting out of meanings, as well as how they interact with mechanisms, motives, and material, then I think that the conflation of any two of ontology, theory, and explanation is a mistake. Perhaps it should be left for the intellectual historians of future generations to decide if this conflation was a result of the anxiety of non-influence that social researchers felt vis-à-vis natural science when they ventured into the messiness and muck of human signification. But whatever those historians decide about how and why a certain set of actors came to act in a certain way, surely in making their claims they will be making an interpretation, and in making an interpretation they will be using theory. This, it seems to me, is the very condition of possibility for a truly human science.

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